Jagdish Mehra
Helmut Rechenberg

The Historical Development of Quantum Theory

VOLUME 3

The Formulation of Matrix Mechanics and Its Modifications 1925–1926

Jagdish Mehra
Helmut Rechenberg

The Historical Development of Quantum Theory

VOLUME 3

The Formulation of Matrix Mechanics and Its Modifications 1925–1926

Springer-Verlag
New York Heidelberg Berlin

Library of Congress Cataloging in Publication Data
Mehra, Jagdish.
The formulation of matrix mechanics and its modifications, 1925–1926.
(The historical development of quantum theory; v. 3)
Bibliography: p.
Includes index.
1. Matrix mechanics. I. Rechenberg, Helmut, joint author
I. Title. III. Series: Mehra, Jagdish. The historical development of quantum theory; v. 3.
QC173.98.M44 vol. 3 [QC174.3] 530.1'2109s 82-3253
[530.1'22] AACR2

Printed in the United States of America.

9 8 7 6 5 4 3 2 1

ISBN 0-387-90675-4 Springer-Verlag Berlin Heidelberg New York
ISBN 3-540-90675-4 Springer-Verlag New York Heidelberg Berlin

Contents

Foreword

This volume deals with the formulation of matrix mechanics by Max Born, Werner Heisenberg and Pascual Jordan in fall 1925, and the immediate applications of this scheme to treat atomic problems (Wolfgang Pauli's triumphant treatment of the hydrogen atom, fall 1925; Heisenberg and Jordan's treatment of the anomalous Zeeman effects by including electron spin, early in 1926; Heisenberg's treatment of the many-body problem and resonance in quantum mechanics, spring 1926). Also treated are the modifications of the matrix scheme by Cornelius (Kornel) Lanczos ('field-like' representation of quantum mechanics) and Max Born and Norbert Wiener's formulation of operator mechanics, both in fall 1925. Matrix mechanics and its modifications, as well as Paul Dirac's work on the fundamental equations of quantum mechanics and the algebra of q-numbers preceded Erwin Schrödinger's formulation of wave mechanics.

I had the great privilege of discussing these developments with all the participants involved. Helmut Rechenberg and I have made use of these discussions to weave the human context in the narrative of the physical and mathematical framework of the discoveries in question. We have also drawn upon the rich source materials of the *Archives for the History of Quantum Physics*, especially the interviews with Max Born, Werner Heisenberg and Pascual Jordan (cited here as 'AHQP Interviews').* In addition, we have made use of the scientific correspondence of the principal physicists in question.

I am grateful to Aage Bohr for allowing me complete access to the archives held at the Niels Bohr Institute in Copenhagen. I am indebted to the architects of quantum mechanics and the heirs of their literary and scientific estates for permission to use the source materials pertaining to them.

JAGDISH MEHRA

*An inventory of these sources is contained in: T. S. Kuhn, J. L. Heilbron, P. Forman and L. Allen, *Sources for History of Quantum Physics*, The American Philosophical Society, Philadelphia, 1967.

Introduction

On 30 August 1925 Niels Bohr delivered an address on 'Atomic Theory and Mechanics' to the sixth Scandinavian Mathematical Congress in Copenhagen. In his remarks on 'an attempt at a rational quantum mechanics,' which he appended to the original address in November 1925 before publication, Bohr said:

> It will interest mathematical circles that the mathematical instruments created by the higher algebra play an essential part in the rational formulation of the new quantum mechanics. Thus the general proofs of the conservation theorems in Heisenberg's theory carried out by Born and Jordan are based on the use of the theory of matrices, which go back to Cayley and were developed by Hermite. It is to be hoped that a new era of mutual stimulation of mechanics and mathematics has commenced. To the physicist it will seem first deplorable that in atomic problems we have apparently met with such a limitation of our usual means of visualisation. This regret will, however, have to give way to thankfulness that mathematics, in this field too, presents us with the tools to prepare the way for futher progress. (Bohr, 1925b, p. 852)[1]

