

The Discovery of the Diffraction of X-Rays by Crystals; A Critique of the Myths

PAUL FORMAN

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I. Introduction

On the 21st of April 1912, in ARNOLD SOMMERFELD'S Institute for Theoretical Physics of the University of Munich, WALTER FRIEDRICH and PAUL KNIPPING, acting upon a proposal by MAX LAUE, observed the diffraction of X-rays by a crystal.¹ This discovery, especially as interpreted and exploited by W. L. BRAGG and W. H. BRAGG, is the source point of an ever-expanding field of crystal structure

¹ The date of the discovery has, to my knowledge, not been published heretofore. (*Cf.*, however, in note 3, below, the date of the tenth anniversary issue of *Naturwiss.*) It is stated in the document reproduced in Fig. 1, and translated below. The document, given by LAUE in 1951 to the Handschriften-Sammlung of the Bibliothek of the Deutsches Museum, was very kindly drawn to my attention by Professor Dr. ARMIN HERMANN.

Since the 21st of April 1912 the undersigned [FRIEDRICH, KNIPPING, LAUE] have been engaged in interference experiments with X-rays passing through crystals. The guiding idea was that interferences arise in consequence of the space lattice structure of the crystals, because the lattice constants are ca. $10 \times$ greater than the conjectured wavelengths of the X-rays. Photographs No. 53 and 54 are deposited as proof.

Irradiated substance: copper sulfate

Exposed 30'. current in the moderately soft tube 2 milliampere.

Distance of the plates from the crystal: No. 53 = 30 mm; No. 54 = 60 mm.

Distance of diaphragm 3 (diameter 1.5 mm) 50 mm.

Distance of the point of origin of the primary rays from the crystal = 350 mm.

Diagram of the experimental setup [see Fig. 1].

The experimental setup depicted is evidently not one of the earliest, but represents, most probably, the first experiment with the "definitiven Apparat," described on p. 316 of W. FRIEDRICH, P. KNIPPING & M. LAUE, "Interferenz-Erscheinungen bei Röntgenstrahlen," *Bayerische Akad. d. Wiss. zu München, Sitzungsber. math.-phys. Kl.* (1912), pp. 303—322, 8 June, issued circa 23 August. Reprinted: *Annalen der Physik* 41, 971—988 (5 Aug. 1913); M. v. LAUE, *et al.*, *Die Interferenz der Röntgenstrahlen*, ed. F. RINNE & E. SCHIEBOLD (Ostwald's Klassiker ..., Nr. 204;

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MÜNCHEN, UNIVERSITÄT,
LUDWIGSTRASSE 17.

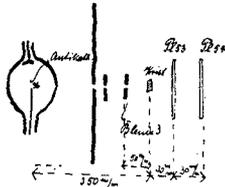
MÜNCHEN, DEN 4 Mai

1912.

die Untersuchungen beschäftigen sich seit 21 April 1912
mit Interferenzversuchen von X-Strahlen beim Durch-
gang durch Kristalle. Letzter Gedanke war, daß Inter-
ferenzen als Folge der Räumgitterstruktur der Kristalle
auftreten, weil die Gitterkonstanten ca 10 x größer sind,
als die mittelmäßige Wellenlänge der X-Strahlen.
Als Beweis wird Aufnahme Nr 53 u 54 niedergeleg.

Röntgenstrahlkörper: Kupfersulfat
Exponiert 30'. Strom in der mittelweichen Röhre 2 Milliamper.
Abstand der Platten vom Kristall: Nr 53 = 30 mm; Nr 54 = 60 mm.
Abstand der Blende (Ø 1,5 mm) 50 mm
Abstand des Ausgangspunktes der Primärstr. vom Kristall = 350 mm.

Schema der Versuchsanordnung.



Friedrich. P. Knipping. M. Laue.

Fig. 1. Sealed note deposited by A. SOMMERFELD with the Bavarian Academy of Sciences on 4 May 1912 in order to protect FRIEDRICH, KNIPPING, and LAUE's priority in the discovery of the diffraction of X-rays by crystals. (Photo Deutsches Museum München, Lichtbildnummer: 30497)

Leipzig, 1923), pp. 5—21; English translation in G. E. BACON, *X-Ray and Neutron Diffraction* ("Selected Readings in Physics"; Oxford: Pergamon, 1966), pp. 89—108. The photographs ("No. 53 and 54") included in the sealed note were no longer in LAUE's possession in 1951, nor do they remain with the Bavarian Academy. They are, however, most probably those reproduced as Figs. 3 and 4 of Table I in this first publication (see also, notes 111 and 114, below).

analysis by means of X-rays — a field lying between, and shared by, physics, chemistry, crystallography, geology, and now biology. The leaders of X-ray crystallography have striven to maintain a separate identity and resisted degradation of their field to the status of a mere technique common to these various sciences. Their separate and uniquely active International Union of Crystallography, sponsoring meetings and publications, their concern with defining “A Crystallographer” and what he ought to know,² testify to this effort. As part of the ritual serving to reinforce this separate identity, during the first fifty years of the existence of the field there accumulated a large number of brief retrospective accounts of the origins and immediate sequels of the discovery out of which it sprang.³ Finally, for the celebration of its fiftieth anniversary, through the initiative and editorial labors of P. P. EWALD, a magnificent Festschrift was prepared. *Fifty Years of X-Ray Diffraction* (Utrecht, 1962) is entirely devoted to personal recollections, histories of the several national schools of X-ray analysis, and a detailed account, by EWALD, of the circumstances of the discovery.⁴

² H. D. MEGAW, *et al.*, *Crystallographic Book List* (Cambridge Engl.: Internat. Union of Crystallography, 1965), p.v.

³ LAUE, “Über die Auffindung der Röntgenstrahlinterferenzen”, *Les Prix Nobel en 1914—1918* (Stockholm, 1920), reprinted in LAUE, *Aufsätze und Vorträge* (Braunschweig, 1962), pp. 5—18, Nobelvortrag, gehalten am 3. 6. 1920; W. FRIEDRICH, “Die Geschichte der Auffindung der Röntgenstrahlinterferenzen”, *Naturwiss.* **10**, 363—366 (21 April 1922, “Zehn Jahre Laue-Diagramm”); P. P. EWALD, “Zur Entdeckung der Röntgeninterferenzen vor zwanzig Jahren und zu Sir William Braggs siebzigstem Geburtstag”, *Naturwiss.* **20**, 527—530 (15 July 1932); W. H. BRAGG & W. L. BRAGG, “The discovery of X-ray diffraction”, *Current Science* **7**, suppl. (1937), pp. 9—13; LAUE, “Zu P. v. Groths 100. Geburtstag”, *Zeitschr. f. Kristallogr. (A)* **105**, 81 (1943), reprinted in LAUE’s *Aufsätze u. Vorträge* (1962), p. 186; LAUE, “Mein physikalischer Werdegang. Eine Selbstdarstellung”, written in 1944, first published in 1952, reprinted in *Aufsätze u. Vorträge* (1962), pp. vii—xxxvi, trans. in EWALD, ed., *Fifty Years of X-Ray Diffraction* (1962), pp. 278—307; LAUE, “Address before the First Congress of the International Union of Crystallography at Harvard University, Cambridge Mass., August 1948”, reprinted by the North American Philips Co. Inc., Research and Control Instruments Division; W. FRIEDRICH, “Erinnerungen an der Entstehung der Interferenzerscheinung bei Röntgenstrahlen”, *Naturwiss.* **36**, 354—356 (1949); KATHLEEN LONSDALE, “Historical Introduction”, *Crystals and X-Rays* (New York, 1949), pp. 1—22; LAUE, “Historical Introduction”, *International Tables for X-Ray Crystallography*, vol. 1 (Birmingham, 1952), pp. 1—5; LAUE, “Zur Geschichte der Röntgenstrahlinterferenzen”, *Naturwiss. Rundschau* **1**, 1—8 (1954), reprinted in *Aufsätze u. Vorträge* (1962), pp. 110—117; EWALD, “William Henry Bragg and the New Crystallography”, *Nature* **195**, 320—325 (1962).

⁴ P. P. EWALD, “The Beginnings”, *Fifty Years of X-Ray Diffraction* (Utrecht: N. V. A. OOSTHOEK’s Uitgeversmaatschappij for the Internat. Union of Crystallography, 1962), pp. 6—80. A brief account of the commemorative congress in Munich in July 1962 is given by A. NIGGLI, “Fünfzig Jahre Röntgeninterferenzen”, *Naturwiss.* **50**, 461—462 (1963).

Recollections of the discovery of X-ray diffraction are also contained in unpublished interviews with P. DEBYE, P. EPSTEIN, P. P. EWALD, and W. FRIEDRICH deposited in the “Archive for History of Quantum Physics”; details are given in T. S. KUHN, *et al.*, *Sources for History of Quantum Physics. An Inventory and Report* (Philadelphia: American Philosophical Society, 1967). Through the kindness of L. PEARCE WILLIAMS I have been able to consult the transcript of interviews with P. DEBYE by D. M. KERR, Jr., and WILLIAMS in 1965/66. These transcripts are on deposit in the Oral History Project, Olin Library, Cornell University.

The origin of this discovery being LAUE's inspiration — rather than either a long experimental search or an accidental observation⁵ — these retrospective accounts often attempt to explain why this happy idea came where and when it did. The question is a good one, but dangerous. It is almost certain that the physicist, having posed the question, will insist upon an answer. And in framing that answer he will be guided by i) logical neatness and true physics, and ii) the motives which led him to raise the question in the first place. The memories of the principals have not been able to withstand these impulses, and they themselves have created and elaborated an account of the conceptual situation in physics circa 1911 which is, in certain respects, utterly mythical.⁶ Since EWALD's account is likely to be regarded as definitive by historians as well as physicists, it seems worthwhile to offer a critical examination of the traditional answer to the question, "Why Munich, spring 1912?", and of the conceptual difficulties standing in the way of the observation of the diffraction of X-rays by crystals.

II. Why Munich ?

The question — "why Munich?" — was first raised publicly, and given its now traditional answer, in LAUE's Nobel lecture 'On the Discovery of the Interference of Röntgen Rays,'⁷ delivered in Stockholm in June 1920. The idea that crystals ought to diffract X-rays arose in Munich, LAUE maintained, because of two unique factors in the intellectual milieu: i) confidence in the hypothesis that the constituent atoms of a crystal are arranged in a space lattice, ii) active advocacy of the 'wave' theory of X-rays. In this section we argue that, on the contrary, the Munich physicists were in no way unique in their adherence to the space lattice hypothesis, and we deny that an attachment to the 'wave' theory of X-rays was a necessary condition for conceiving the experiment. In Section III we go further, arguing that the experiment actually appeared very unpromising from the point of view of the 'wave' theory of X-rays.

1. The Space Lattice Hypothesis. Elaboration of a Myth

As LAUE mentioned in his Nobel lecture, the notion that crystals consist of 'similar molecules similarly situated'⁸ goes right back to the seventeenth century. This picture, which HAÜY had elaborated at the end of the 18th century, was able to explain many empirical regularities in the forms of crystals.⁹ A lattice arrangement of point centers of force had been introduced by SEEBER in 1824

⁵ EWALD, *Naturw.* 20, 527—530 (1932).

⁶ Myths and anecdotes — a species of minor myth — have important, and perhaps even legitimate, functions in contemporary science, especially as devices for expressing the mores of the scientific community without exposing the scientist to the dangers of self-consciousness. But because they purport to be historical, myths and anecdotes are subversive of history.

⁷ LAUE, *Aufsätze u. Vorträge*, pp. 5—18.

⁸ AD. WURTZ, *La Théorie atomique* (5th ed.; Paris, 1889), p. 226. Yet only after 1860 (CHR. WIENER, L. SOHNCKE) was this notion used explicitly as a postulate for the deduction of the possible crystal forms.

⁹ JOHN G. BURKE, *Origins of the Science of Crystals* (Berkeley and Los Angeles, 1966); P. GROTH, *Entwicklungsgeschichte der mineralogischen Wissenschaften* (Berlin: Springer, 1926).

and had been assumed by CAUCHY circa 1830 in calculations which laid the foundations of the theory of elasticity.¹⁰ The theory of space lattices was developed by HESSEL (1830), FRANKENHEIM (1835), and BRAVAIS (1850), amplified by SOHNCKE in the 1870's and 1880's, and completed by SCHOENFLIES and FEDOROW circa 1890 with the compilation of a complete list of the possible space groups.¹¹

What did physicists know of these theories in the first years of the twentieth century, and how did they regard the idea underlying them, namely that in crystals the constituent molecules or atoms were arranged in a space lattice or lattices? LAUE answered in 1920 that: 'No further physical consequences whatsoever had come out of this idea, and so as a dubious hypothesis it eked out an existence rather unknown to the physicists.'¹² But Munich, LAUE maintained, was different. Because SOHNCKE had worked there — and many of his models of lattices were still lying around — and because these theories were strongly advocated by PAUL GROTH, the professor of crystallography, the Munich physicists were acquainted with and adhered to this 'dubious hypothesis' of a space lattice in crystals. And acceptance of this hypothesis being a necessary condition for the proposal that crystals might diffract X-rays, the Munich intellectual milieu was extraordinarily, if not uniquely, favorable to the conception of such an experiment.

In 1920 when LAUE put this thesis forward memories of the situation eight or ten years earlier were still sufficiently distinct that it could be recognized for what it was — a wholly fictional rationalization by a man who regarded himself as lacking originality.¹³ PLANCK felt that his favorite pupil did himself an injustice, and a year later, in welcoming him to membership in the Prussian Academy of Sciences, attributed LAUE'S bright idea to 'the urgent demand of your scientific conscience to seek to clear up the conflict which existed at that time between the conception of the regular atomistic structure of crystals and the widespread assumption of the absence of any diffraction and interference of X-rays.'¹⁴ A year later still, when celebrating the tenth anniversary of the discovery with an account of its history, FRIEDRICH made no mention of LAUE'S thesis regarding the 'dubious hypothesis' of space lattices, but simply stressed

¹⁰ L. B. SEEBER, "Versuch einer Erklärung des inneren Baues der festen Körper," *Ann. d. Phys.* 76, 229—248, 349—372 (1824); ISAAC TODHUNTER & KARL PEARSON, *A History of the Theory of Elasticity and of the Strength of Materials from Galilei to the Present Time*, 2 vols. in 3 (Cambridge University Press, 1886—1893); C. H. MÜLLER & A. TIMPE, "Die Grundgleichungen der mathematischen Elastizitätstheorie", *Encyklopädie der mathematischen Wissenschaften*, vol. 4, pt. 4, pp. 1—54 (1906); A. E. H. LOVE, *A Treatise on the Mathematical Theory of Elasticity* (4th ed.; Oxford University Press, 1927), "Historical Introduction." (LOVE'S discussion of the issues considered in this paper is identical with that in the 2nd edition, Cambridge, 1906, excepting that on p. 14 a reference to sub-atomic particles is substituted for a reference to the aether.)

