Erwin Schrödinger and the Rise of Wave Mechanics. II. The Creation of Wave Mechanics¹

Jagdish Mehra^{2,3}

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This article (Part II) deals with the creation of the theory of wave mechanics by Erwin Schrödinger in Zurich during the early months of 1926; he laid the foundations of this theory in his first two communications to Annalen der Physik. The background of Schrödinger's work on, and his actual creation of, wave mechanics are analyzed.

9. SCIENTIFIC EXCHANGE WITH PLANCK AND EINSTEIN

It has occasionally been remarked that it was the congenial Zurich atmosphere which stimulated the scientific creativity of Erwin Schrödinger, after those tiring "*Wanderjahre*" in Jena, Stuttgart, and Breslau.⁽²⁰³⁾ Schrödinger himself confirmed this opinion when he stated: "Here [in Zurich] I enjoyed the contact, the friendship, and the help of Hermann Weyl, Peter Debye, and others; in addition, this small city serves as a favorable stopover on the trip to Switzerland and to the South, and represents a genuine transfer point for acquaintances and the exchange of ideas with colleagues from near and far."⁽²⁰⁴⁾ In his early Zurich years Schrödinger did not seek the advice of his colleagues at the University or at

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² Instituts Internationaux de Physique et de Chimie (Solvay), ULB Campus Plaine, Code Postal 231, 1050 Brussels, Belgium.

³ Address in the U.S.A.: 7830 Candle Lane, Houston, Texas 77071.

the E.T.H. on scientific problems. He worked completely by himself on questions of his own choice, and for the necessary ideas of others he checked their papers. He was not, at least in the new and foreign surroundings, an outgoing person; it took him time to make friends in Switzerland. In addition, he really struggled with the requirements of the university professorship, with both the teaching load and the prescribed topics. Finally, he suffered from unstable health—some lung trouble—especially at the beginning of the Zurich period. During 1922 Schrödinger spent four months in Arosa to recover his health. In spite of the stable professional situation in Zurich, Schrödinger quite regularly returned to Arosa for vacations or cures. The Schrödingers loved the resort in the mountains; Erwin liked mountaineering, although he never took difficult and strenuous tours. They also spent several months in the summer of 1925 and around Christmas of the same year in Arosa, and it was there that he began to work seriously on the first ideas toward wave mechanics.

Schrödinger's reputation grew slowly but steadily in Zurich and the scientific world. He was not ignored. He was invited, for example, to attend the fourth Solvay Conference on Physics, which took place from April 24-29, 1924 in Brussels. There were perhaps two main reasons for this invitation. First, the topic of the conference was the electrical conductivity of metals, and Schrödinger had just recently, in his paper on gas degeneracy and mean free path,⁽¹⁸¹⁾ made an interesting contribution to this field. Second, and perhaps even more important, the Scientific Council of the Conference availed itself of the opportunity to invite an Austrian physicist from Switzerland. Such a move was necessary because, after World War I, participants from Germany and Austria had been excluded from most international scientific conferences, especially from those occurring in Belgium, which had been invaded by German troops in 1914 and which was not prepared in 1924 to lift the ban. Albert Einstein was invited in his personal capacity, but did not attend. Schrödinger actively participated in the discussions at the conference, especially after the reports of Percy W. Bridgman, Owen Willans Richardson, W. Rosenhain, and Abraham Joffé. The conference itself was not very successful, because the problem of the conductivity of metals was not ripe for a solution in 1924; the appropriate methods for a suitable description of the electrical properties of conductors would become available only after the creation of quantum mechanics in 1925-1926.

The second conference of 1924, in which Schrödinger participated, was the 88th *Naturforscherversammlung*, which took place in Innsbruck from September 21–27. The 88th *Naturforscherversammlung* was the first after World War I that took place outside Germany, and the Austrians made a great effort to ensure its success. Participants came from far and wide, and there were many lectures on topics of quantum and atomic theory. Schrödinger, however, did not read a paper, but listened with interest to many of the talks presented at the Innsbruck meeting. For Schrödinger's current research program on quantum statistics, Max Planck's lecture provided considerable immediate interest. In it, Planck discussed the appropriate definition of the sum of the states ("Zustandssumme") Z in the case of a gas consisting of hydrogen atoms.⁽²⁰⁵⁾ Schrödinger carefully followed Planck's reasoning. In the winter of 1924/1925 Planck treated the quantum theory of ideal gases, especially the definition of its entropy in a series of lectures at the University of Munich. In February 1925 he also presented a paper, entitled "On the Problem of the Quantization of Monatomic Gases," to the Prussian Academy.⁽²⁰⁶⁾ Schrödinger then returned to the problem of like particles and responded to Planck in the summer of 1925.⁽¹⁹¹⁾ Although Albert Einstein did not deliver a lecture at the Innsbruck Naturforscherversammlung, his presence in the Tyrolean capital constituted one of the highlights of the meeting.

In the summer of 1924 Einstein had further reason to renew his strong stand in favor of the light-quantum hypothesis. He had just seen Satyendra Nath Bose's paper providing a derivation of Planck's radiation law on the basis of a new statistics for light quanta⁽¹⁹²⁾; it was a derivation which satisfied all of Einstein's demands with respect to consistency. Einstein was immediately able to generalize Bose's quantum statistical method in developing a quantum theory of ideal gases, in which some of the difficulties persisting in previous approaches could be removed.⁽¹⁹³⁾ In the months following the Innsbruck meeting he continued to work on that theory.^(194,195)

From the encounter at Innsbruck with Einstein and Planck, Schrödinger might have taken away certain hints for his future work on quantum statistics and gas theory: for example, the impression of Planck's stubborn insistence on the division of the probability for the state of a gas by the factorial of the number of identical particles, or the possibility of degeneracy occurring in Einstein's recent theory. Towards the end of the winter semester he began again to study the recent literature on quantum statistics and commenced his exchange of correspondence with Einstein and Planck. Schrödinger wrote to Einstein⁽²⁰⁷⁾ about the difficulties he had encountered in the latter's paper,⁽¹⁹³⁾ and Einstein explained to him that he was not using the Maxwell–Boltzmann statistics, but the *Bose statistics* in which "the quanta or molecules are not considered as being *mutually independent* objects."⁽²⁰⁸⁾

After communicating Schrödinger's paper on the statistical entropy definition of an ideal gas⁽¹⁹¹⁾ (discussed in Section 8 and mentioned here) to the Prussian Academy on July 23, 1925, Planck proposed a new general

definition of the entropy, which did not-as he pointed out-agree with any of the four definitions of entropy mentioned in Schrödinger's paper, namely

$$S = k \ln P \tag{49}$$

where P denoted the number of different stationary states which the system was able to adopt for a given total energy. Planck argued in favor of this definition as follows:

Its main advantage consists in that it avoids in principle all probability considerations and the arbitrariness connected with them, by reducing the problem of calculating the entropy to another independent physical problem, namely, the problem of quantizing the processes occurring in the system under consideration. It [the definition] differs from all other previous definitions in that it provides the entropy directly and not through the study of the individual parts (molecules) comprising the system; the latter procedure might easily imply the danger that the interactions of the parts are not properly taken into consideration.⁽²⁰⁹⁾

In closing his presentation⁽²⁰⁹⁾ to the Prussian Academy of Sciences, Planck mentioned that he planned to give some details of the points he had just touched upon in a later publication. The promised paper, entitled "A New Statistical Definition of Entropy," was indeed received on October 30, 1925 by the *Zeitschrift für Physik* and appeared in print before the end of the year.⁽²¹⁰⁾ It contained not only an extended application of the entropy definition, Eq. (49), but also revealed a change in Planck's attitude toward the quantum-theoretical definition of the entropy: while he had formerly considered all his attempts to be "a suitable further development of the Boltzmann–Gibbs concepts, caused by the quantum hypothesis," he now thought it more appropriate to speak of "a new definition."⁽²¹¹⁾ The main advantage of this new definition was, so Planck claimed, that it embraced both the old Boltzmann statistics and the statistics recently proposed by Einstein.

In the new paper of October 1925 Planck assumed several points of view, which closely approached those of Schrödinger. Schrödinger had, for example, emphasized in his paper, sent to the Prussian Academy and communicated by Planck, the necessity of a "radical departure from the Boltzmann–Gibbs type of statistics"⁽²¹²⁾; he had also, when discussing the possibility of Planck's proposal to quantize a gas system as a whole, held "the difficulties to be so large as to render impossible a performance of this beautiful idea."⁽²¹²⁾ Within several months, however, he succeeded in removing the arbitrariness in a paper on "The Energy States of the Ideal Monatomic Gas," which was communicated by Einstein to the meeting of January 7, 1926 of the Prussian Academy of Sciences.⁽²¹³⁾ Although the results of Schrödinger's renewed approach to the quantum theory of ideal

gases did not confirm those of either Planck or Einstein—he somehow established a third theory, which was formally related more to Planck's than to Einstein's—Schrödinger did achieve definite progress in understanding the gas system. Within certain limitations—i.e., restriction to higher temperatures and not too high densities—Schrödinger showed how one could derive in a unique way [by applying Planck's recent definition of the entropy, Eq. (49)] the quantized energy states of a gas system *as a whole.* Schrödinger consequently rated Eq. (49) highly, calling it a "clear, beautiful crowning" ("*klare, Schöne Krönung*") of the endeavors to find a proper energy definition for gases.⁽²¹⁴⁾ While praising the generality and beauty of Planck's definition, Schrödinger's scientific temperament took the side of Einstein, who had always insisted on a very pragmatic, nonambiguous application of the concept of entropy.