With these remarks Bohr addressed himself to the latest status of atomic theory: the difficulties arising in the usual mechanical interpretation, as well as the progress achieved by Heisenberg's discovery of the quantum-mechanical scheme and its subsequent mathematical formulation by Born and Jordan. In the latter development Bohr saw a revival of the long-established connection between mathematics and physical theory. Since the days of Isaac Newton, mechanics had always been regarded as a part of mathematics. However, in the twentieth century, with the perfection attained by classical mechanics in Henri Poincaré's *Les Méthodes Nouvelles de la Mécanique Céleste* (1892, 1893, 1899), on the one hand, and Einstein's special and general relativity theories (in which, again, the mathematicians like Poincaré, Hermann Minkowski, Marcel Grossmann, David Hilbert, Felix Klein, Curbastro Gregorio Ricci and Tullio Levi-Civita played an important role), on the other, one began to believe that mathematics and physics—of which the Newtonian and relativity mechanics were a completed segment—had no further overlap. Bohr's reminder to the

[1]On 25 November 1925 Bohr wrote to Pauli that, induced by a previous letter of Pauli (Pauli to Bohr, 17 November 1925), he had made a series of changes in his Copenhagen address of August 1925, taking into account the recent progress in quantum mechanics initiated by Heisenberg.

mathematicians, that the unsolved problem of atomic theory would still require the use of appropriate mathematical tools, was therefore timely. As proof, he cited the fact that 'a self-contained theory sufficiently analogous to classical mechanics' had been formulated by Max Born and Pascual Jordan on the basis of 'Heisenberg's quantum mechanics,' and that this new atomic mechanics employed methods which had been developed in higher algebra (Bohr, 1925b, p. 852).

The quick mathematical formulation of the physical ideas, which Heisenberg had proposed in his paper on the quantum-theoretical reformulation of kinematic and mechanical relations, in a consistent theory came as a great surprise to most physicists who had laboured for years on the problems of atomic theory. A wider dissemination of matrix mechanics was limited, however, by the fact that its mathematical methods could not be handled easily for the purpose of calculating the properties of atomic systems. An important breakthrough was achieved by Wolfgang Pauli, who calculated the energy terms of the hydrogen atom in late October 1925. But it was necessary to extend Born and Jordan's original matrix scheme to deal with atoms containing more than one electron. Several authors independently provided such extensions: Cornelius Lanczos of Frankfurt University and Max Born and Norbert Wiener, then at the Massachusetts Institute of Technology. While Lanczos' proposal concerning a field-theoretical formulation of quantum mechanics became fruitful only later on in the wave mechanical theory, which Erwin Schrödinger discovered independently, the operator mechanics of Born and Wiener helped Heisenberg, Pauli and Gregor Wentzel to generalize matrix mechanics into a symbolic calculus, which was also applicable to the action and angle variables. The observed properties of complex atoms could still not be accounted for until two further features were introduced into quantum mechanics: first, the electron spin, discovered by George Uhlenbeck and Samuel Goudsmit in the analysis of multiplet spectra (Uhlenbeck and Goudsmit, 1925); second, the conception of exchange degeneracy (Heisenberg, 1926b). By incorporating these extensions of the matrix scheme, Heisenberg and Jordan successfully calculated the anomalous Zeeman splittings of complex spectra (Heisenberg and Jordan, 1926), and Heisenberg arrived at a qualitative understanding of the energy states of the helium atom (Heisenberg, 1926c). Thus, within a period of less than ten months after the submission of Heisenberg's first paper (1925c), quantum mechanics developed into a theory which provided a satisfactory explanation of many important problems of atomic physics that had concerned physicists for a long time.