¹¹ LEONHARD SOHNCKE, *Entwicklung einer Theorie der Krystallstruktur* (Leipzig, 1879), "Historische Einleitung"; J. J. BURCKHARDT, "Zur Geschichte der Entdeckung der 230 Raumgruppen", *Archive for History of Exact Sciences* 4, 235—246 (Nr. 3, 4 Oct. 1967).

¹² *Aufsätze u. Vorträge*, pp. 9/10.

¹³ LAUE, "Antrittsrede beim Eintritt in die Preuß. Akad. d. Wiss.", *Sitzungsber.* (1921), pp. 479—482, reprinted in *Aufsätze u. Vorträge*, pp. 21—24.

¹⁴ PLANCK, "Erwiderung des Sekretärs", *loc. cit.*

that because of GROTH, and because of RÖNTGEN's interest in crystal physics, 'the Munich physicists had a penetrating knowledge of this area of research.'¹⁵

By the twentieth anniversary, however, memories of the period had become quite plastic, and EWALD's retrospective account on that occasion reasserts LAUE's thesis of the dubious hypothesis unknown or discredited outside Munich. 'For the lattice theory was discredited by the difficulties which resulted from the CAUCHY relations; these difficulties were not counterbalanced by any interpretation which made serious use of the lattice structure and would have allowed, conversely, quantitative conclusions about the lattice.'¹⁶ By the 25th anniversary the BRAGGS had joined the chorus: the conception that crystals consist of atoms arranged in a space lattice 'has now become widely familiar, but at that time crystallography was so much a science apart, and played so little part in physics and chemistry, that the idea of a "crystal pattern" had never presented itself to the majority of scientists.'¹⁷

LAUE gave the sharpest formulation of this thesis in 1943 in a tribute to PAUL GROTH: 'through his instructional activities he kept alive in Munich the ... tradition of the space lattice hypothesis, which scarcely still existed elsewhere in Germany, and so created one of the preconditions without which the discovery of the interference of X-rays would have been purely a matter of luck, and its interpretation would have been entirely impossible.'¹⁸ In 1953 LAUE drew the logical inference, asserting that although many researchers had irradiated crystals with X-rays before FRIEDRICH and KNIPPING, they didn't observe interference 'because in ignorance of this hypothesis [*i.e.*, the space lattice hypothesis] they never had sought for any sort of radiation beside the incident.'¹⁹ Thus, presumably, the idea of X-ray diffraction would have immediately occurred to anyone who was acquainted with the space lattice hypothesis.

This myth — for that is what it is — attained its fullest elaboration, replete with quite fictitious details, in EWALD's retrospective accounts on the occasion of the 50th anniversary of the discovery of X-ray diffraction. Now, rather than simply being without physical consequences and unknown, the space lattice hypothesis is said to have been experimentally refuted. The relations between the elastic constants which CAUCHY had deduced, ca. 1830, for a lattice of identical point centers of force "were not confirmed by experiment, and the failure discredited the entire concept of internal regularity of crystals."²⁰ "Thus it came about that the concept of internal regularity and periodicity as a characteristic for crystalline matter, after having emerged in a very promising way, lay dormant for more than seventy years as a brilliant, but unfortunately not acceptable speculation which neither physicists nor crystallographers dared to use seriously."²¹ Only after X-ray diffraction had demonstrated the existence of a space

¹⁵ FRIEDRICH, *Naturw.* **10**, 365 (1922).

¹⁶ EWALD, *Naturw.* **20**, 528/9 (1932).

¹⁷ W. H. & W. L. BRAGG, *Current Science* (1937), suppl., p. 9.

¹⁸ LAUE, *ZS. f. Kristallographie (A)* **105**, 81 (1943); *Vorträge u. Aufsätze*, p. 186.

¹⁹ LAUE, *Naturw. Rundschau* **1**, 113.

²⁰ EWALD, *Nature* **195**, 320 (1962).

²¹ EWALD, *Fifty Years of X-Ray Diffraction* (1962), p. 24.

lattice, EWALD continued, was CAUCHY'S deduction re-examined by MAX BORN and the fallacy found.

In stressing this allegedly unique feature of the Munich environment as a precondition for his idea, LAUE implied — indeed almost stated — that he himself had no exposure to, and no knowledge of, the space lattice hypothesis before he came to Munich as a Privatdozent in 1909. This point has been especially stressed and elaborated by EWALD, who claims the honor of having informed LAUE in the winter of 1911/12 of the assumption made in Munich about the structure of crystals. And, of course, as soon as LAUE learned of this assumption, the idea of X-ray diffraction by these space lattices occurred to him.²² P. S. EPSTEIN, then one of SOMMERFELD'S students, has repeated this story, but is sufficiently critical to be puzzled that LAUE could have worked in this milieu for two and a half years without becoming aware of the space lattice hypothesis. There was a monthly colloquium, founded by SOHNCKE, which met in SOMMERFELD'S institute and was attended by people from various departments. LAUE, EPSTEIN recalled, "was a member of the SOHNCKE colloquium for years, but he somehow missed who SOHNCKE was. I got the book of SOHNCKE and learned the SOHNCKE theory of crystals. But LAUE didn't know it and didn't know that crystals were lattices."²³ If experience is any guide, we may with some confidence expect that this logical gap will be filled by further elaborations of the myth.

2. The Space Lattice No Hypothesis

In the period prior to the discovery of the diffraction of X-rays by crystals the existence of the space lattice was neither unknown to the physicist, nor indeed regarded by him as a hypothesis; it was an assumption which, despite the lack of any direct evidence, was made universally and implicitly, and in 1911 was regarded as far more secure than, say, the laws of mechanics. In supporting this position we approach the period immediately prior to the discovery through discussions, first, of the theory of elasticity (which, according to EWALD, refuted the space lattice hypothesis) and, second, of crystallography (which, according to LAUE, had forgotten the space lattice hypothesis).

In the period 1820—1830 the theory of the elasticity of solids was tackled from the molecular viewpoint first by NAVIER, then by POISSON and CAUCHY. POISSON and CAUCHY represented a crystal by an arbitrarily large number of identical molecules, placed at the vertices of a space lattice, and exerting central forces upon each other.²⁴ The stress exerted on an element of an elastic solid has six components: three normal stresses, F_x , F_y , F_z , and three shearing stresses F_{xy} , F_{xz} , F_{yz} . Likewise there are six strains. Thus in place of HOOKE'S law, $F \propto x$, one puts the most general linear relation between the six stresses and six strains $\mathbf{F} = C\mathbf{x}$, where \mathbf{F} and \mathbf{x} are vectors with six components and C is a 6×6 matrix of elastic constants, c_{ij} . The molecular space lattice theories of POISSON and CAUCHY did not allow more than fifteen of these thirty-six elastic constants to

²² EWALD, *Naturw.* 20, 529 (1932); *Fifty Years* (1962), p. 41.

²³ P. S. EPSTEIN, interview by Sources for History of Quantum Physics, 25 May 1962, A. M., p. 9.

²⁴ See Footnote 10.

be independent. In particular, for isotropic solids there was only one independent constant, leading, for example, to the prediction that for a rod subject to tensile stress the ratio of the relative decrease in its diameter to the relative increase in its length ("Poisson's ratio") is equal to $\frac{1}{2}$. On the other hand, when GREEN, STOKES, and WM. THOMSON developed the theory of elasticity on a continuum basis, without reference to the underlying structure of matter and the hidden connections of the molecular approach, they found that, in general, twenty-one of the elastic constants were independent.²⁵ In particular, for isotropic bodies there were two independent constants, so that POISSON'S ratio could have any value.

During the latter 19th century there was a running argument²⁶ between the supporters of the "rariconstant" theory (15, 1) and the proponents of the "multiconstant" theory (21, 2) over the empirical validity of the six "Cauchy relations"²⁷ reducing the number of independent constants from twenty-one to fifteen. The counter examples brought forward by the British multiconstantists were resisted by the continental rariconstantists on the grounds that the samples or substances were not truly isotropic or not truly elastic, *etc.* The issue was decided definitely in favor of the multiconstant theory after W. VOIGT began to publish his work on the elastic constants of crystals in 1887.

Thus far we have been able to follow EWALD. But we cannot accept his conclusion that the experimental evidence accumulating against the CAUCHY relations from the middle of the 19th century "discredited the model from which the relations sprang, namely that in the natural state of a crystal its molecules are arrayed in a three-dimensional lattice."²⁸ On the contrary, we doubt it would be possible to find a single physicist, whether rariconstantist or multiconstantist, who believed that the truth — or indeed even the utility — of the space lattice hypothesis was at issue in this dispute. From CAUCHY to VOIGT, it was taken for granted that the molecules of a crystal were arranged in a space lattice, and all discussion dealt with possible modifications of the *further* assumptions should it turn out that the rariconstant theory did not fit the experiments.

In 1851, after WERTHEIM'S measurements raised serious doubts that POISSON'S ratio was always equal to $\frac{1}{2}$, CAUCHY himself pointed out that one would no longer *expect* the relations in question to hold if one supposed not that the molecules are point centers of force, but 'on the contrary, each molecule is composed of several atoms.'²⁹ Two years earlier, CLAUDIUS, after a highly critical review of the POISSON-CAUCHY theory, came again to the result that the molecular theory yields only one elastic constant for isotropic bodies. As experiment does not confirm these equations, what must be sacrificed? — merely the assumption that the external forces do nothing more than displace the molecules from their

²⁵ MÜLLER-TIMPE, *Encykl. d. math. Wiss.* 4, 4, pp. 38—41; LOVE, *Math. Theory of Elasticity* (1927), pp. 13—14; EWALD, *Fifty Years* (1962), pp. 22—23. Actually CAUCHY had already found this result.

²⁶ TODHUNTER & PEARSON, *Hist. of the Theory of Elasticity*, Vol. 1 (1886), par. 921—934.

²⁷ The name is evidently due to LOVE: See *Math. Theory of Elasticity* (1927), p. 14.

²⁸ EWALD, *Fifty Years* (1926), p. 24.

²⁹ Quoted by TODHUNTER & PEARSON, *Hist. Th. Elast.*, Vol. 2, par. 787.

equilibrium positions.³⁰ Again, B. DE SAINT-VENANT, 'who remained always the most consistent representative of the rariconstant theory,'³¹ was perfectly clear that it was possible, and that it might prove necessary, to modify the assumptions about the molecular forces in order to obtain more independent elastic constants for crystals.

When we turn to that arch multiconstantist, Lord KELVIN, we are scarcely surprised to find that he took it for granted that in crystals the molecules were arranged in a space lattice.³² In 1890 KELVIN showed that with a very simple modification of CAUCHY'S assumptions the molecular space lattice yielded the full complement of 21 constants — it was merely necessary to suppose the crystal consisted of two interpenetrating space lattices whose vertices were occupied by two different kinds of point centers of force.³³ Finally there is VOIGT himself, who, although a multiconstantist, stood at the opposite methodologic pole from KELVIN. We will return to VOIGT *qua* phenomenologist; here we merely note that the man who provided conclusive evidence of the inadequacy of the rariconstant theory assumed as a matter of course that crystals were molecular space lattices.³⁴ VOIGT himself showed in 1887/9 that the molecular space lattice would yield twenty-one independent constants if one merely assumed the molecules to be dipoles, and moreover that the necessity of two constants for isotropic bodies followed from the assumption that they were composed of microscopic crystal-lites.³⁵ Thus EWALD'S contention that developments in the theory of elasticity led to the rejection of the space lattice "hypothesis" is evidently a *post factum* fabrication.³⁶ Rather, those who worked in this field in the 19th century — and they were, by and large, physicists — accepted this "hypothesis," with which they were thoroughly familiar, implicitly.

Turning now to the science of crystallography, we must first concede that science and physics were far less fully integrated before than after the discovery of X-ray diffraction. As late as 1904 even PAUL GROTH, very progressive advocate of the structure theories and of the application of physical and chemical methods in crystallography, could speak of "the molecular hypothesis." With this phrase GROTH referred not to the molecular space lattice — which for him was no hypo-

³⁰ R. CLAUSIUS, "Über die Veränderungen, welche in den bisher gebräuchlichen Formeln für das Gleichgewicht und die Bewegung elastischer fester Körper durch neuere Beobachtungen nothwendig geworden sind", *Annalen d. Physik* 76, 46—67 (1849); TODHUNTER & PEARSON, Vol. 1, par. 1399—1401.

³¹ MÜLLER-TIMPE, *Encykl. d. math. Wiss.* 4, 4, pp. 38/9.

³² E.g., "The Size of Atoms" (Feb. 1883) in THOMSON, *Popular Essays and Addresses* vol. 1 (London, 1889), p. 185; or "On the Molecular Tactics of a Crystal" (May 1893) in KELVIN, *Baltimore Lectures on Molecular Dynamics ...* (London, 1904), pp. 602ff.

³³ *Ibid.*, pp. 643—661; MÜLLER-TIMPE, *Encykl. d. math. Wiss.* 4, 4, p. 41.

³⁴ W. VOIGT, "Über die Beziehung zwischen den beiden Elasticitätskonstanten isotroper Körper", *Ann. d. Phys.* 38, 573 (1889).

³⁵ *Ibid.*; MÜLLER-TIMPE, p. 40; W. VOIGT, "Theoretische Studien über die Elasticitätsverhältnisse der Krystalle," *Kgl. Gesellsch. der Wiss. zu Göttingen, Abhandl.* 34 (1887), 100 pp.