Schrödinger thus held an intermediate position between his two Berlin colleagues. On the one hand, he favored the specific results from Einstein's or his own treatment of quantum gas theory, which he believed to have a good chance of describing the real behavior of ideal gases at low temperatures; on the other hand, he agreed with Planck in requiring a proof on the basis of general quantum theoretical considerations of Einstein's statistical methods. Such considerations would indeed soon be available to Schrödinger. However, in order to obtain the proof, he had to make essential use of an idea, to which neither Planck nor he had ever referred, namely the matter waves of Louis de Broglie from Paris, Einstein, through his relations with the Paris physicists (especially his friend Paul Langevin), had learned about the importance of the Broglie's work earlier than most other physicists; and he had quoted, in his second memoir on ideal gas theory, de Broglie's dissertation.⁽²¹⁵⁾ This reference now attracted Schrödinger's attention; and after he obtained the thesis in early November 1925 and had studied it carefully, he was prepared to apply it to gas theory. A month later he reported to Einstein: "I am concerned with some 'wagoner's work' on your 'undulatory theory of gases'."(216) For him the stimulus to use matter waves in gas theory had come from Einstein's Section 8 in the second memoir.⁽¹⁹⁴⁾

10. INITIAL STEPS TOWARD THE HYDROGEN EQUATION

A careful examination of Schrödinger's publications and unpublished notes, written during the vacation months between the summer of 1925 and the beginning of the winter semester of 1925/1926, has not yielded anything that could be considered as the starting point of the discovery of the hydrogen equation. Then, however, the situation changed quickly and drastically. On November 3, 1925 Schrödinger wrote in a letter to Albert Einstein: "A few days ago I read with the greatest interest the ingenious thesis of Louis de Broglie, which I finally got hold of. Because of it also Section 8 of your second degeneracy work has become completely clear to me for the first time." In that section Einstein had discussed the problem of fluctuations in his new theory and had obtained the law for the fluctuation of the number of molecules Δ_{y} ,

$$\overline{\left(\frac{A_v}{n_v}\right)^2} = \frac{1}{n_v} + \frac{1}{z_v}$$
(50)

where n_{ν} denoted the average number of molecules and z_{ν} the number of phase cells in the region between E_{ν} and $E_{\nu} + \Delta E_{\nu}$. Einstein had emphasized the analogy of Eq. (50) with the corresponding one for blackbody radiation density and stated with respect to the second term on the right-hand side: "It arises in the case of radiation from interference fluctuations. We may also interpret it for gases in a corresponding way by associating with the gas, in a suitable manner, a ray phenomenon and then computing the interference fluctuations of the latter."⁽²¹⁷⁾ Einstein had then explained what he meant in greater detail, believing "that one is dealing here with more than a formal analogy."⁽²¹⁷⁾

In doing so, Einstein had first referred to the "notable paper" of Louis de Broglie, quoting his thesis of 1924,⁽²¹⁸⁾ and had shown how the author had associated a scalar wave with any material particle of mass *m* and velocity *v*. That wave should have phase velocity *V*,

$$V = \frac{c^2}{v} \tag{51}$$

and frequency v,

$$v = \frac{v_0}{\sqrt{1 - v^2/c^2}}$$
(52)

with v_0 , the frequency at rest, given by the quantum-theoretical relation

$$v_0 = \frac{mc^2}{h} \tag{53}$$

Evidently, c represented the velocity of light *in vacuo* and h Planck's constant. Einstein had concluded: "One now observes that it is possible to associate a scalar wave field with such a gas, and I have convinced myself by a calculation that $1/z_v$ is the square fluctuation of this wave field."⁽²¹⁹⁾

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The reference in Einstein's paper to de Broglie's notable work had been noticed by Schrödinger, whose interest was increased by two points which Einstein had mentioned in this connection. The first was the observation that the idea of phase waves associated with gas molecules provided a solution of the so-called Gibbs paradox: evidently for two identical molecules (having the same mass) interference effects of the associated phase waves could occur, thus removing the mixing entropy; in the case of different masses the associated phase waves would have different phases and therefore not interfere.⁽²¹⁹⁾ For Schrödinger, who had pondered about the paradox several times before, this explanation spoke clearly in favor of de Broglie's idea. Second, a footnote in Einstein's paper had further increased his curiosity; it stated: "In this dissertation [of L. de Broglie] we also find a very remarkable geometrical interpretation of the Bohr-Sommerfeld quantum rule."⁽²¹⁷⁾ When Schrödinger finally held Louis de Broglie's thesis in his hands, he primarily checked it for more detailed information on exactly these two points.

In a note presented to the Paris Academy of Sciences already on September 10, 1923, Louis de Broglie had suggested not only the idea of a phase wave connected with material particles, but had also shown how one could use it to explain Bohr's quantum condition for the angular momentum p_{ϕ} ,

$$\int_{0}^{2\pi} p_{\phi} \, d\phi = nh \tag{54}$$

with integral n.⁽²²⁰⁾ He had repeated this derivation, and its generalization to include the motion of the nucleus in the hydrogen atom, in Chapters III and IV of his thesis.⁽²¹⁸⁾ In the simplest case, Bohr's quantum condition could be obtained in the following way. One assumed that the length of the circular electron orbit in a hydrogen atom, i.e., $l = 2\pi R$, is an integral multiple $n\lambda$ of the wavelength $\lambda (= V/v)$ of the phase wave, or

$$\oint \frac{v}{V} \, dl = n \tag{55}$$

By inserting Eqs. (51), (52)—in the nonrelativistic limit—and (53) into Eq. (55), one finds

$$\frac{m}{h} \oint v \, dl = \frac{2\pi Rmv}{h} = n \tag{56}$$

which is identical to the quantum condition (54) provided one observes that $p_{\phi} = mRv$.

Upon reading de Broglie's thesis Schrödinger wrote to Einstein: "The de Broglie interpretation of the quantum rules seems to me related in some ways to my note in Zeitschrift für Physik 12, 13 (1922), where a notable property of the Weyl 'gauge factor' $\exp(-\int \phi_i dx_i)$ along the quasi-period [of the atomic system] is shown. The mathematical situation is, as far as I can see, the same, only demonstrated by me in a much more formal, less elegant and not really general manner. Naturally, de Broglie's consideration within his grand theory is altogether of far greater value than my single statement which, at first, I did not know what to make of."⁽²²¹⁾

Schrödinger carefully studied those parts of the thesis which de Broglie had not hesitated to call "the most important consequence that one can draw from it," explaining: "After having recalled the laws of stability for the quantized trajectories, those resulting from numerous recent papers, we have shown that one can interpret them as expressing the resonance of the phase wave on the length of the closed or quasi-closed trajectory. We believe that this is the first explanation, which is physically plausible, proposed for these stability conditions of Bohr and Sommerfeld."(222) Schrödinger tended to agree with de Broglie that the result thus emphasized was notable, as he had called his own observation of 1922; he thought that it was most important, and he began to think of applying the de Broglie waves to further atomic problems than those so far considered by its originator. About two weeks after he had obtained the thesis, he wrote to Alfred Landé in Tübingen: "During the last few days I have been deeply involved in Louis de Broglie's ingenious Thèses. It is extraordinarily stimulating; however, certain points are still very hard to understand. I have tried in vain to visualize the phase wave of an electron on Kepler orbits. The 'rays' certainly correspond to neighboring Kepler ellipses having the same energy. This, however, leads to horrible 'caustics' or the like, for the wave front. On the other hand, the wave should have an extension in length determined by a Zeeman or Stark period."⁽²²³⁾ These remarks clearly reveal the direction in which Schrödinger proceeded first. Thus he tried to establish for himself a geometrical picture of the de Broglie waves in the more complicated cases of a hydrogen atom under the action of a static electric or magnetic field-the situations which he had also considered in his 1922 paper. Louis de Broglie had not dealt with these problems; the only geometrical construction he had presented in his thesis illustrated the two-center system consisting of the hydrogen nucleus and the (nonrelativistic) electron for which he had drawn, besides the orbit of the two particles, the rays of the two phase waves associated with them.⁽²²⁴⁾ While de Broglie considered the geometrical construction as an instructive exercise, Schrödinger had a deeper interest in it and wanted to explore the full range of consequences from the wave picture of electrons.

Besides the indication in the letter to Landé, we have no document available that throws light on how far Schrödinger succeeded and what he learned from his geometrical drawings. One may suspect that he did not get very far with it and, soon after writing to Landé, gave up this enterprise and turned to the more promising approach which made use of a wave equation for the phase wave. Although this particular step seems to be nothing more than a natural consequence of the idea of a phase wave, Louis de Broglie had not made any attempt in this direction. There were two reasons to explain de Broglie's failure⁽²²⁵⁾: First, de Broglie's mathematical education had been insufficient; he especially did not know the theory of differential equations and their eigenvalues, which were to play a crucial role in Schrödinger's procedure. Second, and more importantly, de Broglie wanted to establish a truly dualistic description of objects having wave and particle properties simultaneously; "he was not at all willing to give up the particle concept."(226) On the other hand, Schrödinger settled upon the wave description entirely, as one can see from his letter to Landé quoted above. The question then arises: How did Schrödinger come to think of a wave equation? Two contemporaries of Schrödinger in Zurich have provided statements related to this question.

The first testimony comes from Peter Debye, who recalled more than four decades later: "Then de Broglie published his paper. At that time Schrödinger was my successor at the University of Zurich, and I was at the Technical University, which is a Federal Institute, and we had a colloquium together. We were talking about de Broglie's theory and agreed that we did not understand it, and that we should really think about his formulations and what they mean. So I asked Schrödinger to give a colloquium. The preparation of that really got him started. There were only a few months between his talk and his publications."⁽²²⁷⁾ Debye recalled with certainty that it was the printed article of Louis de Broglie, ⁽²²²⁾ and not the thesis, ⁽²¹⁸⁾ which he had handed over to Schrödinger for the purpose of the colloquium talk.⁽²²⁸⁾ From these remarks it is clear that: First, Debye discussed with Schrödinger rather early in the winter semester of 1925/1926 the thesis work of Louis de Broglie; at that time neither he nor Schrödinger had fully grasped the ideas it contained. Second, he (Debye) had noticed the existence of de Broglie's published paper in Annales de Physiques and brought it to Schrödinger's attention. Third, Debye believed that it was the preparation of the Zurich colloquium talk which started Schrödinger's work on wave mechanics.

The last assertion cannot be taken too literally. We know from Schrödinger's letter to Einstein of November 3, 1925, and also from his letter to Landé on November 16, 1925, that he was delving into the details of applying matter waves to atomic problems which had not been considered in de Broglie's *Thèses*. It is unlikely that the colloquium talk was fixed before November 3, and Schrödinger did not even mention it on November 16. We would like to assume that Schrödinger presented the colloquium on de Broglie's work during the second half of November or the first half of December 1925. Such a date also follows from what was recalled by another Zurich contemporary, the physicist Felix Bloch who was a student there at the time.