Chapter I
The Rediscovery of a Mathematical Tool

Heisenberg's observation that the products of the quantum-theoretical analogues of the classical Fourier series, which were supposed to describe the dynamical variables of atomic systems, did not necessarily commute, led to an obvious conclusion: the calculus used in classical mechanics would not serve as an appropriate tool in atomic theory, and different mathematical methods had to be applied. Max Born discovered that infinite matrices could be used to formulate quantum-mechanical relations consistently. The operations with such matrices had been developed by the mathematicians during the second half of the nineteenth century and brought to some perfection in the theory of linear integral equations by Born's teacher David Hilbert in Göttingen. Born had not only witnessed Hilbert's work on that subject, but had himself applied the methods of infinite matrices to physical problems, as for example to the problem of specific heat of solids; hence he was well acquainted with the details of the matrix calculus. In Pascual Jordan he found an able collaborator for the task of formulating matrix mechanics.

I.1 Max Born's Interpretation of Heisenberg's Quantum Condition

During the summer semester of 1925, Max Born, Professor of Theoretical Physics at the University of Göttingen, had been especially busy. In a long letter to his friend Albert Einstein in Berlin, dated 15 July 1925, he gave an account of the events that had taken place. A number of physicists had visited Göttingen: Paul Ehrenfest from Leyden, who lectured on Bose–Einstein statistics and gas degeneracy, Hendrik Kramers from Copenhagen, Peter Kapitza from Cambridge, and Philipp Frank from Prague. 'For us,' Born wrote to Einstein, 'this is very stimulating, but it is often too much for our wives. So they simply run away; my wife and Mrs. Courant have already left, and Mrs. [Philipp] Frank is due to leave in two days' time.'[2] Then he continued: 'But do not conclude from that that your visit is going to be unwelcome! We are greatly looking forward to it! But it ought to be at a quieter time. In July, most of the foreigners are already on holiday,

[2] Hedwig Born had left Göttingen on 13 July 1925, accompanied by the children, to go to Silvaplana in the Engadine, Switzerland. Max Born joined her soon after the end of the semester, i.e., early in August 1925.

and they descend on us in droves. But you know all this business. There is going to be another rumpus tomorrow; it is the inauguration of [Ludwig] Prandtl's new hydrodynamic institute, with a guided tour, official dinner and gala concert. It will cost me almost an entire working day' (Born to Einstein, 15 July 1925).

Besides being occupied with receiving visitors and other social activities, Born was deeply interested in the problems of quantum theory. For example, he reported to Einstein about the exciting discussions on Louis de Broglie's matter waves that had been taking place in Göttingen at that time, which finally led Walter Elsasser to his explanation of the Ramsauer effect (Elsasser, 1925). He continued: 'But my principal interest is the rather mysterious difference calculus on which the quantum theory of atomic structure is based. Jordan and I are systematically (though with a minimum of mental effort) examining every imaginable correspondence relationship between classical, multiple-periodic systems and quantum atoms' (Born to Einstein, 15 July 1925). Indeed, Born hoped that the reformulation of the equations of atomic theory as difference equations would lead to a breakthrough in the description of atomic phenomena, for his previous investigations had pointed in that direction.

Upon concluding that all the advanced methods of celestial mechanics had failed in the helium problem (Born and Heisenberg, 1923b), Born had decided that new mathematical tools, completely different from those provided by the differential calculus and applied so successfully to the problem of physics from Newton to Poincaré, had to be developed for atomic mechanics. The calculus of differences seemed to present a natural method to account for the most important result connecting the frequencies and energies of atomic systems; in this case, the classical relation

$$\nu_\tau^{\text{cl}} = \tau \frac{\partial W}{\partial J}, \tag{1}$$

giving ν_τ^{cl}, the τth harmonic frequency, as a derivative of W, the energy, with respect to the action integral J, could be replaced by the difference equation

$$\nu^{\text{qu}}(n, n-\tau) = \frac{1}{h}\left[W(n) - W(n-\tau) \right]. \tag{2}$$