³⁶ Thus O. MÜGGE, "Zur Prüfung der Strukturtheorien an der Erfahrung", *Encykl. d. math. Wiss.* 5, 1, pp. 478—492 (1905), reviewing the experimental evidence for and against the *Raumgitter* theory, SOHNCKE'S theory, etc., never even mentions the "Cauchy relations".

thesis — but to the physical doctrine that the molecules he placed at the vertices of the space lattice were actually in continual thermal motion.³⁷ Was it then perhaps the case that with the exception of GROTH the crystallographers of the turn of the century were so little concerned with physical hypotheses about the microscopic structure of crystals that, as LAUE put it, the space lattice theory “was hardly mentioned anymore”?³⁸ This view has indeed been put forward in historical works.³⁹ The facts, however, are otherwise. Although in the mid-19th century textbooks of mineralogy and crystallography often contained no discussion of the molecular structure of matter and the microscopic structure of crystals, of the textbooks published after 1890 only 8 of the 23 examined failed to state that the underlying structure of crystals was a molecular space lattice.⁴⁰ Thus, although the 4th edition of the *Manual of Mineralogy and Petrography* (N.Y., 1889) by 76 year old JAMES DWIGHT DANA remains silent on this point, *A Textbook of Mineralogy* (2nd edition; New York, 1898) published by his son, EDWARD S. DANA, opens with a “Definition of a Mineral” as including “a certain characteristic molecular structure which is exhibited in its crystalline form and other physical properties” (p. 1). Under the heading “Molecular Networks” the

³⁷ GROTH, “Crystal Structure and its Relation to Chemical Constitution”, *B. A. Repts.* (Cambridge, 1904), pp. 505—509.

³⁸ LAUE, “Mein physikalischer Werdegang” (1944), as trans. in EWALD, *Fifty Years* (1962), p. 293.

³⁹ H. D. DEAS, “Crystallography and crystallographers in England in the early nineteenth century: A preliminary survey”, *Centaurus* 6, 129—148 (1959).

⁴⁰ Textbooks of mineralogy and crystallography published after 1890 (a very helpful list of 19th century textbooks is given by MAX BAUER, *Lehrbuch der Mineralogie* (Stuttgart, 1904), pp. 3—12):

1) Those in which the space lattice theory is presented: W. S. BAYLEY, *Elementary Crystallography* (N.Y., 1910); J. BECKENKAMP, *Statische und kinetische Kristalltheorien. Erster Teil* (Berlin, 1913); E. S. DANA, *A Text-Book of Mineralogy* (2nd ed.; N.Y., 1898); C. DOELTER, *Physikalisch-chemische Mineralogie* (Leipzig, 1905); A. FOCK, *An Introduction to Chemical Crystallography*, trans. W. J. POPE (Oxford, 1895); P. GROTH, *Physikalische Kristallographie* (4th ed.; Leipzig, 1905); F. KLOCKMANN, *Lehrbuch der Mineralogie* (5th and 6th ed.; Stuttgart, 1912); O. LEHMANN, *Molekularphysik mit besonderer Berücksichtigung mikroskopischer Untersuchungen* (Leipzig, 1888—9); TH. LIEBISCH, “Das kristallographische Grundgesetz”, *Encykl. d. math. Wiss.* 5, 1, pp. 395—436 (1905); A. H. PHILLIPS, *Mineralogy* (N.Y., 1912); F. RINNE, “Allgemeine Kristallographie und Mineralogie”, *Kultur der Gegenwart*, Teil 3, Abt. 3, Bd. 2, *Chemie* (Leipzig and Berlin, 1913); E. SOMMERFELDT, *Geometrische Kristallographie* (Leipzig, 1906); E. SOMMERFELDT, *Die Krystallgruppen, nebst ihren Beziehungen zu den Raumgittern* (Dresden, 1911); C. SORET, *Éléments de Cristallographie Physique* (Geneva, 1893); G. TSCHERMAK, *Lehrbuch der Mineralogie* (6th ed.; Vienna, 1905); W. VOIGT, *Lehrbuch der Kristallphysik* (Leipzig, 1910); G. H. WILLIAMS, *Elements of Crystallography* (N.Y., 1890).

2) Texts in which the space lattice theory is not presented: M. BAUER, *Lehrbuch der Mineralogie* (2nd ed.; Stuttgart, 1904); W. BRUHNS, *Elemente der Kristallographie* (Vienna, 1902); W. J. LEWIS, *A Treatise on Crystallography* (Cambridge, 1899); TH. LIEBISCH, *Physikalische Kristallographie* (Leipzig, 1891); TH. LIEBISCH, *Grundriss der physikalischen Kristallographie* (Leipzig, 1896); W. VOIGT, *Die fundamentalen physikalischen Eigenschaften der Krystalle* (Leipzig, 1898).

Two of the texts in 1) above, namely TSCHERMAK, *Lehrbuch* . . . , and SOMMERFELDT's introductory *Geometrische Kristallographie* — but not his advanced *Krystallgruppen* — present the space lattice theory without advocating it, emphasizing that their science does not depend upon an atomistic viewpoint and structure theories.

space lattice theory is described and asserted to be what is "believed" (pp. 18 to 21).⁴¹

In this connection we may note one further point often stressed in retrospective literature. As Dame KATHLEEN LONSDALE put it, the directional properties of crystals "were believed to indicate that *something* in crystals must be regularly arranged in space. It was not known whether the grouping or unit ... was an atom or a molecule, a part of a molecule or several molecules."⁴² Although it was most reasonable to emphasize this uncertainty after X-ray analysis showed that the unit cell of the space lattice was simply a box which could indeed be filled by an atom, a molecule, or several molecules, in fact neither crystallographers nor physicists allowed themselves to be upset by this uncertainty; indeed they generally overlooked it. As A. E. H. TUTTON explained in *Crystalline Structure and Chemical Composition* (London, 1910), p. 9, "it is the space lattice which determines the crystal system and represents the type of edifice built up by the chemical molecules; for the points taken analogously in the molecules, one to represent each, are those which build up the space lattice." Thus the picture generally employed was of a single space lattice whose vertices were occupied by the chemical molecules. It was, however, only mildly surprising when W. L. BRAGG's analyses of the alkali halides showed that, in the words of the elder BRILLOUIN, 'the diffracting atoms are not concentrated in a molecule of very small dimensions, separated from the neighboring molecule by a great distance, following the favorite hypothesis of the mathematical physicists.'⁴³ The structure which X-ray diffraction revealed for NaCl — two interpenetrating face centered cubic lattices of sodium and chlorine atoms — had, in fact, already been *assumed* by MADELUNG and by BORN and KÁRMÁN in their calculations of vibrational frequencies and specific heats.⁴⁴

But now, if the molecular space lattice was, among crystallographers at the turn of the century, a common doctrine commonly expressed, is it perhaps nonetheless the case that, as the BRAGGS said, "crystallography was so much a science apart" that the physicists never came in contact with it? It is certain, however, that in Germany at least the majority of physics students also studied some crystallography. This was in large part due to the regulations for the examination to qualify as a Gymnasium teacher in Prussia. Candidates whose principal field was mathematics or physics had also to choose a secondary field

⁴¹ The failure to discuss the internal structure of crystals in a propedeutic work is not, of course, to be regarded as demonstrative of a lack of interest in these questions on the author's part, and even less as evidence of lack of belief in atoms, space lattices, *etc.* To take but one example, JAMES DWIGHT DANA, despite the silence of his text, was much interested in deducing from crystallographic data the forms and properties of "the ultimate particles of bodies." "On the formation of compound or twin crystals," *American Journal of Science* 30, 275—300 (1836); "On certain laws of cohesive attraction," *ibid.* 4, 364—385 (1847).

⁴² K. LONSDALE, *Crystals and X-Rays* (N.Y., 1949), pp. 6/7.

⁴³ M. BRILLOUIN at the second Solvay Congress, *La structure de la matière. Rapports et discussions du conseil de physique tenu à Bruxelles du 27 au 31 octobre 1913* (Paris, 1921), p. 228; likewise W. NERNST, p. 139.

⁴⁴ E. MADELUNG "Molekulare Eigenschwingungen", *Physikalische Zeits.* 11, 898—905 (1910); M. BORN & TH. V. KÁRMÁN, "Über Schwingungen in Raumgittern", *Phys. ZS.* 13, 297—309 (1912).

from among: 1) chemistry and mineralogy; 2) botany and zoology; 3) geography; 4) philosophical preliminaries.⁴⁵ (The regulations in the other German States were probably almost identical as they tended to follow PRUSSIA'S lead in such matters.) There seems little doubt that almost all physics students would select chemistry and mineralogy. Because Gymnasium teaching was at this time the only well defined and reasonably secure career path for one trained in physics, the great majority of students prepared for and took this examination, even if they harbored hopes of a university career. LAUE had himself begun to follow lectures on mineralogy during his first semesters at the university (1899). After receiving his doctorate with PLANCK (1903) he continued his studies at Göttingen, where he took the *Lehramtsexamen*, choosing chemistry and mineralogy as secondary field.⁴⁶ Outside Germany the situation is less clear and certainly less uniform — at Leiden crystallography was required, at Amsterdam it evidently was not⁴⁷ — but in general it appears that an exposure to an elementary text on crystallography, and to the molecular space lattice structure of crystals espoused therein, was an experience shared by the majority of physicists trained at the turn of the century.⁴⁸

What then was the conceptual situation in physics circa 1911? Contrary to the LAUE-EWALD thesis, but as the foregoing discussion would lead us to anticipate, the existence of the space lattice was taken by the physicists as a matter of course; almost no one even thought to label it as an assumption. And yet, curiously, EWALD'S myth of the discredited space lattice hypothesis appears to have its origins in the writings of W. VOIGT from this period. VOIGT, who never had any doubts about the space lattice theory, also had strong phenomenological predispositions. His *Lehrbuch der Kristallphysik* (1910) is a classical example of the phenomenological approach, which limits the physicist to the construction of mathematical relations between macroscopic parameters and avoids 'a special conception about the mechanism of the process.'⁴⁹ Here VOIGT, sharpening an

⁴⁵ Direktion des math.-phys. Seminars, *Ratschläge und Erläuterungen für die Studierenden der Mathematik und Physik an der Universität Göttingen* (Neue Auflage; Leipzig, 1913), p. 28.

⁴⁶ LAUE, "Mein physikalischer Werdegang", *Aufsätze u. Vorträge*, p. xix; *Fifty Years*, p. 289.

⁴⁷ G. UHLENBECK in interview with P. P. EWALD by Sources for History of Quantum Physics, 29 March 1962, p. 11. J. M. BIJVOET in *Fifty Years* (1962), p. 529.

⁴⁸ A further question well worth investigating is the extent to which the textbooks of experimental physics of this period (1890—1910) discussed the microscopic constitution of solids, and the space lattice structure of crystals in particular. A preliminary survey indicates wide variations; nonetheless, the fact that OTTO LEHMANN, ed., *Dr. Joh. Müllers Grundriss der Physik* (14th ed.; Braunschweig, 1896), calls for demonstration of "regelmässige Punktsysteme" in connection with the paragraph (40) on "Krystallisierte und amorphe Körper" suggests that collections of physical apparatus ordinarily contained models of point systems.

⁴⁹ VOIGT, *Lehrb. d. Kristallphys.* (Leipzig, 1910), p. 110. On the other hand, VOIGT, "Phänomenologische und atomistische Betrachtungsweise", *Kultur der Gegenwart*, Teil 3, Abteil. 3, Bd. 1, *Physik* (Leipzig and Berlin, 1915), pp. 714—731, implicitly assumed that the atomistic description, if attainable, was both preferable and truer. It is also interesting to note the way in which VOIGT refers in this article (p. 721) to the discovery of the diffraction of X-rays by crystals: LAUE had the simple but nonetheless genial idea "zu versuchen, ob bei der Durchstrahlung von Kristallen mit Röntgenstrahlen die nach unserer Vorstellung über Kristallstruktur zu erwartenden (sternförmigen) Interferenzerscheinungen zustandekämen." [Emphasis added.]

aperçu he had presented at the *Congrès international de physique* at Paris in 1900,⁵⁰ pointed to the early 19th century calculations of the elastic properties of solids on the basis of the molecular space lattice hypothesis. 'The result was shown to stand in contradiction to experience, and this contradiction was a principal reason for the discrediting of the molecular theory'⁵¹ — not merely, as EWALD has it, the internal regularity of crystals. But the resuscitation of the molecular theory had, according to VOIGT, already occurred a generation earlier, for when he found the proper assumptions about the molecules and crystallites, 'that previously enigmatic contradiction disappeared entirely of itself.' VOIGT justifies devoting a mere eleven pages to the structure theories of BRAVAIS, SOHNCKE, and SCHOENFLIES by pointing out that these theories have predicted almost nothing, and have only served to explain the most elementary properties of crystals — e.g., the law of rational indices and the formation of cleavage planes. 'Postulates for the deduction of the laws of physical phenomena on the basis of a special structural hypothesis are almost entirely lacking, and what has been available until now has little more utility than the symmetry relations derived from the external form of the crystals.'⁵² But VOIGT's use of the argumentation of the late 19th century phenomenologist is, again, purely rhetorical. VOIGT did not harbor the least doubt that the molecular space lattice represented the under-

⁵⁰ VOIGT, "L'état actuel de nos connaissances sur l'élasticité des cristaux" *Congrès International de Physique, Paris, 1900, Rapports*, Vol. 1, pp. 277—347; on pp. 287—289.

⁵¹ *Lehrb. d. Kristallphys.* (1910), pp. 8/9. LOVE, *Theory of Elasticity* (1906 and 1927), p. 13, stated that "the rari-constant equations rest upon a particular hypothesis concerning the constitution of matter, while the adoption of multiconstancy has been held to imply denial of this hypothesis." But "this hypothesis" is neither the space lattice, nor the molecular hypothesis, but "the hypothesis of material points and central forces," i.e., extensionless, inalterable, monopole atoms. This is indeed just the position which VOIGT himself held in 1887: "Denn nicht die molekulare Vorstellung selbst ist durch die erwähnten Beobachtungsergebnisse [invalidity of the CAUCHY relations] widerlegt, sondern nur eine willkürlich specielle Annahme über die Wirkungsweise der Moleküle, die schon an sich unwahrscheinlich ist." *Kgl. Ges. d. Wiss. zu Göttingen, Abh.* 34, 4.