In his "Reminiscences of Heisenberg and the Early Days of Quantum Mechanics" Bloch mentioned that "once at the colloquium [during winter semester 1925/1926] I heard Debye saying something like: 'Schrödinger, you are not working right now on very important problems anyway. Why don't you tell us sometime about that thesis of de Broglie, which seems to have attracted some attention?¹" So, in one of the next colloquia, Schrödinger gave a beautifully clear account of how de Broglie associated a wave with a particle and how he could obtain the quantization rules of Bohr and Sommerfeld by demanding that an integer number of waves should be fitted along a stationary orbit. When he had finished, Debye casually remarked that he thought this way of talking was rather childish. As a student of Sommerfeld he had learned that, to deal properly with waves, one had to have a wave equation. It sounded quite trivial and did not seem to make a great impression, but Schrödinger evidently thought more about the idea afterwards."⁽²²⁹⁾ Bloch further recalled: "Just a few weeks later [i.e., after the first colloquium] Schrödinger gave another colloquium which he started by saying: 'My colleague Debye suggested that one should have a wave equation: well. I have found one."⁽²³⁰⁾ Then he presented what is now called the Schrödinger equation for the hydrogen atom.

We do not have available any document which allows us to decide the case for or against Schrödinger being set on the road to the wave equation by Debye. All we know is that Schrödinger himself considered the introduction of the wave equation as a crucial step, beyond Louis de Broglie's theory, in establishing wave mechanics. The earliest preserved manuscript on wave mechanics is a memorandum of three pages on "H-atom Eigenvibrations" which Schrödinger composed late in 1925.⁽²³¹⁾ This memorandum contains, in sketchy words and some detailed formulas, the derivation and solution of a relativistic wave equation for the hydrogen atom. Schrödinger started from a phase wave for the electron of mass m and velocity v, as envisaged by de Broglie in his thesis and described by the relations for the frequency v and the phase velocity u,

$$v = \frac{mc^2}{h\sqrt{1-\beta^2}} \tag{57}$$

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and

$$u = \frac{c}{\beta} = \frac{c^2}{v} = \frac{mc^2/\sqrt{1-\beta^2}}{mv/\sqrt{1-\beta^2}} = \frac{\text{energy}}{\text{momentum}}$$
(58)

where $\beta = v/c$, c is the velocity of light *in vacuo*, and h Planck's constant. As a first example, he generalized the relations (57) and (58), valid for the phase wave in the field-free space, to the case of electron phase waves in the electric field of a hydrogen nucleus, writing

$$hv = \frac{mc^2}{\sqrt{1-\beta^2}} - \frac{e^2}{r}$$
(59)

and

$$u = \frac{hv}{mv/\sqrt{1-\beta^2}} = \frac{mc^2/\sqrt{1-\beta^2} - e^2/r}{mv/\sqrt{1-\beta^2}}$$
(60)

where e and |e| denote the charge of the electron and of the hydrogen nucleus, respectively.

Next, in his memorandum on the hydrogen eigenvibrations, Schrödinger eliminated the electron's velocity v, expressing the phase velocity u as a function of the frequency v and the nucleus-electron distance r:

$$u = c \frac{hv/mc^2}{\sqrt{(hv/mc^2 + e^2/mc^2r)^2 - 1}}$$
(60')

Now he inserted the expression for u into the (relativistic) wave equation for the phase wave function ψ (= $\psi(x, y, z, t)$), i.e.,

$$\Delta \psi = \left(\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2}\right)\psi = -\frac{4\pi^2 v^2}{u^2}$$
(61)

obtaining

$$\Delta \psi = -Q\psi = -\frac{4\pi^2 m^2 c^4}{h^2} \left[\left(\frac{hv}{mc^2} + \frac{e^2}{mc^2 r} \right)^2 - 1 \right] \cdot \psi \tag{62}$$

Evidently, the particular form of the wave equation, Eq. (62), followed from assuming a periodic behavior of the phase wave in time with the frequency v, or $\psi \sim \exp(2\pi i v t)$.

Equation (62) was the first appearance of the well-known (relativistic) Schrödinger equation. Schrödinger knew well the further treatment of the wave equation (62) from his student days in Hasenöhrl's physics seminar; for the details of solving it, he made use of Ludwig Schlesinger's book on differential equations.⁽²³²⁾ The first rough comparison of Schrödinger's solution of Eq. (62) with Sommerfeld's theory seemed to endow Schrödinger's approach with some respectability, as it established (in spite of the quite different physical foundation) a close analogy to Sommerfeld's relativistic theory of the hydrogen atom.⁽²³³⁾ A more detailed check of the situation, however, revealed a difficulty: Schrödinger's formula for the energy states yielded different values than those obtained by Sommerfeld ten years previously, thus spoiling the earlier perfect agreement between theory and experiment. Thus, for instance, Schrödinger's relativistic formula for the energy states did not account correctly for the fine structure of hydrogen-like atoms, such as hydrogen, ionized helium, doubly-ionized lithium, etc.; the level splitting for the main quantum number 2 became 8/3 as large as the one obtained from Sommerfeld's theory-while the latter was in perfect agreement with experiments, notably for ionized helium. For a while he sought to escape from the dilemma by modifying the wave theory; but he obviously did not get any satisfactory result. That was the status which Paul Dirac recalled as follows⁽²³⁴⁾:

Schrödinger explained it [the story of his treatment of the relativistic equation] many years later, during a little conversation we were having, and he said that he first obtained his wave equation as a generalization of the de Broglie equation, referring to an electron moving in an electromagnetic field. This equation which he first got was a relativistic one. He got a result not in agreement with observation. And then, of course, he applied it to the electron in the hydrogen atom. The reason it did not agree with observation was that there was no reference in it to the spin of the electron. People had just begun to think about the spin in those days, and had not yet set up any detailed theory involving it. Schrödinger's original wave equation had no reference to spin at all and therefore gave the wrong result.

So far Dirac's account completely fits the contents of Schrödinger's outline of the relativistic hydrogen theory; hence it can be taken as essentially completely substantiated. Dirac also recalled what Schrödinger told him⁽²³⁴⁾:

And when Schrödinger found this wrong result, he was excessively disappointed. He thought his whole idea was not good at all, and he just abandoned it [Dirac reported in 1981, continuing]: It was some months later when he went back to this work, and noticed that in nonrelativistic approximation the result of his calculation did agree with observation, that he published his work therefore as a nonrelativistic theory.

On first inspection the information in the last sentence sounds strange. Indeed, in the nonrelativistic limit Schrödinger's relativistic calculation gave rise to the same formula that Sommerfeld had derived in 1915, and this in turn agreed with the original result of Niels Bohr back in 1913 and thus with the spectroscopic data—if one neglected of course (as Schrödinger had done) the motion of the hydrogen nucleus. Why did Schrödinger not jump immediately onto nonrelativistic theory? Here we have to remember the starting point of the whole consideration, namely the relativistic phase waves of Louis de Broglie. If there was anything true in matter waves, it had to be established by a relativistic calculation. Any failure of such a calculation had to cast a deep shadow on the entire wavetheoretical approach to the quantum problem. Consequently, we should be able to understand perfectly Schrödinger's disappointment about the "wrong result," and his temporary abandonment of the hydrogen problem cannot be called really strange; on the contrary, it was completely logical. We see also that this part of the Dirac story deserves full credit. The question to be answered still remains: How long did Schrödinger take until he returned to the hydrogen problem?

Since the date of the reception of Schrödinger's first published paper on the nonrelativistic hydrogen atom is January 27, 1926, and the preliminary memorandum on the relativistic hydrogen equation must be dated after November 16, 1925 (the date of his letter to Landé), the maximum period for the entire development was about two months.⁽²³⁵⁾ On the other hand, one can also determine a minimum period from some additional information. The American physicist David M. Dennison recalled from a meeting with Schrödinger in 1927 that the latter had told him about a report on the relativistic hydrogen equation which he-Schrödinger-had never attempted to publish.⁽²³⁶⁾ Even more definite and authoritative appears the story given by Schrödinger himself in 1956 to Wolfgang Yourgrau, who had served as his assistant in Berlin; Schrödinger mentioned in a letter that he had written a paper on the "relativistically framed" theory which "is incorrect owing to the appearance of half integers instead of integers," adding: "My paper in which this is shown has never been published; it was withdrawn by me and replaced by the nonrelativistic treatment."(237) Based on these statements, one would be inclined to assume a rather late date for the completion of Schrödinger's considerations on the relativistic hydrogen atom, say only a few weeks before the submission of the paper containing the nonrelativistic equation. The period between the failure of the relativistic theory and the new start on the nonrelativistic theory would then shrink to a few weeks or even days.⁽²³⁸⁾

Schrödinger's authoritative statement and testimony notwithstanding, the available documents and information contradict his account. First, there exists no draft of a paper in the Schrödinger Nachla β , except the above-mentioned memorandum of three pages on the relativistic equation. Second, the detailed notebook on "Eigenwertproblem des Atoms," which gives an account of the status of Schrödinger's theory prior to the final publication, is available (it must therefore have been started by late December 1925); it opens right away with the headline: "Ohne Relativistik (erste Näherung)" ("Without Relativity (First Approximation)"). It contains only nonrelativistic approximations to various problems involving the hydrogen atom, the free atom, as well as the atom in a magnetic and an electric field.⁽²³⁹⁾ Third, Schrödinger, who always insisted on a very good fit of experimental data by the theoretical description, would not be prepared—as he had once written to Sommerfeld in another context—to "knock into the face of experimental spectroscopy"⁽²⁴⁰⁾ by publishing his wrong theory of the relativistic hydrogen atom. Indeed, the discrepancies between the observed fine structure and the structures following from Schrödinger's calculations were too large to be acceptable to him. Altogether, we must definitely conclude that Schrödinger gave up the relativistic theory before the end of December 1925 and that he never thought of submitting a paper containing the wrong relativistic treatment.