Born had then concluded that it was necessary to reformulate all the differential formulae of classical multiply periodic systems as difference equations, and had started to search for appropriate examples (Born, 1924b). Although the mathematical method, the calculus of differences, was available to a certain extent (see, e.g., Nörlund, 1924), Born made only slow progress in his programme of 'discretization' of classical mechanical relations. The main difficulty was that it was not altogether clear as to which of these relations should be reformulated at all as difference equations. However, encouraged by the successful procedure employed in dispersion theory (Kramers, 1924a, b; Born, 1924b), Born had arrived eventually at an important prescription concerning the reformulation of mechanical equations in quantum theory. In a paper, submitted together with Pascual Jordan in June 1925, he had proposed to deal only with those classical equations in which all quantities correspond to observable properties of atomic systems (see Born and Jordan, 1925a, especially p. 493). Born referred to that paper in his letter to Einstein with the following words: 'A paper on this subject [i.e., on the application of the difference calculus in atomic theory] in which we examine the effect of non-periodic [electromagnetic] fields on atoms will appear soon. This is a preliminary study for an investigation of the processes occurring in atomic collisions (quenching of fluorescence, sensitized fluorescence *à la* Franck, etc.); one can understand, I think, the essential characteristics of what goes on. The different behaviour of atoms depends mainly on whether they have an (average) dipole moment, or just a quadrupole moment, or even a still higher electric symmetry' (Born to Einstein, 15 July 1925). Born hoped that the long march towards the unknown quantum laws would include as steps his discretization procedure and the use of observable quantities.

Born had barely sent off his letter to Einstein when his collaborator Werner Heisenberg came to see him. He had in the meanwhile 'pursued some work of his own, keeping its idea and purpose somewhat dark and mysterious,' as Born recalled later (Born, 1978, p. 216). During the summer semester of 1925 Born had seen less of Heisenberg than in previous times. The two of them had not worked on common projects; actually Born had been absorbed in his official duties and in his collaboration with Jordan on the application of dispersion-theoretic methods to aperiodic atomic systems, while Heisenberg had devoted his efforts to the general questions arising from the problem of calculating the intensities of hydrogen lines, a problem which was not in the centre of Born's interests. In May and early June 1925, before he went to Helgoland to recover from hay fever, Heisenberg had not discussed his work with Born. As he recalled later: 'At that time everything was so vague. We may, of course, have talked in a general manner about what one could possibly do, but the only time when I really had discussions about it was when he gave me this book on the Bessel functions' (Heisenberg, Conversations, p. 269). These discussions had taken place already earlier in May, when Heisenberg had begun by guessing the intensities of the Balmer lines and Born had expected that Bessel functions would play a role in solving that problem. After returning from Helgoland, Heisenberg had communicated exclusively with Wolfgang Pauli by letters; Pauli had thus been the only person who was familiar with the difficulties and successes of the quantum-mechanical scheme prior to the completion of the paper (Heisenberg, 1925c). Heisenberg had even sent the manuscript first to Hamburg, requesting Pauli's opinion before showing it to anybody in Göttingen. Upon receiving a favourable response from Pauli, he had then completed the paper and gone to see Born.

Born recalled the circumstances of receiving Heisenberg's paper as follows: 'He came to me with a manuscript and asked me to read it and to decide whether it was worth publishing. At the same time he asked me for leave of absence for the rest of the term (which ended about [1] August [1925]), as he had an invitation to lecture at the Cavendish Laboratory in Cambridge. He added that though he had tried hard, he could not make any progress beyond the simple considerations contained in his paper, and he asked me to try myself, which I promised to do' (Born, 1978, p. 216). In spite of the fact that he 'felt that something real was in the paper,' Heisenberg was 'very uncertain about it' (Heisenberg, Conversations, p. 269). Hence he left the question of whether or not the paper should be submitted to *Zeitschrift für Physik* to Max Born, and went away from Göttingen without waiting for the final decision.[3]