⁵² *Op. cit.*, pp. 110/111. LAUE, *Geschichte der Physik* (2nd ed.; Bonn, 1947), p. 119 (4th ed., 1958; reprinted, Frankfurt and Berlin, 1966), p. 133, asserted that 'At first these investigations [of BRAVAIS, SOHNCKE, etc.] exerted no influence upon physics because no physical phenomena compelled the assumption of the space lattice hypothesis. Among the few physicists who concerned themselves at all with the study of crystals many [*manche*] adopted the opposing view that in crystals, as in other forms of matter, the centers of gravity of the molecules are distributed randomly and that the anisotropy is produced solely by the parallel alignment of preferred directions [*die Parallelstellung von Vorzugsrichtungen*].' The circumstance to which *this* myth refers is evidently the following. The phenomenon of "liquid crystals", widely studied after 1890, was often attributed to such a parallel alignment of large organic molecules. OTTO LEHMANN, the most energetic proponent of the view that "liquid crystals" really were crystals (crystallographers would have nothing to do with them), went so far as to maintain that what was essential to crystals was the anisotropy of the molecules, which were oriented by a special "Richtkraft", etc. Even LEHMANN, however, readily conceded that in ordinary crystals the molecules were arrayed in a space lattice. LEHMANN, *Flüssige Kristalle sowie Plastizität von Kristallen im Allgemeinen...* (Leipzig, 1904), p. 9: The space lattice theory "ist heute die herrschende geworden. Sie ist zu Grunde gelegt bei Behandlung der kristallographischen Erscheinungen in allen Lehrbüchern der Physik, Kristallographie und physikalischen Chemie und ver-

lying reality. Although relatively barren up until then, it would, VOIGT anticipated, become most fruitful in the future. While spectroscopy was 'a "pathology" of the molecules', crystal physics, which dealt with 'the normal, healthy molecules' in the perfectly regular environment provided by the space lattice, would make it 'possible to attain entrance into the ultimate problems of physics, the questions regarding the processes in the molecules.'⁵³

VOIGT was 60 in 1910. The younger generation of physicists felt nothing of his phenomenological ambivalencies. Their confidence in the space lattice can be seen most clearly in the discussions of the eigenfrequencies and specific heats of solids, discussions which began early in the century and became particularly lively between 1910 and 1912. The papers of EINSTEIN, MADELUNG, LINDEMANN, HABER, DEBYE, BORN and KÁRMÁN⁵⁴ show very clearly that the atomic or molecular space lattice was not a hypothesis, not a theory, but an implicit assumption — implicit because no alternative was conceivable. As BORN, looking back on his work with VON KÁRMÁN, quite accurately recalls: 'We regarded the existence of atomic lattices as self evident.'⁵⁵ And when, on the assumption of a space lattice, EINSTEIN'S rough calculation of the thermal conductivity of crystals yielded the wrong order of magnitude and temperature dependence, there was no thought of calling the space lattice hypothesis in question. There was only one possibility: 'We must conclude from this that mechanics is not capable of explaining the thermal conductivity of non-conductors.'⁵⁶ It is thus amply evident that the Munich physicists were in no way unique in their belief in space lattices, and thus that belief in the space lattice cannot have been a sufficient condition for conceiving of the diffraction of X-rays by crystals.

mag scheinbar von allen Tatsachen in einfachster Weise Rechenschaft zu geben." Here a footnote cites G. TAMMANN, *Kristallisieren und Schmelzen* (Braunschweig, 1903), who construes LEHMANN'S views as an attack on the space lattice theory. LEHMANN replies: "Der Verfasser scheint der Meinung zu sein, daß ich die Raumgittertheorie überhaupt beseitigen wolle. Daran habe ich natürlich nie gedacht."

⁵³ VOIGT, *Lehrb. d. Kristallphys.*, p. 5. For an excellent discussion of this atomism — phenomenism 'schizophrenia' of the late nineteenth century physicists, see the first chapter of J. L. HEILBRON, *A History of the Problem of Atomic Structure from the Discovery of the Electron to the Beginnings of Quantum Mechanics* (Ph. D. dissertation, University of California, Berkeley, 1964; Ann Arbor: University Microfilms, 1965).

⁵⁴ A. EINSTEIN, "Eine Beziehung zwischen dem elastischen Verhalten und der spezifischen Wärme bei festen Körpern mit einatomigem Molekül", *Ann. d. Phys.* **34**, 170—174 (30 Dec. 1910); F. A. LINDEMANN, "Über die Berechnung molekularer Eigenfrequenzen", *Physikal. ZS.* **11**, 609—612 (15 July 1910); F. HABER, "Über den festen Körper . . .", *Verhandl. der Deutsch. Phys. Ges.* **13**, 1117—1136 (30 Dec. 1911), p. 1128; P. DEBYE, "Zur Theorie der spezifischen Wärmen", *Ann. d. Phys.* **39**, 789—839 (1912), p. 791; papers cited in note 44.

⁵⁵ BORN, "Rückblick auf meine Arbeiten über Dynamik der Kristallgitter" (introductory lecture at the International Conference on Lattice Dynamics, Copenhagen, 5 Aug. 1963), *Physik im Wandel meiner Zeit* (4th ed.; Braunschweig, 1966), p. 280. Cf. BORN, "Erinnerungen an Max von Laues Entdeckung der Beugung von Röntgenstrahlen durch Kristalle", *ZS. f. Kristallographie* **112**, 1—3 (1959). BORN there recalls how the discovery "auf mich und vermutlich auf andere theoretische Physiker gewirkt hat"; namely, he and v. KÁRMÁN 'waren von der Gittertheorie der Kristalle so durchdrungen, daß unsere gemeinsame Reaktion auf die Kunde ihrer Bestätigung sich etwa mit den Worten "Na, also" beschreiben lässt.'

⁵⁶ A. EINSTEIN, "Elementare Betrachtungen über die thermische Molekularbewegung in festen Körpern", *Ann. d. Phys.* **35**, 679—694 (25 July 1911), p. 692.

3. The 'Wave' Theory No Necessary Condition

There remains, then, the second half of the LAUE-EWALD answer to the question "why Munich?", namely that Munich — in particular its Institute of Theoretical Physics under ARNOLD SOMMERFELD — was an active center of research on optics of all wavelengths, where the view that X-rays are simply classical electromagnetic radiation was strongly advocated.⁵⁷ (In what sense this was a 'wave' theory of X-rays we will consider in Section III.) It was SOMMERFELD who took on the task of defending this view against JOHANNES STARK's light quanta ("Lichtzellen").⁵⁸ SOMMERFELD was unwilling to concede that *any* of the properties of X-rays were inexplicable on the basis of the MAXWELL-LORENTZ theory.⁵⁹ Early in 1911 he showed that if an electron moving at nearly the speed of light is brought to rest in a distance of atomic dimensions, then on this theory nearly all the *Bremsstrahlung* is emitted into a narrow region between two concentric cones opening circa 8° around the direction of motion of the electron. Thus, SOMMERFELD maintained, the radiation has 'absolutely the character of a projectile and in its energy localization is no longer appreciably different from a corpuscular radiation or from the hypothetical light quantum.'⁶⁰ This being the case, the demonstration of the diffraction of X-rays would, SOMMERFELD declared, 'constitute a sort of capstone to the theory and definitively exclude every corpuscular theory of X-rays.'⁶¹ And, indeed, early in 1912 SOMMERFELD thought he had found strong evidence of diffraction in the photometric profiles of the diffuse broadening of the image of a wedge-shaped slit.⁶² Thus unquestionably the 'wave' theory of X-rays was a very prominent component of the Munich intellectual environment, and early in 1912 the question of how one might obtain an unambiguous demonstration of the diffraction of X-rays was an especially natural one to ponder in Munich.⁶³

⁵⁷ LAUE, Nobel lecture, *Aufsätze u. Vorträge* (1962), pp. 6—9; EWALD, *Fifty Years* (1962), pp. 14—16, 32—34.

⁵⁸ A. HERMANN, "Albert Einstein und Johannes Stark", *Sudhoffs Archiv* 50, 267—285 (Sept. 1966); "Die frühe Diskussion zwischen Stark und Sommerfeld über die Quantenhypothese (1)", *Centaurus* 12, 38—59 (1968). J. STARK, "Über Röntgenstrahlen und die atomistische Konstitution der Strahlung", *Phys. ZS.* 10, 579—586 (1 Sept. 1909). A. SOMMERFELD, "Über die Struktur der γ -Strahlen", *Bayerische Akad. d. Wiss. zu München, Sitzungsber. math.-phys. Kl.* (1911), pp. 1—60, read 7 Jan. 1911.

⁵⁹ This is an overstatement. By 1911 SOMMERFELD's view was that the MAXWELL-LORENTZ Theory gave a true picture of the electromagnetic radiation field and also of the production and absorption of radiation by charged particles, both inside and outside atoms. The 'duration' of these processes of emission and absorption, or the total energy transferred in one such process, was, however, determined by PLANCK's constant, h .

⁶⁰ SOMMERFELD, *Bayerische Akad., Sitzungsber.* (1911), p. 4.

⁶¹ SOMMERFELD, "Über die Beugung der Röntgenstrahlen", *Ann. d. Phys.* 38, 473—506 (18 June 1912), received 1 March 1912; on p. 473.

⁶² *Ibid.*

⁶³ One should add, however, that although SOMMERFELD and RÖNTGEN were working on X-rays in this period, they did not give X-ray problems to their students. In the five years 1909—1913 only WALTER FRIEDRICH completed a doctoral dissertation on X-rays (*Jahresverzeichnis der an den deutschen Hochschulen erschienenen Schriften*, 25—29). As measured by the number of workers and papers, Cambridge was a far more active center of X-ray research than Munich.

It seems, of course, self-evident that adherence to the 'wave' theory was a necessary condition for *conceiving* of the interference of X-rays scattered by the atoms of a crystal. But was adherence to this view of X-rays a necessary condition for the *discovery*, that is, for performing the experiment and observing the effects which we now *interpret* as due to the interference of X-ray waves?

In April 1912, simultaneous with the first experiments at Munich on diffraction of X-rays, JOHANNES STARK and G. WENDT reported on a series of experiments at Aachen in which a crystal plate about 1 mm thick was exposed for a few hours to a narrow beam of canal rays (3×10^3 — 15×10^3 electron Volt).⁶⁴ The original idea behind these experiments was that the ion (canal ray) would collide head-on with a molecule in the face of the crystal and that this "Stoss" would be propagated down a row of the molecular space lattice, producing a roughening or deformation of the back face 1 mm distant from the point of impact.⁶⁵ (The authors took the space lattice arrangement of the molecules in a crystal as a matter of course; glass, as amorphous and thus lacking any regular arrangement, was used as a control).⁶⁶ In the course of their work, however, they became convinced that this process was out of the question,⁶⁷ and came instead to the conclusion that 'the mechanical action which hydrogen canal rays produce at a depth in solid bodies can be explained naturally by the penetration of the rays between the intermolecular valence fields beneath the surface layer.'⁶⁸ That is, the hydrogen ions pass between the rows of atoms to some considerable depth in the crystal.⁶⁹ 'Whether in fact hydrogen canal rays are, as it appears, able to penetrate into a crystal lattice more easily and more deeply parallel to cleavage planes than perpendicular to them must be the subject of special detailed investigations. This task represents a part of a more general problem, namely the problem of the investigation of crystal structure by means of canal-, α -, and cathode-rays.'⁷⁰

A few days after the appearance of LAUE, FRIEDRICH, and KNIPPING's paper STARK submitted for publication a 'Remark on the Scattering and Absorption of β -Rays and X-Rays in Crystals.'⁷¹ 'In continuation' of the experiments described above, said STARK, 'I wanted to investigate the phenomena which arise when a thin bundle of β - and X-rays passes through crystal plates. Since I am presently hindered from doing so, I communicate the considerations according to which

⁶⁴ J. STARK & G. WENDT, "Über das Eindringen von Kanalstrahlen in feste Körper", *Ann. d. Phys.* **38**, 921—940 (13 Aug. 1912), dated 13 April, received 25 April; "Pflanzt sich der Stoß von Kanalstrahlen in einem festen Körper fort?" *Ibid.*, pp. 941—957, dated 20 April, received 25 April.

⁶⁵ *Ann. d. Phys.* **38**: 942/3.

⁶⁶ *Ibid.*, p. 946.

⁶⁷ *Ibid.*, p. 957.

⁶⁸ *Ibid.*, p. 939.

⁶⁹ Just how deeply STARK & WENDT do not say. They seem, however, to accept the results of Goldsmith, cited *ibid.* p. 926, which imply about 10^4 intermolecular distances.

⁷⁰ *Ibid.*, p. 939. Cf. the "channeling" technique described by L. ERICKSSON, J. A. DAVIES & J. W. MAYER in *Science* **163**, 627—633 (14 Feb. 1969).

⁷¹ J. STARK, "Bemerkung über Zerstreung und Absorption von β -Strahlen und Röntgenstrahlen in Kristallen", *Phys. ZS.* **13**, 973—977 (15 Oct. 1912), dated 26 Aug.

experiments of this sort could be performed.’⁷² What follows is a lengthy description of how one might expect the β -ray and the X-ray *Lichtzelle* (whose diameter is the λ obtained from $\lambda = c/v = ch/E$) to pass down the ‘shafts’ between the rows of molecules of the crystal lattice. Then STARK gives elaborate directions for setting up an experiment — an experiment which turns out, of course, to be identical with that of FRIEDRICH and KNIPPING. ‘It is remarkable,’ STARK finally

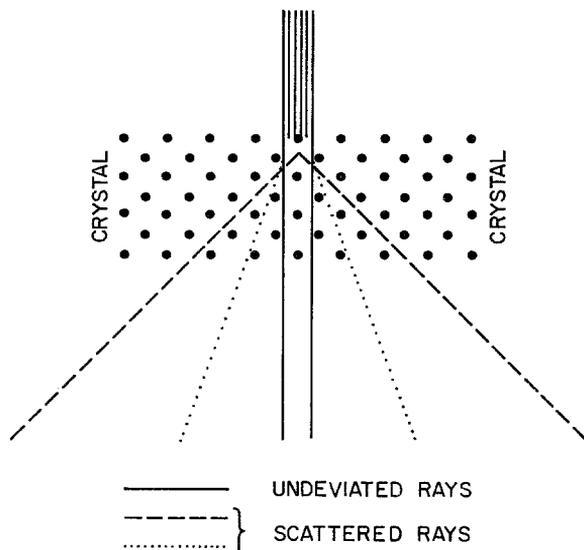


Fig. 2. The “diffraction” of X-rays according to J. STARK

comes to observe, ‘that I thought this through on the basis of the *Lichtzelle* hypothesis even before I had become acquainted with the observations which W. FRIEDRICH and P. KNIPPING, at the suggestion of M. LAUE, made on the scattering of X-rays in crystal plates, and which these authors interpret as an interference of X-rays.’⁷³

This is but a mild example of the sort of behavior which caused STARK to be regarded as an absolutely impossible man. But after discounting STARK’S penchant for deducing recent discoveries from his own peculiar models, there remains a distinct possibility that had STARK not been ‘hindered’, X-ray “diffraction” would have been discovered virtually simultaneously in Aachen under the guidance of the corpuscular theory. At the very least, it is evident that adherence to the ‘wave’ theory of X-rays was not a necessary condition for the discovery.