To determine the time delay between Schrödinger's relativistic hydrogen theory and his nonrelativistic theory, one must also take into account how the entire problem was related to the other problems studied by Schrödinger in the last months of 1925. In that period he submitted three papers for publication: one on color theory, which we have discussed earlier, was presented to the Vienna Academy of Sciences on December 17, $1925^{(158)}$; the other two were devoted to the theory of ideal gases. We have discussed earlier that the first of these papers, entitled "The Energy States of the Ideal Monatomic Gas" and communicated to the Prussian Academy of Sciences by Einstein on January 7, 1926, was completed in early December 1925,⁽²¹³⁾ and that Schrödinger by that time had already begun to work on a second paper which also dealt with gas theory. He described the idea pursued in this work to Einstein as follows: "Aim: it is preferable not to alter the type of statistics but rather to exchange the concepts 'material substrate' and 'energy content.' The phase cells are-similar to those in the 'quantization of the aether body' by Jeans-Debye---the material substrate."⁽²⁴¹⁾ He immediately added an outline of the results achieved, reporting: "It is evident that one must, by an application of the old statistics, rediscover your results [i.e., the results of Einstein's gas theory]. I have succeeded in doing so, totally by applying the usual Planck method of the sum of states. The condition of a constant number of molecules, which is so trivial in conventional gas theory, appears in my view to be very strange and nearly a bit mystical. One would almost prefer it to be able to drop it, which would very much simplify the calculation although, of course, that would lead to totally wrong results. This condition of a constant number of molecules provides the main difference between your gas

theory and radiation theory."⁽²⁴¹⁾ Finally, he stated the explicit results of the calculations: in radiation theory the energy of a quantum was proportional to $s^{1/3}$ and in gas theory to $s^{2/3}$, where s denoted the (quantum) number of the state.

Ten days after he had written to Einstein, Schrödinger completed a paper containing the detailed results. The *Physikalische Zeitschrift* received that paper, "On Einstein's Gas Theory," on December 15, 1925 and it was published in the issue of March 1, 1926.⁽²⁴²⁾ The paper on Einstein's gas theory involved the concept of the phase wave explicitly. Schrödinger presented a detailed demonstration of what he had already written to Einstein on November 3, namely that through de Broglie's thesis Einstein's "second degeneracy paper has become completely clear." We can imagine that Schrödinger first got the idea of studying the gas problem already early in November; but he does not seem to have obtained any particular result before the middle of the month, otherwise he would have taken the opportunity of reporting either to Einstein (in his letter of November 3) or to Landé (in his letter of November 16).⁽²⁴³⁾

We conclude that both lines of research, the gas theory and the hydrogen wave equation, were begun simultaneously after the middle of November 1925 when Schrödinger started to extend de Broglie's geometrical phase wave construction to more complex atomic situations. He may also have obtained his first results on the hydrogen equation at the beginning of December, when he reported the gas-theoretical results to Einstein (i.e., on December 4).⁽²⁴¹⁾ If so, why did the same letter not contain any hint to the hydrogen equation? Three reasons may be given: either Schrödinger wished to concentrate only on the gas problem, or he still hoped to improve on the bad relativistic result; or, he had not yet evaluated the equation. An alternative possibility is, of course, that Schrödinger started the wave equation approach to hydrogen only after December 4, 1925.

In any case, the scenario following from all our information is perfectly consistent with the assumption that Schrödinger interrupted his work on the hydrogen equation somewhere in the first half of December 1925. After all, he had enough to work on at that time. Besides finishing the work on Einstein's gas theory, he also submitted a paper on a completely different subject, "On the Relation between the Four-Color and the Three-Color Theory," to the Vienna Academy of Sciences, where it was communicated to the meeting of December 17, 1925.⁽¹⁵⁸⁾ The investigation on color theory, which we have discussed earlier, provided Schrödinger with a, perhaps, most welcome opportunity to escape from the problems arising in atomic theory. Soon after the middle of the month, after all the above-mentioned papers had been completed and submitted for publication, and especially after the Christmas vacation in 1925 which had liberated Schrödinger from his teaching duties, he would again throw himself into the hydrogen problem; in this second round he was able to find a successful solution to the problem, at least when restricting himself to the nonrelativistic case.

11. THE NONRELATIVISTIC HYDROGEN EQUATION

The analysis of Schrödinger's work in 1925—the published articles as well as the studies contained in private notebooks or indicated in letters to colleagues—which we have presented thus far, reduced the crucial period of inventing the nonrelativistic wave equation to roughly six weeks. That is, Schrödinger started work on it after December 16, 1925 and completed it with the submission of the first paper on it on January 26, 1926. Three sources throw some light on these decisive weeks: (1) two unpublished manuscripts; (2) the letters exchanged around that period, especially with Wilhelm Wien; and (3) the published paper on "Quantization as an Eigenvalue Problem. (First Part)."⁽²⁴⁴⁾ Only a properly weighted combination of all three separate pieces of information, together with selected recollections of physicists who were close to Schrödinger in those days, can resolve the apparently enormous step that lies between the recognition of the significance of Louis de Broglie's matter wave concept for the problem of quantization in atomic theory, and the final wave equation for the hydrogen atom and its successful solution.

A careful examination of Schrödinger's scientific correspondence is most important for fixing the final steps leading to the hydrogen wave equation and its solution. The letters which Schrödinger exchanged from the summer of 1925 with Wilhelm Wien in Munich deserve special attention. Their content allows us to fill essential gaps existing in our knowledge of the first half of the crucial period, i.e., up to January 8, 1926.⁽²⁴⁵⁾ After that, however, the route to the paper on the nonrelativistic hydrogen wave equation appears to be evident and straightforward, especially if one takes into account the information contained in the 72-page notebook entitled *"Eigenwertproblem des Atoms. I.*" Schrödinger then devoted himself to a refinement of the mathematical methods which enabled him to obtain the energy values of the nonrelativistic hydrogen atom.

The Wien-Schrödinger correspondence began on May 25, 1925, with Wilhelm Wien inviting Schrödinger to contribute an article on color theory and physiological processes to the *Handbuch der Experimentalphysik*, which he was editing with Friedrich Harms. Several letters were exchanged between Wien and Schrödinger concerning this matter, but ultimately the contribution on physiological optics for the Handbuch was made by Albert König of Jena.⁽²⁴⁶⁾ With the progress of this correspondence. Wien began to take a great personal interest in the scientific and professional life of Schrödinger, and they discussed numerous topics of common concern-as, for instance, the Bohr-Kramers-Slater theory of radiation and its disproof by the experiments of Hans Geiger and Walther Bothe. Another item of common interest was the repetition of the Michelson-Morley experiment by Dayton Clarence Miller, which the latter had performed in 1921 and which seemed to yield a small positive effect in contrast to the original experiment. Even Einstein was moderately concerned about the results of Miller's experiment, although he "did not believe in [their] accuracy."⁽²⁴⁷⁾ The only way to deal with the problem properly and adequately seemed to be to check the results of Miller thoroughly by an additional experiment, possibly at higher altitudes-because Miller had not observed a definite effect at sea level. Wien and Schrödinger discussed the plans for the repetition of Michelson's experiment; it was ultimately decided that Georg Joos would carry out the Michelson experiment, while Rudolf Tomaschek-a former student of Philipp Lenard and then his assistant at Heidelberg-would work on the Trouton-Noble experiment at Jungfraujoch in Switzerland. Tomaschek performed his experiment in April 1926, and the previous negative result, i.e., no effect from ether motion, was confirmed. The final death blow to a positive result of the Miller experiment was provided by Georg Joos, who after four years of preparation and collaboration with the Zeiß Company in Jena, also arrived at a negative result in September 1930.⁽²⁴⁸⁾

In spite of serious efforts on both sides the first project proposed by Wien to Schrödinger-the Handbuch article on physiological optics-did not materialize, while the second project, initiated by Schrödinger-the repetition of the Michelson-Miller experiment-was soon taken out of their hands by the Wissenschaftliche Jungfraujochkommission. A third enterprise, however, again promoted by Wien, worked much better. Early in their correspondence, back in June 1925, Schrödinger had requested to publish a small paper "On the Performability of the Relativity Postulate in Classical Mechanics" in the Annalen der Physik, of which Wien was the editor.⁽²⁴⁹⁾ Wien had promptly accepted, and the paper, received on June, 16 was published in the August issue of the Annalen.⁽²⁵⁰⁾ Six months later, on December 24, 1925, Wien closed a letter to Schrödinger by saying: "I have not received anything for the Annalen from Zurich for a long time. I would very much enjoy obtaining again several Swiss papers soon." He repeated this request in the following letter: "I would like to ask you whether or not you want to submit your papers again to the Annalen."⁽²⁵¹⁾ Schrödinger immediately raised Wien's expectation by promising him a new paper for the Annalen: "I now want to get on with the vibration theory of the atom at once, and I hope to be able very soon to send you a communication on that problem."⁽²⁵²⁾ Actually, this hint about the "vibration theory of the atom" was not the first notice which Wien had received concerning Schrödinger's main recent scientific research. One and a half weeks earlier, on December 27, 1925, Schrödinger had reported: "At the moment I am plagued by a new atomic theory.... I believe that I can write down a vibrating system—constructed in a completely natural manner and not by ad hoc assumptions—which has as its eigenfrequencies the term frequencies of the hydrogen atom.... I soon hope to be able to write a bit more in detail and more illuminatingly about this. At the moment I still have to learn the mathematics to handle the vibration problem fully—[it's] a linear differential equation, similar to Bessel's, however less known and exhibiting strange boundary conditions; these are connected with it and not imposed from outside."⁽²⁵³⁾

Schrödinger wrote the letter of December 27 to Wien from Arosa, where he spent about two weeks of Christmas vacation. Schrödinger tried to recover from the hardships of his work during the semester, and also attempted to find time for making progress in his scientific research problems. Indeed, in the Christmas vacation of 1925 he had a subject to work on. After completing the paper "On Einstein's Gas Theory" in the middle of December, he again threw himself with fresh energy into the approach to atomic structure implying the de Broglie matter waves. The content of Schrödinger's letter to Wien, dated December 27, 1925, thus provides the first documentary evidence of the progress achieved during the Christmas vacation. If compared with the earlier memorandum on "Hatom. Eigenvibrations"—which we have analyzed in Section 10—several new aspects appeared here that had not been present in the older treatment. First, Schrödinger explicitly wrote down the formula for the difference of the term frequencies of the hydrogen spectral lines,

$$v_n - v_m = R\left(\frac{1}{m^2} - \frac{1}{n^2}\right)$$
 (63)

where R denotes the Rydberg constant and n and m different integers. Second, each hydrogen frequency must be composed as the difference of two "term frequencies" $(v_n = m_e c^2/h - R/n^2, v_m = m_e c^2/h - R/m^2, m_e$ being the mass of the electron). Third, in the result obtained the relativistic effects had been neglected—hence Schrödinger attempted at this time a nonrelativistic theory of the hydrogen atom.