Born received Heisenberg's paper on the quantum-theoretical re-interpretation of kinematic and mechanical relations around the middle of July, the time when he wrote the letter to Einstein.[4] Because of his preoccupation with other events—such as the inauguration of Prandtl's hydrodynamic institute—he delayed studying the new work. Many years later he said: 'I remember that I did not read this manuscript at once because I was tired after the term and afraid of hard thinking. But when, after a few days, I read it I was fascinated' (Born, 1978, p. 216).[5] He began to think especially about the meaning of the strange rule, which Heisenberg had given for the multiplication of two quantum-mechanical transi-

[3] According to the recollection of Friedrich Hund (private communication), Heisenberg gave an informal seminar on his new quantum-mechanical scheme to some friends in Göttingen after the completion of his paper (Heisenberg, 1925c). Besides Hund, these people included (most probably) Pascual Jordan, Hertha Sponer and Wilhelm Hanle.

[4] Heisenberg sent his manuscript to Pauli with a letter, dated 9 July 1925; this was on a Thursday. He could scarcely have received Pauli's reply before Monday, 13 July; Born, therefore, got the paper a couple of days later.

[5] In Born's letter to Einstein, dated 15 July 1925, there occurred the sentence: 'Heisenberg's latest paper, soon to be published, appears rather mystifying but is certainly true and profound.' In his commentary on *The Born–Einstein Letters*, Born interpreted this sentence as follows:

> Then comes the most important matter: a few lines about Heisenberg's new paper, which must have appeared "mystifying" but nevertheless true. This must have been the treatise in which he formulates the basic concepts of quantum mechanics and explains them by using simple examples. As my recollection of this time, which marked the beginning of a revolution in physical thinking, is a little hazy, I wrote to Professor van der Waerden, who confirmed my assumption. His book [van der Waerden, 1967] will enable the reader to look up the sequence of events in complete detail. (Einstein and Born, 1971, p. 87)

Van der Waerden, in his introduction to the collection of papers, *Sources of Quantum Mechanics*, reconstructed the events as follows: 'On 11 or 12 July Heisenberg gave Born the final version of his paper, asking him to decide whether it was worth publishing . . . . Born must have studied Heisenberg's manuscript before July 15, for on July 15 he writes in a letter to Einstein: ' . . . *Heisenberg's neue Arbeit die bald erscheint, sieht sehr mystisch aus, ist aber sicher richtig und tief* . . . ' (van der Waerden, 1967, p. 36).

It is easy to see that this conclusion is wrong because the sentence on Heisenberg's new paper in Born's letter to Einstein (15 July 1925) continues as follows: 'It enabled Hund to bring into order the whole of the periodic system [of elements] with all its complicated multiplets.' Born also indicated that Hund's paper on the interpretation of complex spectra (Hund, 1925c), which was built on Heisenberg's paper, would appear soon; this was indeed the case as Hund's paper was published in August. In that paper, however, Hund referred only to Heisenberg's paper on complex multiplets, which he had submitted from Copenhagen in April 1925 and which appeared in July 1925 (Heisenberg, 1925b). Hence it is clear that Born in his letter to Einstein did not refer to Heisenberg's paper on the new quantum-mechanical scheme; moreover, he had not yet even studied it, for otherwise he would have mentioned to Einstein that it was even more 'mystical' than the paper on multiplets.