⁷² *Ibid.*, p. 974.

⁷³ *Ibid.*, p. 975. W. L. BRAGG also pointed out to his father, who was the principal English-language advocate of a corpuscular interpretation of X-rays, that when the X-ray beam is incident upon a cubic crystal in the (100) direction the LAUE-diagram could be construed by observing that “all the directions of the secondary pencils in this position of the crystal are ‘avenues’ between the crystal atoms”. W. H. BRAGG, “X-rays and crystals”, *Nature* 90, 219 (24 Oct. 1912), dated 18 Oct.

III. An Unpromising Proposal

Thus far we have granted that a possible route to the discovery of diffraction by crystals was a consistent application of the view that X-rays are classical electromagnetic radiation; we merely denied that this was the *only* possible route. But when we find that SOMMERFELD, the chief proponent of this view, regarded LAUE's proposal as so unpromising that he refused to interrupt the experimental program he had planned for FRIEDRICH,⁷⁴ we are obliged to reconsider our initial concession.

LAUE had a bright idea — to use a crystal as a diffraction grating for X-rays. But how would this idea have stood up when scrutinized in the light of the orthodox physics of the day, and especially of the 'wave' theory of X-rays, by 'the recognized masters of our science' whom LAUE consulted?⁷⁵ In fact, as we argue below, not very well. Yet if a SOMMERFELD or a WIEN had doubts about the feasibility of a proposed experiment, surely any adherent of the 'wave' theory of X-rays would immediately assent to LAUE's interpretation of the phenomenon which experiment then revealed? That was not, however, the case. While BRAGG and STARK, the advocates of a radical corpuscular view of X-rays, were immediately able to fit the observed phenomenon into their own theoretical framework, leading adherents of the wave theory found it difficult to do so. Thus Lord RAYLEIGH wrote BRAGG: "I am glad that you are giving attention to Laue & Co's spots and that you have an explanation w^h fits the facts."⁷⁶ Again C. G. BARKLA, the most vigorous and inflexible English-language advocate of the 'wave' theory of X-rays, confided to RUTHERFORD: "I have had a copy of Laue's paper for some little time and certainly am sceptical of any interference interpretation of the results. A number of features do not point in that way This in no way affects my absolute confidence of the truth of the wave theory of X-rays."⁷⁷

The circumstance that it was precisely advocates of the 'wave' theory who rejected LAUE's proposal, and who found the discovery difficult to assimilate, suggests, then, that the 'wave' theory, far from being uniquely favorable to the

⁷⁴ LAUE, Nobel lecture (1920), *Aufsätze u. Vorträge*, p. 11; FRIEDRICH, *Naturwiss.* **10**, 365 (1922). The personal relations between SOMMERFELD and LAUE were very poor at this time (LAUE to SOMMERFELD, 3 August 1920, in SOMMERFELD's correspondence; microfilm of this correspondence is deposited in the Archive for History of Quantum Physics). It is thus necessary to consider the possibility that personal rather than intellectual moments led to SOMMERFELD's refusal. Since, however, there is evidence, both direct and indirect, that to *any* adherent of the wave theory of X-rays LAUE's proposal would seem highly dubious, we may perhaps omit the personal factor.

⁷⁵ This phrase which LAUE used in his Nobel lecture was intended to refer principally to SOMMERFELD, but no less to W. WIEN. LAUE to P. P. EWALD, 1 May 1924, in A. SOMMERFELD's correspondence.

⁷⁶ Lord RAYLEIGH to W. H. BRAGG, 31 October 1912, in BRAGG's papers at the Royal Institution, London.

⁷⁷ C. G. BARKLA to E. RUTHERFORD, 29 October 1912, in RUTHERFORD's papers at the Cambridge University Library. Again, BARKLA & G. H. MARTYN, "An X-ray fringe system," *Nature* **90**, 647 (13 Feb. 1913): "We thus have what *appears* [BARKLA's italics] to be a series of X-ray spectra of different orders . . . Of the experimental results there can be no doubt, and we cannot at present suggest any probable explanation except the very obvious one of interference." In other words, the "very obvious" interference interpretation is very unsatisfactory.

discovery, actually rendered the discovery inaccessible to its adherents. And this is, after all, just what we might have anticipated from the wide diffusion of the 'wave' theory of X-rays and its status as the orthodox view.⁷⁸

1. Thermal Motion

In the early retrospective accounts the precise grounds upon which 'the recognized masters of our science' doubted the realizability of LAUE's proposal are not specified; LAUE, in 1920, and FRIEDRICH, in 1922, were silent on this point. Recent recollections, however, apart from a few abstentions, have been virtually unanimous that the single and sole ground was the thermal motion of the atoms of the crystal. On the ski trip which SOMMERFELD and a few of his colleagues took each year during the spring vacation (late March or early April) LAUE's proposal was discussed and dismissed. "It was argued", EWALD reports, "that the inevitable temperature motion of the atoms would impair the regularity of the grating to such an extent that no pronounced diffraction maxima could be expected." Both EWALD and FRIEDRICH named WILLY WIEN as one of those most strongly convinced that the thermal motion would prevent the experiment from succeeding (and, presumably, responsible for persuading SOMMERFELD of this), while EPSTEIN points to DEBYE.⁷⁹

The argument, as EWALD reconstructs it, would have been that if the amplitude of the thermal motion of an atom was comparable to the 'wavelength' of the X-rays, then the lattice would no longer be regular enough to give a distinct diffraction pattern. The 'wavelength' of the X-rays (a concept discussed in more detail below) emitted by a tube with a platinum anticathode operated under standard conditions had been estimated by both WIEN and SOMMERFELD as about 0.5 Å.⁸⁰ What then is the amplitude of the thermal motion in a crystal at room temperature? The papers of 1910 and 1914 on the eigenfrequencies and specific heats of solids, cited earlier for their testimony about the belief in space lattices, are also relevant to this question. These papers take it as reasonably

⁷⁸ It is also interesting to note that after the announcement of the FRIEDRICH-KNIPPING-LAUE phenomenon other physicists thought it worthwhile to try the experiments which STARK and WENDT had originally suggested: EDGAR MEYER encouraged WALTHER GERLACH to try the 'diffraction' experiment with α particles, assuring him, as GERLACH recalls, that "kein Versuch ist so dumm, daß man ihn nicht machen soll." *Physikalische Blätter* 19, 101 (1963). This attitude again argues that the newly discovered phenomenon was not perceived as a straightforward deduction from the wave theory, but rather partook of the character of an unforeseen discovery, like that of X-rays themselves, with its characteristic effect of liberating the scientific imagination.

⁷⁹ EWALD, *Fifty Years* (1962), p. 42. EWALD, who was not himself in Munich at the time, attributes the thermal motion story to LAUE and FRIEDRICH. (Interview with EWALD by Sources for History of Quantum Physics, 8 May 1962, p. 5.) Recently FRIEDRICH (interview by S. H. Q. P., 15 May 1963, pp. 2, 5, 10) has indeed advanced it, as has P. S. EPSTEIN (interview by S. H. Q. P., 25 May 1962 AM, p. 12). LAUE does not mention thermal motion as an obstacle to the experiment in any of his published retrospective accounts; neither did DEBYE in his interviews with S. H. Q. P. or with KERR and WILLIAMS make any mention of the thermal motion in this connection.

⁸⁰ Discussed in the papers cited in notes 60 and 61.

well established that the narrow absorption and emission bands which many crystals show in the infrared ("Reststrahlen") are the frequencies with which the individual atoms or molecules of the crystal vibrate with respect to one another.⁸¹ EWALD therefore quite justly suggests:

An evaluation of the thermal deformation of the crystal lattice could have been made by comparing the known average thermal energy of an oscillator at room temperature to that of an oscillator of amplitude A and frequency corresponding to a "Reststrahl" wavelength of, say, 50 microns as for rock salt or KCl. Assuming the mass of the oscillator to equal that of the chlorine atom, an amplitude A of about 0.75 Å is obtained. This is larger than the X-ray wavelength as given by WIEN (0.6 Å), or SOMMERFELD (0.4 Å), and thus the regular phase relations between the individual scattered wavelets, which are essential for the formation of a diffracted beam, would be destroyed.⁸²

But let us try to reconstruct EWALD'S calculation. At high temperature, $h\omega \ll kT$, the average energy of a linear harmonic oscillator is kT . Here h is PLANCK'S constant, ω the angular frequency of the oscillator, k "BOLTZMANN'S" constant, and T the absolute temperature. When the oscillator is at its maximum amplitude, \bar{A}_{\max} , this average energy is all potential, namely $\frac{1}{2}\alpha \bar{A}_{\max}^2$, where α , the "force constant," is equal to $m\omega^2$, m the mass of the oscillator. Thus the average maximum amplitude is given by:

$$kT = \frac{1}{2} m \omega^2 \bar{A}_{\max}^2, \quad \bar{A}_{\max} = \frac{1}{\omega} \sqrt{\frac{2kT}{m}} = \frac{\lambda}{\sqrt{2\pi c}} \sqrt{\frac{kT}{m}},$$

$$\bar{A}_{\max} = \frac{50 \times 10^{-4} \text{ cm}}{1.41 \times 3.14 \times 3 \times 10^{10} \text{ cm/sec}} \sqrt{\frac{1.4 \times 10^{-16} \text{ erg/}^\circ\text{K} \times 2.9 \times 10^2 \text{ }^\circ\text{K}}{1.66 \times 10^{-24} \text{ gram} \times 35}}$$

$$= 1.0 \times 10^{-9} \text{ cm.}$$

Thus $\bar{A}_{\max} = 0.10$ Å, not EWALD'S 0.75 Å. If, moreover we consider not the maximum amplitude but the root mean square amplitude, $\sqrt{\bar{A}^2} = \bar{A}_{\max}/\sqrt{2}$, we find 0.071 Å.⁸³ Thus the maximum disordering of the lattice by the thermal motion is only about 15 per cent of the X-ray 'wavelength', and such a calculation,

⁸¹ Papers cited in notes 44, 54 and 56. This interpretation is due to P. DRUDE, "Optische Eigenschaften und Elektronentheorie", *Ann. d. Phys.* **14**, 677—725, 936—961 (1904), p. 682.

⁸² EWALD, *Fifty Years of X-Ray Diffraction*, pp. 42/43.

⁸³ Our assumption of a *linear* harmonic oscillator can be justified both historically and physically. It was made, for example, by F. A. LINDEMANN, "Über die Berechnung molekularer Eigenfrequenzen," *Physikalische Zeitschrift* **11**, 609—612 (15 July 1910), who wrote precisely our relation $kT = \frac{1}{2}\alpha \bar{A}_{\max}^2$, and then solved the equation for ω . Physically, it is justified by the fact that only those displacements in the direction of the normal to the reflecting plane impair the constructive interference of the reflected wave trains. In truth, the root-mean square displacement in any given direction of the chlorine atoms in NaCl at room temperature, as determined from the temperature dependence of the interference maxima, is 0.13 Å. (R. W. JAMES, *The Optical Principles of the Diffraction of X-Rays* (London, 1962), pp. 236—239, and also pp. 20—25, 193—201.) The *principal* reason that the above relation *underestimates* the amplitude of the thermal motion is that, like the first post-discovery calculations of the effect of the thermal motion on the interference maxima, it treats the atoms as elastically bound to *fixed* equilibrium positions, rather than to their neighbors, thus neglecting the cumulative displacements produced by lattice vibrations whose wavelengths are long compared to the interatomic distance.

if it was actually performed, ought to have been encouraging rather than discouraging.

There were at least two other methods of computing the amplitude of the thermal motion in solids which were implicit in the contemporary discussion of eigenfrequencies, namely inferring the force constant of the atomic oscillator from the compressibility of the crystal (MADELUNG, SUTHERLAND), or from its melting point (LINDEMANN, A. STEIN). The first of these methods would probably have given somewhat larger values for the amplitude. The physical postulate of the second, and far more popular, theory — that at the melting point the amplitude of the thermal vibrations is equal to half the distance between the “surfaces” of the molecules — could lead to definite values for these amplitudes only when supplemented by values for molecular or ionic radii. The values then employed would have led, again, to $\bar{A}_{\max} \cong 0.1 \text{ \AA}$ at room temperature.⁸⁴

But did LAUE or SOMMERFELD feel any need for reassurance — and some such computation would, evidently, have provided it — that the amplitude of the thermal vibrations was considerably less than the ‘wavelength’ of the X-rays? Probably not. In the two years prior to LAUE’S proposal a very lively interest had arisen in the properties and consequences of the thermal motion in solids. Yet, curiously, throughout these discussions of eigenfrequencies, heat content, melting temperature, and electrical resistivity⁸⁵ no consideration was given to the actual numerical values of the amplitudes which entered repeatedly in the calculations. The reason for this is, however, not far to seek. The actual values of the amplitudes were of no interest precisely because the assumption upon which all these calculations rested was that these amplitudes were negligibly small in comparison with the interatomic distance — for only on that assumption could the thermal vibrations be regarded as harmonic, no matter what the actual form of the lattice potential. The first explicit numerical estimate of the amplitude of the thermal motion was, apparently, that which GRÜNEISEN included in a paper submitted for publication a month after the announcement of LAUE, FRIEDRICH, and KNIPPING’S discovery.⁸⁶

And yet, more curiously still, the warrant for this myth of the thermal motion derives directly from LAUE’S first paper on X-ray diffraction. Indeed EWALD’S assertion that the thermal motion “displaces the molecules over considerable

⁸⁴ F. A. LINDEMANN, “Über Beziehungen zwischen chemischer Affinität und Elektronenfrequenzen”, *Verh. d. Dtsch. Phys. Ges.* **13**, 1107—1116 (30 Dec. 1911). LINDEMANN gives (atomic diameter)/(interatomic distance)=80—90%, from which, using LINDEMANN’S assumptions, one would have been able to infer that at the melting point \bar{A}_{\max} =5—10% of the interatomic distance, or 2—4% $\cong 0.1 \text{ \AA}$ at room temperature. An estimate of the amplitude of the thermal vibrations on the basis of the compressibility of NaCl would have given about 0.3 \AA : P. DEBYE, *Verh. d. Dtsch. Phys. Ges.* **15**, 874 (15 Sept. 1913).