Schrödinger began the first notebook on "Eigenwertproblem des Atoms," which he started to compose on arrival in Arosa for the Christmas

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vacation of 1925, with a section on "Without Relativity (first approximation)." The basic content of this section consisted of a derivation of the nonrelativistic hydrogen equation by using the same line of thought as in his previous memorandum on the relativistic hydrogen equation. That is, Schrödinger started from an equation for the de Broglie phase wave frequency of an electron with velocity v in the electric field of a hydrogen nucleus, i.e.,

$$hv = m_e c^2 + \frac{m_e v^2}{2} - \frac{e^2}{r}$$
(64)

From this equation he derived the expression for the (nonrelativistic) momentum $m_e v$ of the electron,

$$m_{e}v = \sqrt{2m_{e}\left(h_{v} - m_{e}c^{2} + \frac{e^{2}}{r}\right)}$$
(65)

which he then inserted into the relation for the phase velocity u of the de Broglie wave, i.e.,

$$u = \frac{hv}{m_e v} = \frac{hv}{\sqrt{(2m_e(hv - m_e c^2 + e^2/r))}}$$
(66)

The u thus obtained could be used in the wave equation (61), yielding the following differential equation for the nonrelativistic hydrogen atom:

$$\Delta \psi = -\frac{4\pi^2}{h^2} 2m_e \left(hv - m_e c^2 + \frac{e^2}{r}\right)\psi$$
 (67)

Felix Bloch, then a student at Zurich, recalled that Schrödinger presented, a few weeks after his colloquium on de Broglie's matter waves, still another colloquium talk in the winter semester of 1925/1926, which he opened with the words: "My colleague Debye suggested that one should have a wave equation: well, I have found one."⁽²²⁹⁾ He meant by this remark the wave equation for the nonrelativistic hydrogen atom. About this second colloquium, Erwin Fues, another Zurich contemporary—at that time assistant to Schrödinger—remembered that it was again Debye who provided the idea of deriving wave mechanical equations from a variational principle. As Fues recalled: "The stimulus occurred in a colloquium discussion, after Schrödinger had presented his nonrelativistic theory. As far as I recall, Debye said—in this sense, not exactly these words—nothing but that one has succeeded in putting the most fundamental theories of physics into the form of a variational principle which gives an impressive summary [of the theories]."⁽²⁵⁴⁾

These recollections of Schrödinger's contemporaries in Zurich raise the important question as to what influences helped him in creating wave mechanics. Both Bloch and Fues implied that Schrödinger obtained an essential stimulus at two crucial stages of the development: namely, first, when he passed over from the geometrical approach of matter waves in atoms to the description by a wave equation in November or December 1925: and, second, when he set out in January 1926 to find the systematic and complete solution of the nonrelativistic hydrogen equation. It is very difficult to trace any of Debye's influence in Schrödinger's preliminary notes and the first publication, even apart from the fact that Debye's name is not mentioned there at all. Debye himself, when later confronted with the story that he might have helped to initiate the idea of the wave equation, replied: "No, I don't remember that. I don't remember that. I had no influence. Certainly not."(255) In spite of this rejection, one cannot exclude the possibility that Debye pleaded, in the discussion of Schrödinger's first Zurich colloquium, for having a wave equation for matter waves, and that his remark played some role in taking the wave equation approach. That is, he either pushed Schrödinger to the idea of developing a wave equation. or he confirmed Schrödinger's previous thoughts in that direction. With respect to the origin of the idea of using a variational principle as the basis for the wave equations, the available documents tend to answer the question of Debye's influence rather clearly. Schrödinger had already conceived the possibility of deriving the hydrogen equation before he presented the second Zurich colloquium-which he did after the Christmas vacation and after January 8, 1926. The proof can immediately be found in the fact that he hinted in the notebook "Eigenwertproblem des Atoms. I" early on (i.e., on p. 3) at the formulation of a variational principle for the wave equation, a method which he knew existed from his student days in Vienna. Moreover, he returned to the variational principle in a latter part of the same notebook, namely on p. 31, where he attempted to formulate a new program allowing him to put his previous results on the hydrogen wave equation into more systematic order. It appears evident that this new program constituted the first step in the fulfillment of his promise to Wilhelm Wien on January 8, 1926: "I soon hope to bring it into some orderly shape." As in the earlier situation with the wave equation we cannot, and do not wish to, exclude the possibility that Debye's remark about the usefulness of having a variational principle either paved the way to the new program or-what is even more plausible-it confirmed Schrödinger's original ideas formulated soon after returning to Zurich from the Christmas vacation.

Under the title "Programm," beginning on p. 30 of the notebook "Eigenwertproblem des Atoms. I," listed two topics, namely: "1. Co-motion

of the nucleus relativistically. 2. Old Hamiltonian analogy between optics and mechanics." As the first item he again treated (on pp. 30-31) the free relativistic hydrogen atom. Since the previous evaluations had vielded a wrong result for the fine structure, Schrödinger now hoped for a correction of the result if he considered in addition the motion of the hydrogen nucleus. He made suitable assumptions and elaborate calculations, but again he failed to solve the relativistic hydrogen problem. He then quickly turned to the second topic of his program, which should supply a justification and deepening of the whole wave equation approach. In his notebook, Schrödinger succeeded, in the case of the nonrelativistic free hydrogen atom and the Stark effect, in finding the same results as the old Bohr-Sommerfeld method of quantization. With the help of the mechanical-optical analogy, Schrödinger possessed the first elements of a general method by which he was able to derive, from a classical dynamical description of a mechanical system, the corresponding wave equation description. That is, early in January 1926, Schrödinger merely needed the existence of the analogy between the principle of the least action in mechanics.

$$\delta \int (E - V) dt = \int \sqrt{(E - V)} ds$$
 (68)

and Pierre de Fermat's variational principle of optics,

$$\delta \int n \, ds = 0 \tag{69}$$

establishing the close analogy between the index of refraction, n, and the square root of the kinetic energy of a mechanical system, $\sqrt{\mathfrak{I}}$, which can be expressed as $\sqrt{(E-V)}$ (i.e., the square root of the difference between total energy and potential energy). This analogy provided a direct motivation for obtaining the wave equation.

On January 27 and February 23 1926 the Annalen der Physik received two papers from Erwin Schrödinger, bearing the titles "Quantisierung als Eigenwertproblem" ("Quantization as a Problem of Eigenvalues") and "Quantisierung als Eigenwertproblem (Zweite Mitteilung)" ("Quantization as a Problem of Eigenvalues, Part II"), which were duly published in the issues of March 13 and April 6 1926.^(244.256) They constituted the first two of a series of six papers by Schrödinger, which Max Born would later admire as being "magnificent" ("großartig"),⁽²⁵⁷⁾ and which signified the birth of a new quantum theory: wave mechanics. Friedrich Hund described in short, in his Geschichte der Quantentheorie, what these papers contained in the following words: "In January the first paper was finished; it gave the calculation of the energy values of the hydrogen atom and kept the connection with de Broglie's ideas somewhat concealed. The second paper, in February 1926, carried out the de Broglie point of view: namely, that the new mechanics was related to classical mechanics just as wave optics was related to geometrical optics; it also contained simple applications."⁽²⁵⁸⁾

The basic content of Schrödinger's first communication on "Quantization as a Problem of Eigenvalues"⁽²⁴⁴⁾ was restricted to one question: the nonrelativistic hydrogen problem, or the Kepler problem of one electron in the Coulomb field of an atomic nucleus. Schrödinger selected this problem from the bulk of problems that he had been treating with the wave equation approach in the course of the previous month (in his notebook "Eigenwertproblem des Atoms. I") for two reasons: first, the hydrogen problem represented the starting point of his concern with matter waves; second, he had worked out the mathematics completely and could use the solution to demonstrate the power of the new method. Schrödinger hoped to be able to remove the arbitrariness of the quantum conditions of the old quantum theory of atomic structure; he claimed that he could derive them from a deeper, more fundamental principle, a kind of new Hamiltonian principle, which he had obtained by connecting the Hamiltonian principle of classical dynamics with the idea of the matter wave equation. In other words, he argued that the undulatory features of mechanical systems might give rise to the quantum conditions in atomic systems. He wrote: "In this paper I wish to consider, first, the simplest case of the hydrogen atom (nonrelativistic and unperturbed), and show that the customary quantum conditions can be replaced by another postulate, in which the notion of 'whole numbers,' merely as such, is not introduced." He explained further: "Rather when the integralness does appear, it arises in the same natural way as it does in the case of *node numbers* of a vibrating string. The new conception is capable of generalization, and strikes, I believe, very deeply at the nature of the quantum rules."⁽²⁵⁹⁾

Schrödinger first devoted himself to producing the desired "derivation" of quantum numbers in the case of the nonrelativistic hydrogen atom. He demonstrated, repeating the steps of the notebook on "Eigenwertproblem des Atoms. I," how to establish the variational problem for a general quantum-dynamical problem. In classical physics—and the old quantum theory—that problem was described by Hamilton's partial differential equation,

$$H\left(q,\frac{\partial S}{\partial q}\right) = E \tag{70}$$

with H denoting the Hamiltonian function depending on the position variables q, the momentum variables $p = \partial S/\partial q$, and E the energy of the

system. Schrödinger further assumed that a solution of Eq. (70) existed, in which S could be given by a sum of functions each depending only on one variable q. He then replaced the classical function S by a new function ψ via the relation

$$S = K \ln \psi \tag{71}$$

where K had the dimensions of an action. He noticed further that Eq. (70), if expressed in terms of the new function ψ , assumed the particular shape: "quadratic form (of ψ and its first derivatives) equated to zero," as long as the relativistic mass dependence was neglected.⁽²⁶⁰⁾ He wrote: "We seek a function ψ , such that for any arbitrary variation of it the integral of the said quadratic form, taken over the whole coordinate space, is stationary, ψ being everywhere real, single-valued, finite, and continuously differentiable up to the second order." Finally he stressed in italics: "The quantum conditions are replaced by this variational problem."⁽²⁶⁰⁾

The replacement of the Hamiltonian differential equation by the variational problem involving the space function ψ appears to be an enormous step, both conceptually and mathematically. Certainly, from the point of view of logic, the connection is a rather natural one; after all, Eq. (70) itself followed from a variational principle, i.e., its solution also solved the extremum problem of dynamics. In this sense one must understand the assertion of Debye, in the Zurich colloquium reported earlier, that all important fundamental theories of physics might be cast in the form of a variational principle. However, in mechanics, one was dealing thus far with the variation of the *time integral* over the Lagrange function, while Schrödinger's formulation led to a variational principle of a space integral. This shift in the nature of the problem originated from the earlier ideas conceived by Schrödinger in applying Louis de Broglie's matter waves. Notably the replacement of electron orbits by standing matter waves introduced a function of space only, i.e., the time-independent wave amplitude ψ . The whole process appeared to be completely natural and straightforward to Schrödinger who had been, from his student days, familiar with the connection between differential equations for vibrations in continuum mechanics and variational principles, a connection which he now could take over literally into atomic theory.