tion amplitudes, namely, the equation

$$C(n,n-\beta) = \sum_{\alpha=-\infty}^{+\infty} A(n,n-\alpha)B(n-\alpha,n-\beta), \tag{3}$$

in which the product amplitude $C(n,n-\beta)$ was expressed as a sum of products of amplitudes $A(n,n-\alpha)$ and $B(n-\alpha,n-\beta)$. This multiplication rule, which was different from the usual multiplication of two Fourier series describing the variables of classical periodic systems, seemed to provide the key to Heisenberg's quantum-mechanical scheme; hence it was necessary to understand its significance. 'I began to ponder over his symbolic multiplication,' Born recalled, 'and was soon so involved in it that I thought about it the whole day and could hardly sleep at night. For I felt there was something fundamental behind it, the consummation of our endeavours of many years. And one morning, about the 10 July 1925, I suddenly saw light: Heisenberg's symbolic multiplication was nothing but the matrix calculus, well known to me since my student days from the lectures of [Jakob] Rosanes in Breslau' (Born, 1978, p. 217).[6]

According to Pascual Jordan's recollection Born had himself thought about the mathematical process of multiplication in quantum theory even before seeing Heisenberg's paper. Thus he reminded Born: 'By the way, I recall with certainty that, following our joint work on absorption [Born and Jordan, 1925a], you considered a symbolic multiplication of quantum-theoretical "transition amplitudes" yourself; at that time we used to discuss together almost every day, and you told me about it. Only, at that time, we did not see clearly what purpose this multiplication should serve; that one could use it to establish analogous equations of motion, we had not seen immediately' (Jordan to Born, 3 July 1948). However, exactly this recognition had provided the starting point for Heisenberg's general considerations in May 1925, which had also led to the noncommutative multiplication rule, Eq. (3). Although Heisenberg had thus proceeded faster and arrived at a definite conclusion, Born was familiar with the problem and prepared to grasp clearly the importance of the result.

It seems to be remarkable that Born, at the moment of recognizing the meaning of Heisenberg's multiplication rule, thought so far back as the lectures on linear algebra, which he had attended in the beginning of the century at the University of Breslau. However, the fact that two matrices do not commute like ordinary numbers—which is mentioned in all elementary lectures on matrix

[6]As we have discussed above, the idea of identifying the multiplication rule, Eq. (3), with the known matrix multiplication, must have occurred to Born after 15 July.

Born studied in Breslau from 1901 to 1904, except the summer semesters 1902 (Heidelberg) and 1903 (Zurich). He attended Rosanes' lectures on algebra probably in his first semester. Rosanes lectured on '*Elemente der Determinantentheorie*' ('Elements of the Theory of Determinants,' summer semester 1901), '*Analytische Geometrie des Raumes*' ('Analytical Geometry of [Three-Dimensional] Space,' winter semester 1901–1902), and *Einführung in die Theorie der Invarianten* ('Introduction to the Theory of Invariants,' winter semester 1902–1903). (See *Vorlesungsverzeichnis*, *Phys. Zs.* **2**, 1900–1901, p. 395, p. 728; **3**, 1901–1902, p. 590.)

calculus—leaves a definite impression when one learns it for the first time.[7] In order to connect Eq. (3) with the familiar notation used in matrix algebra, Born had merely to rewrite it as

$$C_{nm} = \sum_{n'=-\infty}^{+\infty} A_{nn'} B_{n'm}, \tag{4}$$

where the two numbers on which the transition amplitudes depend are written as subscripts with $n' = n - \alpha$ and $m = n - \beta$. Equation (4) thus denoted the product of two quadratic matrices, A and B, whose discrete indices take on infinitely many values.

Born discovered the first example of the occurrence of two noncommuting matrices in Heisenberg's quantum condition (Heisenberg, 1925c, p. 886, Eq. (16)),

$$h = 4\pi m \sum_{\alpha=0}^{\infty} \left\{ |a(n, n+\alpha)|^2 \omega(n+\alpha, n) - |a(n, n-\alpha)|^2 \omega(n, n-\alpha) \right\}. \tag{5}$$