⁸⁵ F. A. LINDEMANN, “Untersuchungen über die spezifische Wärme bei tiefen Temperaturen. IV.”, *Preuss. Akad. d. Wiss., Berlin, Sitzungsber.* (6 Mar. 1911), pp. 316—321.

⁸⁶ E. GRÜNEISEN, “Theorie des festen Zustandes einatomiger Elemente”, *Ann. d. Phys.* **39**, 257—306 (24 Sept. 1912), received 14 July 1912; on pp. 296—298. Actually, F. A. LINDEMANN, *Über das Dulong-Petitsche Gesetz* (Doctoral Dissertation; Berlin, July 1911), p. 49, had mentioned in passing that the atoms of a metal “bei gewöhnlicher Temperatur, nur ganz kleine Oszillationen vollführen.”

fractions of the lattice constant a and therefore in some cases over several wavelengths"⁸⁷ is but a paraphrase of LAUE's statement:

Die Wärmebewegung bei den Molekülen verrückt diese nämlich schon bei Zimmertemperatur um einen erheblichen Bruchteil der Gitterkonstanten und infolgedessen um ein Vielfaches der Wellenlänge, ein Umstand, der durchaus der Berücksichtigung bedarf.⁸⁸

Does this then topple our argument and establish the contention of EWALD *et al.* that a consideration of the thermal motion formed the grounds for SOMMERFELD's refusal to support the proposed experiment? On the contrary, it enables us to state our position even more sharply: the very fact that this estimate of the amplitude of the thermal motion was in error by an order of magnitude argues that the amplitude question could scarcely have been the crux of the discussions of the feasibility of the experiment. For if it had, the error, whatever its source, would soon have been recognized, especially because it radically contradicted a tacit assumption of the contemporary theory of solids. We must therefore rather suppose that this estimate — and LAUE gives no indication how he came to it — was an afterthought whose fallacious character LAUE's contemporaries immediately recognized.^{89, 90}

2. Interference of Radiations in the Primary Beam?

The stress which recent retrospective accounts have laid upon the thermal motion in the crystal is not merely mistaken; it is also misleading. It leads one to skip over the logically and physically prior question whether LAUE and his contemporaries, who adhered to the 'wave' theory of X-rays, had reason to feel confident that in the absence of the thermal motion the experiment would succeed. Although it is often assumed, and sometimes asserted, that the theory of the diffraction of X-rays by a molecular space lattice had been worked out in advance of the experimental demonstration, it is almost certain that this was *not* the case.⁹¹

⁸⁷ EWALD, *Fifty years*, p. 51.

⁸⁸ LAUE, *op. cit.* (note 1), p. 309.

⁸⁹ R. W. POHL, *Die Physik der Röntgenstrahlen* (Braunschweig, 1912), in a "Nachtrag" on LAUE, FRIEDRICH, and KNIPPING's discovery, written sometime between June and August 1912, stated that "Der Abstand der Molekülzentra schwankt infolge der Wärmeschwingungen bei Zimmertemperatur nur um einige Proz." (p. 150). This remark, in passing, in a footnote, was sufficient, in POHL's view, to indicate that the serviceability of the "Kristallraumgitters als Beugungsgitter" was not impaired by the thermal motion.

⁹⁰ The influence of the thermal motion on the diffraction pattern was calculated by P. DEBYE, "Über den Einfluss der Wärmebewegung auf die Interferenzerscheinungen bei Röntgenstrahlen", *Verh. d. Dtsch. Phys. Ges.* **15**, 678—689 (15 August 1913), dated 26 July 1913; "Über die Intensitätsverteilung in den mit Röntgenstrahlen erzeugten Interferenzbildern", *ibid.*, pp. 738—752 (30 August 1913), dated 29 July. H. G. J. MOSELEY & C. G. DARWIN, "The reflection of X-rays," *Phil. Mag.* **26**, 210—232 (July 1913), p. 222, had given a simple qualitative argument that the thermal motion will only reduce the intensity of the diffraction maxima, and those of higher orders more than lower. JOHN L. HEILBRON, "The Work of H. G. J. Moseley", *Isis* **57**, 336—364 (1966), also gives background and references relevant to the following sections.

⁹¹ In his Nobel lecture (1920; *Vorträge u. Aufsätze*, p. 11), LAUE said that, "Die Theorie war ja eigentlich durch Übertragung vom gewöhnlichen und vom Kreuzgitter her schon vorher fertig", but as several (yet not all) stages in the experimental de-

And the fact that LAUE had not developed a theory until after a successful experiment had been performed argues that, thermal motion aside, nobody really knew what one ought to see or why one ought to see anything. It is, therefore, necessary to consider more closely the views about X-rays accepted in Munich, and the expectations about the interaction of X-rays with the molecular space lattice to which these views would presumably have given rise.

We have spoken of the 'wave' theory of X-rays as that which was advocated in Munich (and accepted almost everywhere else). The term used at the time, however, was the aether pulse, or impulse, theory, and this term expresses more accurately the sort of electromagnetic radiation X-rays were believed to be. In the X-ray tube, driven by an induction coil generating a maximum potential of about 40,000 volt, a high velocity cathode ray is stopped suddenly by impact with a platinum atom in the anticathode and emits an approximately square pulse of radiation whose width is of the same order of magnitude as the radius of an atom (Fig. 3). This view of X-rays as *Bremsstrahlung* had been put forward

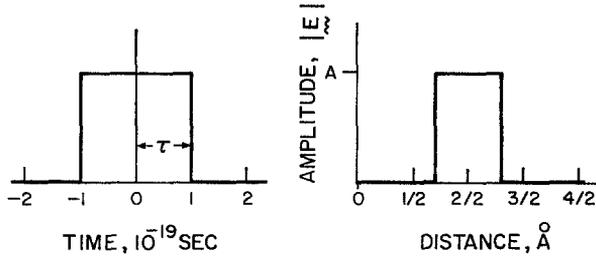


Fig. 3. X-ray as square pulse

immediately after RÖNTGEN'S discovery, and had been developed especially by SOMMERFELD.

On the other hand, after 1907 the picture was complicated by the discovery that each heavy element could emit one or more characteristic, highly homogeneous X-radiations, and that an X-ray tube emitted a substantial amount of the characteristic radiation of the material of the anticathode.⁹² (Although it was monstration were described before this assertion, the "schon vorher" is ambiguous. In 1922 FRIEDRICH (*Naturwiss.* 10, 366) evidently wished to make this point perfectly clear: "Wenn auch die Theorie der Interferenzerscheinung im Prinzip schon fertig war, so war sie doch von Laue noch nicht genauer durchgearbeitet; vor allen Dingen war die Form der Erscheinung noch nicht bekannt". Yet even FRIEDRICH goes too far, if only because, as we develop below, the phenomenon sought in the initial experiments was not that described by the theory to which he and LAUE refer, *i.e.* it was not diffraction of the incident beam by a three-dimensional grating. In his autobiographical sketch (1944; *Vorträge u. Aufsätze*, p. xxv) LAUE is quite explicit that only after seeing the first successful photograph "kam mir der Gedanke für die mathematische Theorie der Erscheinung". For completeness we mention that DEBYE, who had left Munich in 1911, recalled (interview by KERR and WILLIAMS, 22 Dec. 1965, p. 31) that LAUE had worked out the diffraction by a three-dimensional grating as an exercise and had mentioned, casually, to FRIEDRICH that some day he might look for this phenomenon. There is no reason to give this story the least credence.

⁹² P. FORMAN, "Charles G. Barkla", *Dictionary of Scientific Biography*, vol. 1 (New York: Scribners, in press); J. STARK, *Prinzipien der Atomdynamik* II. Teil (Leipzig, 1911), pp. 238—258; R. W. POHL, *Die Physik der Röntgenstrahlen* (Braunschweig, 1912).

generally taken for granted that homogeneity was equivalent to monochromaticity, R. W. POHL in a monograph on the *Physik der Röntgenstrahlen* published in 1912 could ignore that assumption and treat these homogeneous radiations as simply pulses of equal width.)⁹³

To which constituent of the X-ray beam could LAUE have looked for a diffraction phenomenon? At first glance the characteristic radiation of the anticathode (if one assumed it to be periodic and not just uniform pulses) would probably seem most promising. But SOMMERFELD had just completed his comparison of the photometric intensity profiles of WALTHER and POHL'S photographs with the calculated diffraction pattern of a wedge-shaped slit. The calculations, which originally stemmed from 1900, included only the square *Bremsstrahlung* pulse. 'And,' SOMMERFELD found, 'the comparison with the measurements has produced not a sign of a more periodic component.'⁹⁴ SOMMERFELD himself believed the characteristic radiation to be monochromatic and thought it the dominant constituent of the primary beam; to account for his own result he could only suggest that the wavelength of the periodic component may be much greater than the width of the pulse, in which case nothing was to be expected near the point of the wedge except the diffraction pattern of the pulse. Be that as it may, the conclusion to which this result pointed, and which the design of the initial experiments shows LAUE and FRIEDRICH accepted, was that, employing the yet narrower interatomic distances in a crystal, one could not look to a periodic component of the primary beam for a diffraction phenomenon.

On the other hand, SOMMERFELD felt he had found strong evidence of diffraction of the *pulses* by a slit; could not LAUE therefore have expected the pulses also to be diffracted by the space lattice of a crystal? But the result of the diffraction of a square pulse by a slit is merely a diffuse broadening of the geometrical image without any of the alternating maxima and minima which are characteristic for diffraction and the criterion by which it is ordinarily recognized.⁹⁵ This diffuse

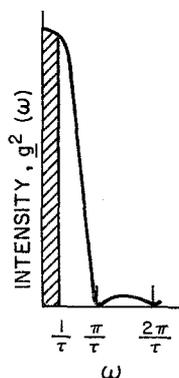
⁹³ *Ibid.*, pp. 73/74, 149/150.

⁹⁴ Note 61, p. 483.

⁹⁵ The Fourier transform of the square pulse of Fig. 3, $g(\omega) = \int f(t) e^{i\omega t} dt$, with

$$f(t) = \begin{cases} A, & -\tau \leq t \leq \tau \\ 0, & t < -\tau, t > \tau \end{cases}, \quad \text{is } g(\omega) = \sqrt{\frac{2}{\pi}} A \frac{\sin \omega \tau}{\omega}.$$

Thus all wavelengths greater than $2\pi c \tau$ ($\cong 2 \text{ \AA}$) occur with about equal intensity, while the intensity of wavelengths less than $2c\tau$ ($\cong 0.6 \text{ \AA}$) is reduced by a factor greater than 25. There is then, effectively, a shortest wavelength component in the radiation. If this component is diffracted by a slit of width d the first intensity minimum in the diffraction pattern appears at an angle θ such that $\sin \theta \cong \lambda/d$. But this minimum is overlaid by the pattern due to $\lambda + d \lambda$, etc., and as all these wavelengths are of equal intensity there are no alternations of maxima and minima in the pattern. (For SOMMERFELD'S derivation of this result see his "Theoretisches über die Beugung der Röntgenstrahlen," *Zeitschrift für Mathematik und Physik* 46, 11—97 (1901), reprinted in SOMMERFELD'S *Gesammelte Schriften* (Braunschweig, 1968), 4, 240—326, esp. pp. 324—326).



broadening of the collimated X-ray beam might argue for the 'wave' theory when produced by a narrow slit; it certainly would no longer do so if it arose from the passage of the beam through matter.

The crystal, however, is not a slit. It is a grating of some sort. Now a line grating, or a crossed grating, will show something characteristic even with a pulse. Namely, just as with white light, there will be a central spot due to the transmitted beam, and then off to the sides, separated by a dark gap, the continuous spectrum will begin with the shortest wavelengths and spread outward. But a crystal is an indefinitely large number of crossed gratings tilted with respect to one another (Fig. 4). At first sight it would probably appear to LAUE, SOMMER-

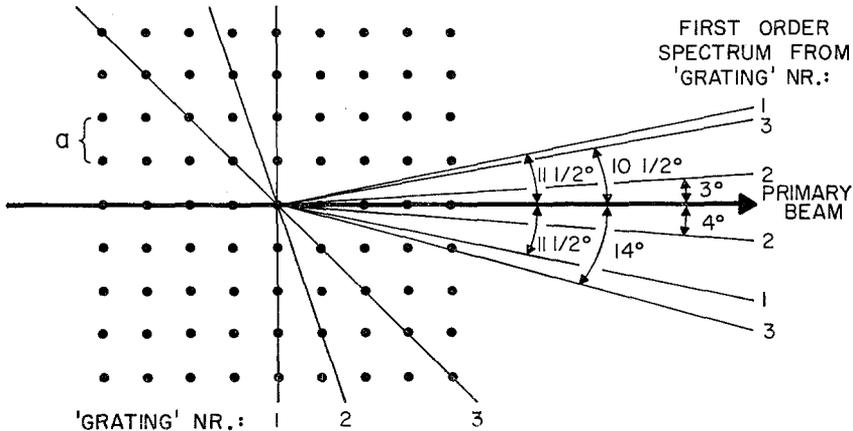


Fig. 4. Crystal lattice as a collection of tilted gratings. (Drawn for $\lambda_{\min} = 0.2a$)

FELD, and their colleagues that even with pulses of a single width the result would be an indefinitely large number of spectra with angular deviations ranging all the way down to zero. Thus the *Bremsstrahlung* pulses in the primary would produce a general diffuse darkening of a photographic plate set up behind the crystal — a result which, again, would scarcely argue for diffraction. There is still less reason to expect a distinctive interference phenomenon if one adds, as SOMMERFELD did, that 'The unavoidable variability of the hardness of the X-rays during a long exposure causes one to expect not impulses of a single width, but rather a continuous series of widths.'⁹⁶ The same circumstance also results from the fact that an induction coil produces a continuum of accelerating voltages during the course of each discharge. In fact, when LAUE finally did work out the theory of diffraction by a three-dimensional grating, he found that it justified this conclusion in the sense that with a perfectly heterogeneous beam (*i.e.*, one in which all wavelengths are represented with equal intensity) 'the possible interference maxima lie densely everywhere, so that the photographic plate must be completely blackened.'⁹⁷ Of course, from the classical point of view the pulse does not contain arbitrarily short wavelengths in appreciable intensity, and from

⁹⁶ Note 61, p. 483.