Schrödinger noticed that Eq. (70) gave rise, with the transformation (71), to the quadratic form

$$F(x, y, z) = \left(\frac{\partial \psi}{\partial x}\right)^2 + \left(\frac{\partial \psi}{\partial y}\right)^2 + \left(\frac{\partial \psi}{\partial z}\right)^2 - \frac{2m}{K}\left(E + \frac{e^2}{r}\right)\psi^2$$
(72)

in the case of the nonrelativistic hydrogen problem, expressed in Cartesian

coordinates (r, the radial variable, being equal to $\sqrt{x^2 + y^2 + z^2}$). This quadratic form had to be inserted into the variational problem

$$\delta J = \delta \iiint F(x, y, z) \, dx \, dy \, dz = 0 \tag{73}$$

Schrödinger took over the evaluation of the variation from his notebook [where, following from the variational principle,

$$\delta \int \left[\left(\frac{\partial \psi}{\partial x} \right)^2 + \left(\frac{\partial \psi}{\partial y} \right)^2 + \left(\frac{\partial \psi}{\partial z} \right)^2 + \frac{2m}{K^2} (V - E) \psi^2 \right] d\tau = 0, \quad d\tau = dx \, dy \, dz,$$

he had

$$2\int \delta\psi \frac{\partial\psi}{\partial n} df - 2\int \delta\psi \left[\Delta\psi + \frac{2m}{K^2} (E - V)\psi \right] d\tau = 0$$

with $\Delta \psi = \partial^2 \psi / \partial x^2 + \partial^2 \psi / \partial y^2 + \partial^2 \psi / \partial z^2$]. If the surface integral, extended over an infinitely distant closed surface, provided no contribution, i.e.,

$$\int \left(\delta\psi\,\frac{\partial\psi}{\partial n}\right)df = 0\tag{74}$$

—which implied a condition for the behavior of ψ , respectively its normal derivative, for large r values—then the variational principle possessed, as an Eulerian equation, the wave equation

$$\Delta \psi + \frac{2m}{K^2} (E - V) \,\psi = 0 \tag{75}$$

The solution of this equation, with ψ fulfilling the conditions mentioned above, thus also satisfied the variational principle given by Eqs. (72) and (73).

Equation (75) represented, of course, the second-order differential equation for the matter wave function ψ , which Schrödinger had considered, from the previous December, as the candidate for the description of the nonrelativistic hydrogen atom [see Eq. (67)]. He had discovered that it possessed a very peculiar property: acceptable solutions seemed to exist merely for selected values of the energy *E*. From the equivalence of the variational principle and the differential equation, he was confident of being able to show "that ψ can be so chosen for *all positive*, but only for a *discrete set of negative*, values of *E*. That is, the above-mentioned variational problem has a discrete and a continuous spectrum of eigenvalues."⁽²⁶⁰⁾ This property appeared to be very desirable from the

point of view of the physical interpretation, as "the discrete spectrum corresponds to the Balmer terms and the continuous [spectrum] to the energies of the hyperbolic orbits."⁽²⁶⁰⁾

Schrödinger now presented a complete investigation of the mathematical questions involved in the solution of Eq. (75). In clarifying the details of these questions, Hermann Weyl became active, hence Schrödinger felt that he "owed deepest thanks to Hermann Weyl" as he acknowledged in his paper.⁽²⁶¹⁾ For the purpose of dealing with Eq. (75), Schrödinger introduced the polar coordinates, r, θ , and ϕ . He assumed, then, that ψ might be written as the *product* of three functions, each only of r, θ , or ϕ , and claimed that the method was well known. Indeed, Schrödinger had learned, more than fifteen years earlier, in Hasenöhrl's physical seminar on the treatment of the problem of electromagnetic radiation, that the very same procedure applied to the wave equation

$$\Delta \psi + q^2 \psi = 0 \tag{76}$$

and it yielded, as now in the case of Eq. (75), "for the dependence [of the function ψ] on the polar angles [θ and ϕ] a surface harmonic,"⁽²⁶¹⁾ $P_n(\theta, \phi)$, with integral number *n*. Schrödinger emphasized: "The limitation of *n* to integral values is *necessary* so that the dependence [of the surface harmonic] on the polar angles may be single-valued."⁽²⁶¹⁾

In the case of Eq. (75), Schrödinger obtained for the *r*-dependent part of ψ , which he called χ , the second-order differential equation

$$\frac{d^2\chi}{dr^2} + \frac{2}{r}\frac{d\chi}{dr} + \left(\frac{2m_e E}{K^2} + \frac{2m_e e^2}{K^2 r} - \frac{n(n+1)}{r^2}\right)\chi = 0$$
(77)

with n = 0, 1, 2, 3... Evidently, Eq. (77) possessed the same mathematical structure as the first matter-wave equation (62), which he had considered many weeks earlier for the relativistic hydrogen problem. The standard procedure for solving this equation was displayed in Schlesinger's textbook on differential equations,⁽²³²⁾ and Schrödinger simply took over the details in his paper.⁽²⁶²⁾ The first step consisted of studying the singularities of Eq. (77) in the complex *r*-plane, namely at the points r = 0 and $r = \infty$. The variational principle demanded solutions that were finite and unique in the whole configuration space, i.e., for all real, nonnegative *r*. Schrödinger now claimed: "The equation has in *general* no integral which remains finite at *both* end points; such an integral exists only for certain special values of the constants in the equation. It is now a question of defining these special values." He emphasized subsequently: "This is the *jumping-off* point of the whole investigation."⁽²⁶³⁾

Closely following *Schlesinger*, Chapter 3, Schrödinger first investigated the singular point r = 0. The integral of Eq. (77) could be assumed to have the form

$$\chi = r^{\alpha} U(r) \tag{78}$$

where the exponent α is a positive root of the so-called indicial equation

$$\rho(\rho - 1) + 2\rho - n(n - 1) = 0 \tag{79}$$

associated with Eq. (77). Evidently,

$$\alpha = n \tag{80}$$

By inserting the substitution (78) with the above α value into the differential equation, a new differential equation of similar type arose for the function U, i.e.,

$$\frac{d^2U}{dr^2} + \frac{(2n+1)}{r}\frac{dU}{dr} + \frac{2m_e}{K^2}\left(E + \frac{e^2}{r}\right)U = 0$$
(81)

The solution of this so-called Laplacian differential equation was again given in Schlesinger's book⁽²³²⁾; it yielded

$$U = \int_{L} \exp(zr)(z-c_1)^{\alpha_1-1} (z_2-c_2)^{\alpha_2-1} dz$$
 (82)

with

$$c_1 = \sqrt{-\frac{2m_e E}{K^2}}, \qquad c_2 = -\sqrt{-\frac{2m_e E}{K^2}}$$
 (82a)

and

$$\alpha_{1} = \frac{m_{e}e^{2}}{K\sqrt{-2m_{e}E}} + n + 1$$
(82b)
$$\alpha_{2} = -\frac{m_{e}e^{2}}{K\sqrt{-2m_{e}E}} + n + 1$$

The path of integration L has to satisfy the condition

$$\int \frac{d}{dz} \left[\exp(zr)(z-c_1)^{\alpha_1} (z-c_2)^{\alpha_2} dz = 0 \right]$$
(83)

Schrödinger had previously analyzed the path of integration in detail in his notebook on "Eigenwertproblem des Atoms. I."⁽²⁶⁴⁾

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As in the notebook, Schrödinger considered separately the two cases: E > 0 and E < 0. In the first case, the parameters c_1 and c_2 became purely imaginary quantities. Because of Eqs. (78) and (80), the radial function χ converged to zero then, and Schrödinger concluded: "The Eulerian differential equation (75)] of our variational problem has, for every positive E, solutions which are everywhere single-valued, finite, and continuous, and which tend to zero with r^{-1} at infinity, under continual oscillations."⁽²⁶⁵⁾

In the case of negative energy E, corresponding to a bound hydrogen atom, Schrödinger first considered the situation in which c_1 , Eq. (82a), did not assume an integral value. In this case, Schrödinger inferred: "Our integral function U, which alone of the solutions of [Eq. (81)] is considered for our problem, is therefore *not* finite for large r," or, more sharply, "for negative values of E which do not satisfy the condition [that c_1 is an integral number] our variational problem has no solution."⁽²⁶⁶⁾

The consequence was, of course, that negative E-solutions of the variational problem existed only for discrete values of E, namely for those values that satisfied the equation

$$\frac{m_e e^2}{K\sqrt{-2m_e E}} = l, \qquad l = 1, 2, 3,...$$
(84)

Hence also the crucial exponents $\alpha_1 - 1$ and $\alpha_2 - 1$ assumed likewise integral values,

$$\alpha_1 - 1 = l + n, \qquad \alpha_2 - 1 = -l + n$$
 (85)

The regularity behavior of U now depended on whether the integral l was greater than the integer n or not. Schrödinger could exclude the case $l \le n$, because it led to no solution which was both finite at r = 0 and $r = \infty$. But the case l > n admitted proper solutions, because it implied that the integrand on the right-hand side of Eq. (82) became zero at the point c_1 , while it exhibited a pole of order one or greater at c_2 . The function U, in that case, was simply given by the residue at this pole,⁽²⁶⁷⁾ and χ became

$$\chi = f\left(r\frac{\sqrt{-2m_eE}}{K}\right) \tag{86}$$

with

$$f(x) = x^{n}(-x) \sum_{k=0}^{l-n-1} \sum_{k!}^{(-2x)^{k}} \binom{l+n}{l-n-1-k}$$
(87)

Schrödinger summarized the situation with the words: "For E negative, our variational problem has solutions if, and only if, E satisfies

the condition [(84)]. Only values smaller than l (and there is always one such at our disposal) can be given to the integer n, which denotes the surface harmonic appearing in the equation. The part of the solution depending on r is given by [Eq. (86)]."⁽²⁶⁸⁾ And since the surface harmonic of degree n involved (2n + 1) constants, also "the discovered solution has exactly (2n + 1) arbitrary constants for any possible (n, l) combination, and therefore for a prescribed value of l has l^2 arbitrary constants."^(268,269) Of the remaining mathematical questions, Schrödinger did not deal with the problem of the completeness of the whole system of eigenfunctions which solved the hydrogen equation.