By expressing Heisenberg's q, the position coordinate of the periodic system under investigation, as

$$q_{nn'} = a(n, n') \exp\left[i\omega(n, n')t \right], \tag{6}$$

and the conjugate momentum $p_{nn'}$ by

$$p_{nn'} = m \frac{dq_{nn'}}{dt}, \tag{7}$$

where m is the moving mass of the system, Born reformulated the quantum condition (5) as the equation[8]

$$\frac{h}{2\pi i} = \sum_{n'=-\infty}^{+\infty} (p_{nn'} q_{n'n} - q_{nn'} p_{n'n}). \tag{8}$$

'I recognized at once its formal significance,' he recalled many years later. 'It meant that the two matrix products pq and qp are not identical. I was familiar

[7] Later, Born had to deal with matrices often, as for example in his continuation of Hermann Minkowski's work on electrodynamics (Born, 1909b). The noncommutativity property of matrices is such an elementary property that it is often not even mentioned in the more sophisticated applications of matrix calculus (see, e.g., Hermann Weyl's discussion of tensor algebra in *Raum-Zeit-Materie*, Chapter I, §§5–8, Weyl, 1918c). However, it was referred to in Richard Courant's book *Methoden der mathematischen Physik* (Courant–Hilbert, 1924, Chapter One, Section 3).

[8] Evidently, one can express the right-hand side of Eq. (5) as 2π times the factor $m\sum_{\alpha=-\infty}^{+\infty}\{|a(n, n+\alpha)|^2\omega(n+\alpha, n) - |a(n, n-\alpha)|^2\omega(n, n-\alpha)\}$, or with the help of Eqs. (6) and (7), as 2π times the factor $(1/i)\sum_{n'}(q_{nn'}p_{n'n} - p_{nn'}q_{n'n})$.

with the fact that matrix multiplication is not commutative; therefore I was not too much puzzled by this result' (Born, 1978, p. 217).

It was at about this stage that Born, full of excitement with his discovery about what Heisenberg's quantum rule of multiplication signified mathematically, travelled to Hanover on Sunday, 19 July 1925, to attend the meeting of the *Gauverein Niedersachsen* (Lower Saxony Section) of the German Physical Society.[9] He recalled afterwards: 'A considerable number of physicists from Göttingen went there by train; it is about an hour's journey on the North Express. In the train we met physicists from other universities, among them my former assistant Pauli' (Born, 1978, p. 218). Born happily 'joined him [Pauli] in his compartment' and 'at once told him about matrices.'[10] Pauli, who had been Heisenberg's confidant while his new ideas had taken shape, who knew all about his work and had approved it only a week ago, did not respond favourably to Born's excitement about matrices. When Born asked Pauli whether he would care to collaborate with him on the further development of Heisenberg's ideas, he gave a 'cold and sarcastic refusal.' 'Yes, I know,' Pauli retorted, 'you are fond of tedious and complicated formalisms. You are only going to spoil Heisenberg's physical ideas by your futile mathematics' (Born, 1978, p. 218).

Born's approach to Pauli and the latter's response were typical of the attitudes of both men, who had been the first to learn about the new quantum-mechanical scheme. Born was happy when he discovered the fact that Heisenberg's multiplication rule, Eq. (3), was none other than matrix multiplication, an operation well known to mathematicians in the work of Arthur Cayley since the 1850s, for he could now exploit the entire formalism of matrix methods more effectively than he had been able to do, e.g., in his work on electron theory sixteen years previously (Born, 1909b). It occurred to Born that the well-established methods of matrix algebra would probably offer a far better tool in discovering the real quantum laws of atomic mechanics than the calculus of differences, with which he had been stuck for almost two years. Quickly abandoned now was Born's own correspondence principle, i.e., the formulation of quantum laws by transforming

[9] The record of this meeting (*Verh. d. Deutsch. Phys. Ges. (3)* **6**, 1925, pp. 36–39) states that Born discussed there his recent work on aperiodic motions (Born and Jordan, 1925a) and Hund his interpretation of complex spectra (Hund, 1925c).