⁹⁷ LAUE, "Zusätze (März 1913)," to the reprinting in the *Ann. d. Phys.* **41**, 989—1002 (1913) of "Eine quantitative Prüfung der Theorie für die Interferenzerscheinungen bei Röntgenstrahlen", presented to the Bayer. Akad. d. Wiss. on 6 July 1912; 2. Zusatz, p. 1000.

the quantum viewpoint it does not contain them at all. Yet a year after the discovery LAUE was still maintaining, quite mistakenly, that the result quoted was a decisive refutation of every attempt to regard the LAUE-diagram as produced by X-radiation with a continuous distribution of wavelengths. This puzzling refusal to see what we now recognize to be the very essence of the LAUE-diagram is evidence of a strong anterior conviction that a distinctive diffraction effect could only be due to a limited number of discrete, monochromatic components in the X-radiation. In other words, LAUE had ruled out the pulses before he had worked out the theory.

It appears, therefore, that the views held in Munich (and elsewhere) of the nature of X-rays, far from being uniquely favorable to the discovery of their diffraction by crystals, actually precluded the observation of diffraction in the sense in which we now usually understand the term — *i.e.*, the interference of radiations scattered out of the primary beam.

3. *Interference of the Characteristic X-Radiations of the Atoms of the Crystal?*

The radiations in the primary beam were not, however, the only ones to be considered. If the crystal employed contained elements of atomic weight greater than about 40 (calcium), then, as one knew from BARKLA'S work, the primary beam would excite copious emissions of the characteristic radiations of these elements. "In most cases," BARKLA warned, "unless special precautions are taken, the ionizing effect of these radiations from any particular element is much greater than that of the scattered X-rays — indeed it completely swamps the effect of the latter."⁹⁸ Perhaps, then, one should make a virtue of a necessity and look for interference effects due to the characteristic radiation emitted by the atoms of a crystal? This is, in fact, what LAUE and FRIEDRICH were looking for in their initial experiments.⁹⁹

Neither LAUE nor FRIEDRICH could have presented SOMMERFELD with any argument for the probable success of such an experiment. First of all, there was again the problem that these characteristic X-rays had not made themselves evident as a monochromatic radiation in the diffraction pattern of a slit. A far more serious objection, however, would certainly have arisen. These characteristic X-rays were also known as "fluorescent X-rays" — with good reason. Their lower frequency, their isotropic distribution, their lack of polarization showed clearly that they were not emitted through a direct resonance with the radiation in the primary beam. Thus there was no reason for assuming any coherence, any determinate phase and polarization relations, between the characteristic radiations emitted at different points in the crystal. What sort of interference effect could one then possibly hope for?¹⁰⁰ Far from being a mere extension of an optical

⁹⁸ C. G. BARKLA, "The spectra of the fluorescent Röntgen radiations", *Phil. Mag.* **22**, 396—412 (Sept. 1911), on p. 399.

⁹⁹ "Da wir anfangs glaubten, es mit einer Fluoreszenzstrahlung zu tun zu haben, mußte ein Kristall verwendet werden, der Metall von beträchtlichem Atomgewicht als Bestandteil enthielt ..." FRIEDRICH, KNIPPING & LAUE, "Interferenz-Erscheinungen bei Röntgenstrahlen", *Bayer. Akad. d. Wiss., Sitzungsber.* (1912), p. 314.

¹⁰⁰ Following a false alarm in 1923 (WM. DUANE, G. MIE) this exceedingly weak effect was found in 1935 by GERHARD BORRMANN, working in WALTHER KOSSEL'S laboratory: "Röntgenlichtquelle in Einkristall", *Naturwiss.* **23**, 591—592 (22 Aug. 1935).

experiment from a two-dimensional to a three-dimensional transmission grating, this was an experiment without analogy or precedent. No wonder SOMMERFELD refused machine time.

IV. Experiment, Discovery, Publication

Although LAUE could not say why the experiment should succeed, or indeed just what "success" would look like, neither could he bring himself to discard his bright idea. Fortunately, SOMMERFELD'S experimental assistant, WALTER FRIEDRICH, the only young physicist at the university with a fair measure of experience with X-rays,¹⁰¹ declared himself ready to try the experiment, and, evidently, was prepared to do so even without his chief's blessing. For one reason or another, however, — and here we follow a letter from LAUE to EWALD written twelve years after the event — at the beginning of April FRIEDRICH wanted to defer the experiment. LAUE, impatient, put pressure upon FRIEDRICH by inducing PAUL KNIPPING to take up the question. KNIPPING, a doctoral student of RÖNTGEN'S, called "the watchmaker" on account of his skill in instrumental matters, was apparently just then preparing to do a series of experiments, in RÖNTGEN'S institute, on the passage of X-rays through metals. The result was, LAUE recalled, that it then went just as in *Wallenstein*: "Wenn es denn doch geschehen soll und muß, so mag ich's diesem Pestaluz nicht gönnen."¹⁰²

¹⁰¹ W. FRIEDRICH, "Intensitätsverteilung der X-Strahlen, die von einer Platinaantikathode ausgehen", *Ann. d. Phys.* 39, 377—430 (24 Sept. 1912). This is FRIEDRICH'S dissertation, which was accepted in July 1911, and for which the research was completed a year earlier still. If one may judge from this paper, FRIEDRICH may well have had no first hand experience with fluorescent X-rays; in general, this was a field which the German physicists avoided.

¹⁰² This version, offering a rather different perspective on the background of the first experiments than that fostered by FRIEDRICH'S recollections, is intimated in LAUE to EWALD, 1 May 1924 (Sommerfeld Correspondence, Archive for History of Quantum Physics). LAUE'S *implication* that KNIPPING was in a position to attempt such experiments in RÖNTGEN'S institute is supported by P. KNIPPING, "Durchgang von Röntgenstrahlen durch Metalle. Bemerkung zur Veröffentlichung des Herrn Hupka," *Phys. Zeitschr.* 14, 996—998 (15 Oct. 1913), dated "mitte Juli 1913": "Ich bemerke zunächst, daß ich mich bereits vor mehr als einem Jahre mit derselben Materie beschäftigt habe." On the other hand, KNIPPING'S doctoral dissertation, *Über den Einfluß der Vorgeschichte auf verschiedene Eigenschaften des Bleies* (Borna-Leipzig, 1913), presented to the Munich Philosophical Faculty on 28 February 1913, shows that he had made some independent X-ray diffraction experiments on metal crystals, but gives no indication that he had begun to work with, or planned to work with X-rays before he became involved with LAUE and FRIEDRICH.

The lines, which LAUE quotes from memory, are spoken by MACDONALD in act 5, scene 2 of *Wallenstein's Tod*: "Ja wenn/Er fallen muß und soll und's ist nicht anders./ So mag ichs diesem Pestalutz nicht gönnen." Their sense is that if WALLENSTEIN (the experiment) is to be killed (performed) in any case, then MACDONALD (FRIEDRICH) would rather do it himself than see Captain PESTALUTZ (Doktorand KNIPPING) reap the reward. KNIPPING'S moniker "the watchmaker" is reported by DEBYE, interview by D. M. KERR, JR., and L. P. WILLIAMS, 22 Dec. 1965, p. 31. While we have dismissed (Footnote 91) the supposed casualness with which LAUE proposed the experiment to FRIEDRICH, we might understand the casualness of the response which DEBYE attributed to FRIEDRICH — "Oh, yes, some day I will come to it" — as referring to the circumstance that at one point FRIEDRICH wanted to defer the experiment.

In choosing a crystal FRIEDRICH looked to BARKLA'S recent review article for guidance. "The radiations from elements of atomic weights in the neighborhood of iron, copper, zinc, etc., when subject to a radiation of ordinary penetrating power, are the most homogeneous, that is contain the smallest proportion of scattered radiation"; this proportion being perhaps one per cent.¹⁰³ "In a number of cases the characteristic radiations from elements were obtained using compounds containing the elements in combination with light atoms. The only effect of these light atoms was then to add a little scattered radiation to the fluorescent radiation which it was desired to study. The effect of this was, however, negligible."¹⁰⁴ The ideal crystal, FRIEDRICH would have been led to conclude, was one containing elements of atomic weight in the range 55—65, but in combination with light elements so that its density, and thus its absorption of the fluorescent X-rays, was low. Fitting very well with these conditions was copper sulfate ($\text{Cu}_2\text{SO}_4 \cdot 5\text{H}_2\text{O}$; specific gravity 2.3), of which well developed crystals were readily obtained.

In the design of the apparatus a guiding principle must certainly have been that a distinct diffraction effect would be most likely to occur with a narrow, well collimated X-ray beam. Thus all but a small fraction of the radiation from the tube had to be blocked, and the exposition times had to be correspondingly long. But just what were the beam diameters and exposure times in the initial experiments? In 1922 FRIEDRICH spoke of exposures of several hours (*mehrstündig*), and by 1963 of ten hours.¹⁰⁵ It seems certain, however, that he was transferring the conditions of later experiments, with a far finer apparatus, back to the first, exploratory, experiments. The sealed note of 4 May (see Fig. 1 and note 1) gives the exposition time as 30 minutes with a beam 1.5 mm in diameter — substantially narrower than that employed in the initial experiments.¹⁰⁶ It is therefore most unlikely that the initial experiments themselves involved exposure times greater than half an hour.

Finally, the photographic plate, or plates, had to be set up. On this point, the actual positioning of the plates in the first experiment, the accounts are at variance and it does not seem possible to settle the question with certainty. FRIEDRICH was quite explicit in 1922 that the photographic plates 'were set up parallel to the primary beam.'¹⁰⁷ And this would indeed have appeared to be the optimal arrangement for detecting interference effects due to the characteristic radiation of the atoms of the crystal, *if* that characteristic radiation were fairly penetrating. In such a case all positions would intercept the characteristic radiation

¹⁰³ Note 61, pp. 399/400, 404/405.

¹⁰⁴ Note 61, p. 406.

¹⁰⁵ FRIEDRICH, *Naturw.* **10**, 366 (1922); S. H. Q. P. interview, 15 May 1963, p. 2. Already in April 1913 LAUE, "Interférences de rayons Röntgen produites par les réseaux cristallins", *Archives des sciences physiques et naturelles, Geneva*, **35**, 391 (1913), had told the Société Suisse de Physique this was the "raison pour laquelle ces interférences ont échappé si longtemps aux observations", namely "il faut exposer de longues heures pour les voir."

¹⁰⁶ FRIEDRICH, KNIPPING, LAUE, *Bayer. Akad. Sitzungsber.* (1912), p. 316.

¹⁰⁷ FRIEDRICH, *Naturwiss.* **10**, 366. In his address to the First Congress of the Internat. Union of Crystallography in 1948 (see Footnote 3) LAUE also asserted that in the first attempts the plates were placed parallel to the X-ray beam.

equally well since it is isotropic. But placing the plates parallel to the beam minimizes the exposure to background radiation due to: i) stray radiation from X-ray tube, diaphragms, and beam absorber; ii) radiation scattered by the crystal out of the primary beam;¹⁰⁸ iii) passage of the primary beam through the photographic plate.

A difficulty arises, however, when we consider that, as was well known at the time, CuK-radiation is not very penetrating. In fact, it is reduced in intensity by a factor e^{-1} in traversing 0.1 mm of aluminum, and almost as much in traversing the same thickness of copper sulfate; it would thus be completely absorbed before penetrating 1 mm.¹⁰⁹ If this fact was taken into consideration, then an effect could have been anticipated only from the characteristic radiation emitted by copper atoms within a few tenths of a millimeter of the surface at the points where the primary beam entered and emerged from the crystal. In this case the photographic plate ought certainly be placed *perpendicular* to the primary beam either in front of or behind the crystal. This is, in fact, the way EWALD asserts the experiment to have been set up — “The photographic plate was placed between the X-ray tube and the crystal on the assumption that the crystal would act like a reflection grating” — although the reason EWALD gives is scarcely applicable to the experiment as then conceived.¹¹⁰ In either case — whether the plates were placed to the sides of the crystal or in front of it — the first experiment could not and did not succeed. Only when in a subsequent attempt a plate was set up behind the crystal was a distinctive result obtained.

Now, of course, there was great excitement. SOMMERFELD was enthusiastic — too enthusiastic, LAUE thought. FRIEDRICH'S assigned research program was tossed aside. The resources of SOMMERFELD'S institute were put at FRIEDRICH and KNIPPING'S disposal, and in less than two weeks a new apparatus was constructed, the “definitive” apparatus illustrated in their first publication. On the 4th of May the discoverers took advantage of the monthly meeting of the mathematical-physical class of the Bavarian Academy of Sciences in order to secure their priority. SOMMERFELD deposited for them a sealed envelope containing two of the earliest photographs obtained with this new apparatus and a statement of the idea behind their experiment (Fig. 1).¹¹¹ Soon after a thin, accurately oriented crystal plate arrived from the firm Steeg und Reuter — a plate just like those which STARK had been buying from that firm — and they obtained the handsome, symmetrical patterns displayed in their first paper. Now it was necessary to have a theory of the phenomenon, and LAUE, unable to provide one for *this*

¹⁰⁸ The “Thomson” scattering distribution, $I(\theta) = (1 + \cos^2 \theta) I(\pi/2)$, gives the minimum intensity in the plane perpendicular to the primary beam.

¹⁰⁹ $I = I_0 e^{-\lambda x} = I_0 e^{-(\lambda/\rho)\sigma}$, where ρ is the density, and σ the surface density, of the substance. Using BARKLA'S data, for CuK, λ/ρ in Al is about 50, so that $I = I_0 e^{-1}$ for $\sigma = 1/50$ gm/cm², or $x \cong 0.01$ cm.

¹¹⁰ EWALD, *Fifty Years*, p. 44.