The interpretation of the results thus achieved in terms of the Bohr-Sommerfeld theory of atomic structure presented no particular problem. Schrödinger noted: "The well-known Bohr energy levels, corresponding to the Balmer terms, are obtained if to the constant K, introduced into [Eq. (71)] for reasons of dimensions, we give the value

$$K = \frac{h}{2\pi}$$
 [(88)]

from which comes

$$-E_{I} = \frac{2\pi^{2}m_{e}e^{4}}{h^{2}l^{2}}$$
 [(89)]"⁽²⁷⁰⁾

Thus the following relation existed between the integral numbers arising in the above treatment of the hydrogen atom as a variational problem on the one hand, and the quantum numbers of the old theory on the other. Schrödinger wrote: "Our l is the principal quantum number. n + 1 is analogous to the azimuthal quantum number. The splitting up of this number through a closer definition of the surface harmonic can be compared with the resolution of the azimuthal quantum into an "equatorial" and a "polar" quantum."⁽²⁷⁰⁾ Finally, the reciprocal of the factor multiplying r in the expression for χ —see Eq. (86)—determined somehow the extension of the hydrogen atom; it could be related to the semiaxes of Sommerfeld's quantized Kepler ellipses, a_i , by

$$\frac{K}{\sqrt{-2m_e E}} = \frac{h^2 l}{4\pi^2 m_e e^2} = \frac{a_l}{l}$$
(90)

With these results Schrödinger completed his new treatment of the nonrelativistic hydrogen problem. Also, in the same communication, he attempted—in a preliminary and incomplete way—to throw light on the physical significance of the undulatory process in the atom which is connected with the wave function ψ .

12. FOUNDATIONS OF UNDULATORY MECHANICS

In Section 3 of his first communication, *Quantisierung als Eigenwertproblem*, Schrödinger had indicated the possibility that the wave function of the hydrogen atom might be interpreted as some "vibration process in the atom, which would more nearly approach reality than electronic orbits" and had added that he had "originally intended to found the new quantum conditions in this more intuitive manner."⁽²⁷¹⁾ However, in the paper he had restricted himself to a "neutral mathematical formulation," i.e., he had replaced the mechanical Hamilton–Jacobi equation (70) of the system via the mathematical substitution (71) by the wave equation (75). Four weeks later he was really ready to present publicly the "more intuitive" formulation, and he wrote right away in the introduction to his second paper on quantization as an eigenvalue problem:

Before we go on to consider the problem of eigenvalues for further special systems, let us throw more light on the *general* correspondence which exists between the Hamilton–Jacobi differential equation of a mechanical problem and the "allied" *wave equation*, i.e., Eq. [(75)] of Part I in the case of the Kepler problem. So far we have only briefly described this correspondence on its external analytic side by the transformation [(71)], which is in itself unintelligible, and to the equally incomprehensible transition from the equating in zero of a certain expression by the postulation that the *space integral* of the said expression shall be *stationary*.⁽²⁷²⁾

Schrödinger now did not want "to pursue further this procedure," which "was only intended to give a provisional, quick survey of the connection between the wave equation and the Hamilton–Jacobi equation."⁽²⁷²⁾

Although Schrödinger thus gave up his belief in the general value of his earlier "derivation" of the wave equation—e.g., he noticed that " ψ is not actually the action function of a definite motion in the relation stated in [(71)] of Part I"—he still held that "the connection between the wave equation and the variation problem is of course very real," since "the integrand of the stationary integral [see Eqs. (72) and (73)] is the Lagrange function for the wave process."⁽²⁷²⁾ That is, he spoke in his second communication, or Part II, definitely about a wave process in the atom. Hence, it is this publication that finally established the foundations and the definite outlines of what was later called "wave mechanics."⁽²⁷³⁾

The progress in the foundation and formulation of undulatory mechanics, presented by Schrödinger in the second communication, resulted from a deepened pursuit of the mechanical-optical analogy, which we have mentioned in the previous section.

During the crucial period when wave mechanics was created, Schrödinger had a regular correspondence with his Munich colleagues, the experimentalist Wilhelm Wien and the theoretician Arnold Sommerfeld. Wien, with his request to obtain manuscripts from Zurich for the *Annalen der Physik* (of which he was an editor), had stimulated Schrödinger to send his completed papers to Munich. Schrödinger asked Wien, before submitting the papers for printing, to show them to Sommerfeld, whom he regarded as a kind of referee in theoretical questions. Wien did so, and both he and Sommerfeld were the first physicists to respond to the new theory of atomic systems.

Wien raised a couple of questions—about the absorption and emission of high frequencies and how one would derive blackbody radiation in Schrödinger's theory—but he received Schrödinger's work quite favorably, as he found it to possess "the great advantage that one can also treat in it time changes of [quantum] states." Therefore he closed his letter by encouraging Schrödinger with the words: "In any case, I congratulate you on your achievement and wish you good luck with all my heart. Perhaps one will now succeed in solving the quantum problem which up to now seemed to be so hopeless."⁽²⁷⁴⁾

At Schrödinger's request, Wien had first passed the paper on Quantisierung als Eigenwertproblem to Sommerfeld before submitting it for printing in the Annalen. Almost simultaneously Schrödinger resumed his correspondence with Sommerfeld that had stayed in abeyance since July 1925; in a letter dated January 29, 1926 he explained the contents of his paper in a few words. He then mentioned the preliminary results from the calculations of further examples, such as the linear oscillator, the rotator, and the force-free motion of a mass point; and he raised the still unresolved problem of how to evaluate in his theory the intensity and polarization of spectral lines. Sommerfeld was thrilled by the news from Schrödinger. He informed Schrödinger that he was just about to prepare for a visit to London, where he wanted to present his usual views on atomic structure, when like a "thunderclap" ("Donnerschlag") he was hit by the content of Schrödinger's manuscript. He wrote to Schrödinger on February 3, 1926:

It is my impression that your method is a substitute for the new quantum mechanics of Heisenberg, Born, and Dirac (R[oyal] Soc[iety] Proc[eedings] 1925); in particular, a simplified method, so-to-speak, an analytic resolvent of the algebraic problem stated there, because your results agree with theirs.⁽²⁷⁵⁾

By this identity of results he meant those obtained for the oscillator and the rotator, which Schrödinger had reported to him in his letter. In his letter of February 3, 1926, Sommerfeld also commented on some details of Schrödinger's new paper.⁽²⁴⁴⁾ For example, he criticized the fact that the combination principle was not automatically, even approximately, satisfied. He raised the question: "By the way, are your vibrations undamped? Do

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they not radiate away or will the energy radiated away be replaced?"⁽²⁷⁵⁾ Finally, Sommerfeld asked the question: "Does there exist, perhaps, an analogy between your formulation of the variational principle and the vibration rectangle of Ritz, which led him to the Ritz formula?⁽²⁷⁶⁾ Nobody remembers anymore this intricate construction, but perhaps you will restore to it some respectability."⁽²⁷⁵⁾

Schödinger, on Sommerfeld's suggestion, studied Ritz' work in February 1926; he replied two weeks later: "With a shiver, I looked up the Ritz vibrations of the rectangle, but also with some admiration. I do not believe that this attempt has anything to do with the present one." (277) He saw the main difference in this:

Unfortunately, Ritz always attempted to represent the line frequencies; he believed that he had to derive eigenvalues of the form $1/n^2 - 1/m^2$, while one is dealing with the term values $1/n^2$. This fact of course, must, complicate the situation enormously. The most peculiar, great, and eternal discovery of Ritz, the combination principle, was then still too new to be grasped in its entirety, even by the author himself. Twenty-three years later one is admittedly in an easier position.⁽²⁷⁷⁾

Ritz' considerations, especially his square-plate model of the atom, did not help Schrödinger in understanding the wave equation approach. Meanwhile, he had been able to ponder a little more on what the atomic vibrations meant. Schrödinger obtained some help in trying to answer Sommerfeld's question on the damping of the ψ -vibrations; obviously these vibrations *per se* could neither be emitted nor absorbed. He wrote to Sommerfeld:

The ψ -vibrations are naturally not electromagnetic vibrations in the old sense. Between them some coupling must exist, corresponding to the coupling between the vectors of the electromagnetic field and the four-dimensional current in the Maxwell-Lorentz equations. In our case the ψ -vibrations correspond to the four-dimensional current, that is, the four-dimensional current must be replaced by something that is derived from the function ψ , say the four-dimensional gradient of ψ . But all this is my fantasy; in reality, I have not yet thought about it thoroughly.⁽²⁷⁷⁾

Sommerfeld's letter of February 3 did indeed stimulate Schrödinger in many respects. He agreed with Sommerfeld's cautious warning in assuming a simple physical reality for the ψ -waves. He wrote back: "By the way, my general presentation still approaches your wishes on a second point, namely with respect to the 'physical reality of the ψ -vibrations.' Since ψ in general depends on many more than three variables, the immediate interpretation as vibrations in the three-dimensional space is made difficult in any desirable manner."⁽²⁷⁷⁾ However, he protested against Sommerfeld's accusation that he, unlike Werner Heisenberg, "had put so many, possibly

unnecessary, assumptions *into* the theory." He closed his letter with the remark: "Thank you again, highly revered Professor, for your kind interest, and for a hundredfold direct and indirect assistance which I owe to you."⁽²⁷⁷⁾