[10] At this particular point Born's memory was not accurate. As he recalled: 'Pauli had meanwhile become famous from many excellent papers, among them that on his celebrated exclusion principle, on which Niels Bohr had built his theory of the periodic system of elements. He was coming from Zurich (where the summer vacation started earlier than in Germany) to take part in our meeting in Hanover' (Born, *Recollections*, 1978, p. 218).

Pauli actually went to Hanover from Hamburg; it was almost three years before he moved to his professorship in Zurich (spring 1928). He did not give a talk at Hanover himself; however, he was writing his article on quantum theory for the *Handbuch der Physik* and was interested in discussing the latest status of spectroscopic theory with such experts as Ralph Kronig, Wilhelm Hanle and Friedrich Hund, who reported about their work at Hanover. As far as the meeting of Pauli and Born is concerned, Pauli certainly did not join Born in Göttingen to go to Hanover; however, the two might have had occasion to talk privately at the railway station after the meeting, before leaving Hanover in opposite directions to Hamburg and Göttingen, respectively.

differential equations into difference equations, and there appeared to him the vision of a new territory to be explored by means of an obviously erudite, fully developed, and general mathematical apparatus. Moreover, Born believed that he could make sense of Heisenberg's new ideas, and thereby respond to the latter's appeal which had been expressed to Pauli: 'But perhaps people, who know more, may be able to do something reasonable with them' (Heisenberg to Pauli, 9 July 1925).

Wolfgang Pauli, however, did not share Born's enthusiasm. He had in fact been glad that 'Heisenberg has learned a little philosophy from Bohr in Copenhagen, and does indeed turn away noticeably from the purely formalistic approach' (Pauli to Kramers, 27 July 1925). And now Born talked about a well-defined mathematical scheme, whereas he, Pauli, only saw in Heisenberg's work the first tender green shoots in the barren terrain of the difficulties of atomic physics. No, Pauli was not the right collaborator to work with Born in applying matrix methods to Heisenberg's scheme. Moreover, he was convinced that the product of the physical quantities, p and q, had to commute, and Born, in his opinion, just could not be on the right track.[11] In any case, Pauli did not wish to interfere with Heisenberg's thoughts and the manner in which he would develop his theory. As Jordan reported later: 'To Miss Mensing . . . he had mentioned at that time that he wanted to leave the topic to Heisenberg himself for the moment' (Jordan to Born, 3 July 1948).

Since Pauli had given him a negative answer concerning a collaboration on quantum mechanics, Born turned to Pascual Jordan, his main helper in those days, because he was 'extremely tired and felt unable to make progress alone' (Born, 1978, p. 218).[12] Jordan was glad to assist his professor again and, within a short time, achieved important progress in the matrix formulation of Heisenberg's ideas. Soon Born was able to spread the news about the new theory outside Göttingen. On the way to spend his vacation in Switzerland, he delivered a lecture in Tübingen on 30 July 1925. Alfred Landé still remembered Born's visit four decades later: 'I first heard of matrix mechanics when Born was in Tübingen . . . . He told me something about it; that they [i.e., the Göttingen theoreticians] had a completely new approach to quantum mechanics, and everything [was] dominated by multiplication, [in which] A times B differs from BA. I did not understand a single word about it, and I don't think that Born and the whole group in Göttingen "understood" much more than the mere formulas' (Landé, AHQP Interview, 3 July 1962, p. 21). However, the mathematical methods of matrices enabled Born and Jordan to formulate the first consistent theory of quantum mechanics within less than two months.

[11] Pascual Jordan, who had not witnessed Born and Pauli's discussion in Hanover, but heard about it immediately afterwards from Born, recalled: 'Pauli had then told you that one had to assume $pq = qp$' (Jordan to Born, 3 July 1948).

[12] One may wonder why Born did not think of Jordan right away. He probably believed that the task of formulating Heisenberg's ideas was very difficult and that Jordan might not be experienced enough.