¹¹¹ Protokoll über die Sitzung der mathem.-physik. Klasse der kgl. Bayerischen Akademie der Wissenschaften zu München vom 4. Mai 1912, Ziffer 7): “Ferner übergibt Herr Sommerfeld ein versiegeltes Kouvert mit 2 Films zur Wahrung der Priorität einer wissenschaftlichen Entdeckung.” For this, and the following, extract from the minutes of the math.-phys. class I am indebted to Oberregierungsarchivrat Dr. R. FRITZ of the Archiv der Bayerischen Akademie der Wissenschaften.

phenomenon, gave instead a theory of a three dimensional diffraction grating.¹¹² This first paper, consisting of a 'Theoretical Part' by LAUE, followed by an 'Experimental Part' by FRIEDRICH and KNIPPING, was presented by SOMMERFELD at the following meeting of the mathematical-physical class of the Bavarian Academy on 8 June; the paper was accepted, and it was further resolved to spare no expense in the reproduction of the photographs.¹¹³ A day or two earlier, however, LAUE had already begun publication of his discovery by sending colleagues, especially eminent colleagues, one of the photographs, and a day or two later LAUE set out on a lecture tour; the printed publication was, however, eleven weeks in appearing.¹¹⁴

V. Retrospect: Mythicization

The function of myth, briefly, is to strengthen tradition and endow it with a greater value and prestige by tracing it back to a higher, better, more supernatural reality of initial events.

Myth is, therefore, an indispensable ingredient of all culture. It is, as we have seen, constantly regenerated; every historical change creates its mythology, which is, however, but indirectly related to historical fact.¹¹⁵

¹¹² It is uncertain at just what point between 23 April and 8 June LAUE worked out his theory of the phenomenon. Although LAUE was to claim many years later that it was immediately after he saw the first successful photograph, we ought certainly to give greater credence to FRIEDRICH's statement in the spring of 1913, *Physikalische Zeitschrift* 14, 317 (15 April 1913), that the interference experiments were made on the basis of a "Vermutung" of LAUE's, and that only after these experiments resulted in "sehr schöne und äusserst regelmässige Interferenzfiguren" did LAUE work out a theory: "An Hand dieser Figuren hat dann Laue eine eingehende Theorie entwickelt," and FRIEDRICH here cites the "Theoretischer Teil" of the first paper as well as LAUE's second, quantitative paper. It is thus possible, indeed probable, that the theory presented in the first paper was not worked out before the middle of May. (In a subsequent paper I hope to discuss in some detail the discordance between LAUE's physical conception of the phenomenon and his mathematical theory of it.)

¹¹³ Protokoll, 8 Juni 1912, A). Vortrag 1).: "Herr A. Sommerfeld legt eine Arbeit vor 'Interferenzerscheinungen mit Röntgenstrahlen beim Durchgang durch Krystalle' von W. Friedrich, P. Knipping und M. Laue. Es gehören dazu Abbildungen, für deren Herstellung ein Kostenvoranschlag von A. Köhler im Betrage von 242—260 M vorliegt. Die Klasse beschließt die Aufnahme in die Sitzungsberichte. Die Abbildungen sollen möglichst gut ausgeführt werden, auch wenn dazu ein höherer Betrag als der von Köhler veranschlagte notwendig sein sollte. Für den Buchhandel sollen 100 Exemplare gedruckt werden."

¹¹⁴ W. GERLACH, "Münchener Erinnerungen aus der Zeit von Max von Laues Entdeckung," *Physikalische Blätter* 19, 97—103 (1963), and the congratulatory letters from EINSTEIN (10 June) and ZEEMAN (16 June) in the Handschriftensammlung of the Deutsches Museum, München. On 6 July LAUE presented, through SOMMERFELD, to the Bavarian Academy "Eine quantitative Prüfung der Theorie für die Interferenzerscheinungen bei Röntgenstrahlen," in which he applied his theory to one of the photographs. The first printed publication of the discovery was a separatum comprising these two papers, and distributed in late August. (LAUE to P. GROTH, 25 August 1912, Bayerische Staatsbibliothek, Munich.) GERLACH, *loc. cit.*, quotes a letter dated 2 August 1912 to EDGAR MEYER in which LAUE complains: "Wann die Abhandlung erscheint? — das weiß der Himmel ... Immerhin hoffe ich als Optimist, daß die Versendung der Sonderdrucke noch im September möglich sein wird." However, as the "noch" suggests, for "September" we must read "August".

¹¹⁵ B. MALINOWSKI, "Myth in Primitive Psychology" (1926), pp. 93—148 of *Magic, Science, and Religion and Other Essays* (Garden City, 1954), on p. 146.

In the preceding sections the traditional account of the conceptual obstacles to the discovery of the diffraction of X-rays by crystals has been subjected to a critical examination. We found that the principal theses of that account are gross misrepresentations of the conceptual situation. Moreover, the accounts by participants, or onlookers, of the immediate circumstances of the discovery were found to be generally unreliable, and although some progress was made by selecting the more probable of two conflicting accounts, the several accounts were equally often unanimous in their misstatements. Our investigation thus serves, at the very least, to emphasize the reserve with which any "historical" assertion by a scientist must be handled. But it is perhaps possible to go further, to consider the process of mythicization itself in the hope of finding guidelines for interpreting those statements by scientists which purport to be historical.¹¹⁶ Moreover, from the perspective of the sociology of science the myth, as an element of the culture of the fraternity of crystallographers or physicists, has an intrinsic interest. It is to be analyzed not for its historical information but for its socio-cultural function. The appropriateness, indeed the necessity, of such an analysis derives from the circumstance that the scientist, *qua* scientist, places no value upon historical fact; history is wholly subordinated to the needs of the present, and indeed only survives to such extent, and in such form, as serves present needs.¹¹⁷ We therefore begin our discussion by borrowing this essentially anthropological perspective, afterwards taking up the problem of specifying the relation of myth to historical reality.

As we have already emphasized in Section I, the traditional account of the discovery of the diffraction of X-rays by crystals has generally been recited to celebrate an important anniversary of that event and/or before a large gathering of the clan of X-ray crystallographers. This circumstance, and its evident social function of reinforcing a separate identity, strongly suggests that the traditional account may be regarded as a "myth of origins," comparable to those which in primitive societies recount the story of the original ancestor of a clan or tribe.

The traditional account can, of course, be traced back to its inventor, LAUE, and at that point an analysis of its function must refer to LAUE's largely personal

¹¹⁶ The problem is similar to that facing the historian of primitive societies; this latter problem has been analyzed very carefully by JAN VANSINA, *Oral Tradition; A Study in Historical Methodology*, trans. H. M. WRIGHT (Chicago, 1965); first published in 1961 as *De la tradition orale*. I am grateful to CHARLES WEINER for drawing this work to my attention.

¹¹⁷ It might appear that inasmuch as physicists have a most vivid interest in questions of "priority" — assertions of original authorship of still accepted discoveries or theories — they can scarcely be said to place no value upon historical fact. But even if we put aside the circumstance that the very notion of priority is historiographically and epistemologically untenable (*e.g.*, T. S. KUHN, "The historical structure of scientific discovery," *Science* 136, 760—764, June 1962), the point is precisely that the physicist's obligation to history begins and ends with questions of "priority". Thus if he does not touch upon questions of original authorship, the physicist is free to represent the history of his field (*e.g.*, the conceptual situation at a particular time, or even the chronologic order of discoveries) in whatever way he finds convenient. That is, so long as he avoids questions of "priority", his colleagues are not obliged — indeed, not even entitled — to criticize his exposition on the grounds that the historical facts are stated incorrectly. A situation more conducive to myth-making can scarcely be imagined.

motives — his desire to explain how he, a man of no great originality (for so he regarded himself), came to conceive this experiment; his desire to acknowledge a debt to the Munich intellectual milieu, as distinct from SOMMERFELD personally. This circumstance does not, however, conflict with our attribution of a social function to the traditional account, but merely suggests that the specific details of that account are to some extent irrelevant to its social function; *qua* myth of origins its utility is largely independent of its content. At the same time, *qua* myth of origins, the account of the discovery will tend to be elaborated in quite predictable ways. Thus the myth will the better serve its social function the more numerous and difficult are the obstacles to be surmounted by the mythic hero. It is in this way that we may understand the increasingly categorical assertions of the disreputability of the space lattice theory. So also may we understand the assertion that the first experiments involved exposures of many hours, when it is almost certain that in fact they did not last thirty minutes.

The physicist, however, demands something more from his myths than does the savage — they are to be consonant with what he knows to be good physics, and they are to be internally consistent, even if implausible. LAUE's original account satisfied the first criterion, but at a number of points was deficient in logical consistency. In particular, if the Munich milieu, permeated by the wave theory as well as the space lattice theory, was uniquely favorable to conceiving the diffraction experiment, why did SOMMERFELD, and others, refuse LAUE support and encouragement? In order then to eliminate this inconsistency the myth must be further elaborated; a stratum of metamyths of justification is thus laid over the original account. And so we have the thermal motion myth.

The foregoing observations, especially the recognition that the myth itself may stimulate further inventions, offer little hope of a definite and invariable relation between the myth and historical reality. Nonetheless, if myths are not entirely fanciful inventions, *i.e.* if there is some historical circumstance which is *referred to* by the myth (although, to be sure, misrepresented by it), then it may still perhaps be possible to educe interpretive guidelines which will help us to form an idea of that historical circumstance more accurate than the representation contained in the myth. In any case, a recapitulation of the myths encountered and their probable referents should give us a better grasp of the process of mythicization.

We consider first the "Cauchy relations" myth, for although it was a rather late addition to the account of the discovery of the diffraction of X-rays by crystals, it had already come into existence in another connection before 1912. In the mid-19th century the invalidity of the CAUCHY relations was indeed held to argue against inalterable, extensionless, monopole molecules; in VOIGT's version of the myth (1900, 1910) this becomes a refutation of the molecular hypothesis; in EWALD's version it becomes a refutation of the regular internal arrangement of the molecules in a crystal. Thus the very points which, historically, no one thought to call in question, become in the myth the points which everyone doubted. We may therefore understand EWALD's version of the CAUCHY relations myth as referring to (but inverting) the circumstance that despite the experimental evidence against the rari-constant theory, the physicists were not prepared to question the assumption of a space lattice.

Consider next LAUE's myth that among crystallographers the space lattice theory was "hardly mentioned anymore." The myth just inverts the real situation — the theory was very widely, almost universally, mentioned, although in *certain* contexts a small minority avoided doing so. Thus we may understand the myth as referring to the circumstance that before the discovery of the diffraction of X-rays by crystals it was *possible* for a crystallographer to write an introductory treatise without discussing the space lattice theory. Closely connected with the "hardly mentioned anymore" myth is the assertion of LAUE's ignorance of the space lattice hypothesis — before arriving in Munich, or before the winter of 1911/12, or before We are not able to say anything stronger than that this is highly implausible. It might be construed as referring to the phenomenologic predispositions of the men with whom he studied, especially the professors of physics and mineralogy at Göttingen: W. VOIGT, E. RIECKE, and TH. LIEBISCH — all three of whom omitted all discussion of the microscopic structure of crystals in their introductory texts, but who, needless to say, made ample use of such theories in their own researches.

Next there is the thermal motion myth, which has the physicists giving such weight to a consideration of the amplitude of the thermal vibrations that they were prepared to reject LAUE's proposal on these grounds. Here, again, as we have seen, the situation is approximately the exact opposite. We may rather understand the myth as referring to (but again inverting) the circumstance that the physicists showed astonishingly little concern about the amplitude of the thermal motion, disregarding it as negligible without subjecting it to a close examination. Indeed, so little attention had been given to the amplitude of the thermal motion that when LAUE himself obtained a figure an order of magnitude too large he was astonished and puzzled by his result, but failed to recognize that it had to be wrong.

In any case, as we have argued, this myth is needless, because the interpretation of the discovery which it shores up is also a myth. Far from being uniquely favorable to the discovery of the diffraction of X-rays by crystals, the impulse theory advocated in Munich precluded the existence of a distinguishable interference pattern, and LAUE's interpretation of the phenomenon was received with scepticism by adherents of that theory.

Finally we may consider briefly the "liquid crystals" myth mentioned in Footnote 52. In his *Geschichte der Physik*, LAUE naturally advanced the space lattice myth, and elaborating upon it explained how the physicists — not believing in the space lattice — conceived the internal structure of crystals. 'Many,' LAUE said, took the view that in crystals as in liquids the centers of gravity of the molecules are distributed randomly, and that the anisotropy of crystals arises solely from parallel alignment of physically distinguished directions in the molecules. Although one might think that this was pure fantasy on LAUE's part, and that our assumption that myths refer to some historical circumstance is therefore unjustified, in fact this myth also refers to (and inverts) a real situation — namely, that no one, not even the one man (OTTO LEHMANN) to whom such opinions had been ascribed, actually held this view.

This last example is amusing; it also suggests an hypothesis about the processes of mythicization, with its concomitant phenomenon of inversion. An opinion

which historically was beyond the fringe, which was decidedly unorthodox and which, for one reason or another, the orthodox scientists regarded as dangerous, becomes in the myth the dominant, orthodox opinion in that science. The myth then has that threatening "widespread" opinion being overthrown by the mythicized event or discovery. The applicability of this hypothesis to the myths discussed above, especially the space lattice myth, is clear. It seems likely that it could also be helpful in analyzing a number of other characterizations of the conceptual situation in physics at the end of the 19th century, *e.g.* that there was a general disbelief in atoms, or "that," in MAXWELL'S well-known words, "in a few years all the great physical constants will have been approximately estimated, and that the only occupation which will then be left to men of science will be to carry these measurements to another place of decimals."¹¹⁸

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¹¹⁸ J. C. MAXWELL, "Introductory lecture on experimental physics" (October 1871), *Scientific Papers* 2, 244. Needless to say, MAXWELL rejected this opinion, categorically. For the physicists' continuing fascination with the question of the origin of this opinion, see the literature cited by S. G. BRUSH, *Physics Today* (January 1969), p. 9, who raises instead the historically significant questions "why would anyone think it was a valid description of the state of physics," and how did "the myth of a Victorian calm in physics" become established? On atomism, see HEILBRON, *op. cit.* Footnote 53, and S. G. BRUSH, "Mach and atomism," *Synthese* 18, 192—215 (1968).

Department of History
University of Rochester
New York 14627

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