In a treatment of the concepts and fundamental laws of field physics, written for the *Handbuch der Experimentalphysik*, Hans Thirring spoke about the relation between the optical and mechanical theories of Hamilton, referring at the end to their renewed application in the quantum theory by Louis de Broglie and Erwin Schrödinger. He then added, in contrast to this successful idea, a section reporting on "*Die verdorrten Ansätze der Mechanik*" ("The Dried-Up Ideas in Mechanics"), which he introduced by the statements:

Hamilton's optical-mechanical analogy would have nearly fallen into oblivion, had it not recently obtained a renewed importance through the just-mentioned considerations which are connected with the quantum phenomena. Hamilton's idea had in this case suffered the same fate as certain other, not uninteresting, thoughts that have emerged in the course of the last century, but which have not proven to be fruitful. Of the many examples existing, let us mention in the following only two, namely the attempt by Heinrich Hertz to eliminate forces from mechanics and replace them with the effects of inertia caused by hidden motions, and the hypotheses of Bjerknes and Korn concerning the nature of gravitation and Coulomb forces.⁽²⁷⁸⁾

While he was certainly—or so far?—right about the theoretical speculations mentioned on gravitation and electric forces, $^{(279)}$ Thirring apparently had not carefully studied the foundation of wave mechanics which Erwin Schrödinger, his friend and former colleague, provided in his second communication. In that paper, which was received by Wilhelm Wien in Munich on February 23, 1926, Schrödinger gave, in Section 1 entitled "*Die Hamiltonsche Analogie zwischen Mechanik und Optil*" ("The Hamiltonian Analogy between Mechanics and Optics"), definite hints on the use of ideas which Heinrich Hertz had discussed in his *Principles of Mechanics*, though without mentioning the name of Hertz.⁽²⁸⁰⁾ Schrödinger fully displayed in this section the mechanical-optical analogy along the lines he had sketched in the notebook on "*Eigenwertproblem des Atoms*. *II*." He started right away from the "complete" Hamiltonian partial differential equation for the principal characteristic function *W*, i.e.,

$$\frac{\partial W}{\partial t} + \Im\left(q_k, \frac{\partial W}{q_k}\right) + V(q_k) = 0 \tag{91}$$

W was of course identical with the action function, or with the time integral of the Lagrangian function taken along the minimum path of the system in its configuration space (the space spanned by its position coor-

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dinates). The expression \Im denoted the kinetic energy of the system, in principle a function of the coordinates q_k and the momentum variables p_k , in fact depending quadratically on the latter, which could be replaced by the derivatives, $\partial W/\partial q_k$. The V finally stood for the potential function or the potential energy. With the Ansatz

$$W = -Et + S(q_k) \tag{92}$$

the time-dependent equation (91) passed over into the time-independent equation,

$$2\Im\left(q_k, \frac{\partial W}{\partial q_k}\right) = 2(E - V) \tag{93}$$

with *E* representing an arbitrary constant of integration which can be physically interpreted as the total energy of the system. Schrödinger wrote \Im as a function of the position variables, q_k , and the corresponding velocities, \dot{q}_k , that is, $\bar{\Im}(q_k, \dot{q}_k)$, and introduced the line element *ds* by the equation

$$ds^2 = 2\bar{\mathfrak{J}}(q_k, \dot{q}_k) dt^2 \tag{94}$$

where the right-hand side is really a quadratic form of $dq_k(=\dot{q}_k dt)$. Equation (94) was exactly the same which Schrödinger had studied in his first study of Hertz' mechanics and Einstein's gravitation theory back in 1918.⁽²⁸¹⁾ Schrödinger then proceeded to write down the equation for the gradient of Hamilton's function W,

$$|\text{grad } W| = \sqrt{2(E - V)} \tag{95}$$

He commented on this result as follows: "This requirement is easily analyzed. Suppose that a function W, of the form [(92)], has been found which satisfies it [i.e., Eq. (95)]. Then this function can be clearly represented for every definite t, if the family of surfaces W = const be described in qspace and to each member a value of W be ascribed."⁽²⁸²⁾ He now presented the method of constructing the ensembles or families of W surfaces, starting from a given, arbitrary value W_0 when provisionally the time is regarded as a constant. He noted: "The construction rule... exhausts the contents of the differential equation, [because] each of its solutions can be obtained from a suitably chosen surface and W value."⁽²⁸²⁾ The construction rule consisted in assuming the surface $W = W_0$, choosing a positive side, erecting the normal at each point of the W_0 -surface in the positive direction, and cutting-off the step

$$ds = \frac{dW_0}{\sqrt{2(E-V)}} \tag{96}$$

the end points then constitute the surface $W = W_0 + dW$. Similarly, one could construct the surface $W = W_0 - dW$ in the other, negative direction. From Eq. (96) it followed that in a time interval dt the surface $W = W_0$ moved in the positive direction along the normal by a distance

$$ds = \frac{E \, dt}{\sqrt{2(E-V)}} \tag{97}$$

Equation (97) implied a (normal) velocity of the surface u,

$$u = \frac{ds}{dt} = \frac{E}{\sqrt{2(E-V)}} \tag{98}$$

depending—since E possesses a given, fixed value—on the position coordinates.

Hence one can say that the W = const surfaces constitute a system of wave surfaces describing a progressive but stationary wave motion in the qspace having a space-point dependent phase velocity, Eq. (98). Schrödinger argued: "The 'index of refraction' is proportional to the reciprocal of [(93)], and is dependent on the position but not on the direction. The q-space is thus optically nonhomogeneous but is isotropic. The elementary waves are 'spheres,' though of course—let me repeat expressly once more—in the sense of the line element [(94)]."⁽²⁸³⁾ Schrödinger emphasized, in particular, that the "function of action W plays the part of the *phase* of our wave system" and that "the Hamilton–Jacobi equation is the expression of Huygens' principle."⁽²⁸⁴⁾ Especially, Fermat's principle "led directly to Hamilton's principle in the form given by Maupertuis (where the time integral is to be taken with the usual grain of salt, i.e., $\Im + V = E = \text{const}$, even during variation),"⁽²⁸⁴⁾ since the formal relation

$$\delta \int_{P_1}^{P_2} \frac{ds}{u} = \int_{P_1}^{P_2} \frac{\sqrt{2(E-V)}}{E} \, ds = \frac{1}{E} \int_{t_1}^{t_2} 2\Im \, dt = 0 \tag{99}$$

holds. Schrödinger concluded: "The 'rays', i.e., the orthogonal trajectories of the wave surfaces, are therefore the *paths* of the system for the value E of energy, in agreement with the well-known system of equations

$$p_k = \frac{\partial W}{\partial q_k} \qquad [(100)]$$

which states that a set of system paths can be derived from each special function of action, just like a fluid motion from its velocity potential."⁽²⁸⁴⁾

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In a footnote, added to the last statement, Schrödinger quoted for support a paper that Einstein had presented to the German Physical Society in Berlin on May 11, 1917.⁽²⁸⁵⁾In that paper Einstein had discussed the quantum conditions for several degrees of freedom, as proposed shortly before by Arnold Sommerfeld, Paul Epstein, and Karl Schwarzschild, on the basis of the Hamilton–Jacobi theory. He had especially attempted to give the time-independent characteristic functions of Hamilton, which he called J^* (=J+Et, with J the time-dependent function, E the energy, and t the time) and which satisfied the equations

$$\frac{\partial J^*}{\partial E} = t - t_0 \tag{101}$$

$$\frac{\partial J^*}{\partial q_i} = p_i \tag{102}$$

an intuitive ("anschauliche") interpretation. For that purpose, he further assumed that J^* represented a potential function, implying the equation

$$\frac{\partial p_i}{\partial q_k} - \frac{\partial p_k}{\partial q_i} = 0 \tag{103}$$

for all *l* degrees of freedom of the system.⁽²⁸⁶⁾ The "anschauliche" interpretation was the following: Einstein had assumed in the coordinate space an (l-1)-dimensional surface and associated with each point q_i of this surface the corresponding momentum coordinate p_i ; if the p_i on the surface are continuous functions of the q_i , then the dynamical paths of the system (described by the Hamilton–Jacobi equation) filled continuously the phase-space or some part of it, and through each point passed a definite path with a definite p_i ; thus the p_i constituted a vector field in the coordinate space. Einstein had finally discussed the Sommerfeld–Epstein quantum conditions in this "anschauliche" interpretation, finding that they were equivalent to the condition that "the integral $\int \sum_{i=1}^{i} p_i dq_i$, extended over all closed curves of the rational q_i -space, that can be continuously transformed into each other, must have the same value."⁽²⁸⁷⁾

Schrödinger had read Einstein's paper and had quoted it recently in an investigation on the energy levels of the ideal monatomic gas.⁽²¹³⁾ The same had been done by Louis de Broglie in his thesis,⁽²⁸⁸⁾ which had initiated Schrödinger's path to wave mechanics. De Broglie's reference to Einstein's paper reminded Schrödinger of the usefulness of Einstein's proposal. He now declared: "The framing of the quantum conditions here is the most akin, out of all older attempts, to the present one."⁽²⁸⁴⁾ The reason for Schrödinger's statement is obvious. Besides the fruitful application by de

Broglie in connection with the phase waves, ⁽²⁸⁹⁾ Einstein's whole argumentation showed many similar points to those of Schrödinger's in Section 1 of the second communication on quantization as an eigenvalue problem: the emphasis on the Hamilton–Jacobi methods, even the same use of timedependent and time-independent Hamilton–Jacobi equations⁽²⁹⁰⁾; the introduction of the vector field p_i , through Eq. (102)—which is of course nothing other than Schrödinger's equation (100)—and its "anschauliche" interpretation (in certain cases) as a potential field. But Einstein provided yet another even more important key stimulus in a later publication, namely his second paper on the quantum theory of ideal gases.⁽¹⁹⁴⁾

In his second publication on the ideal gas theory mentioned above, Einstein had, after presenting a sketchy outline of Louis de Broglie's matter waves, spoken about the possibility of connecting "with every process of motion of an undulatory field" (*"mit jedem Bewegungsvorgang ein undulatorisches Feld*"),⁽²⁹¹⁾ or as Schrödinger wrote later, he had posed the question "of searching for an undulatory mechanics."⁽²⁹²⁾ Actually, Einstein had not explicitly put forward the task, but had rather raised the question of "proving this undulatory field—whose physical nature is so far still in the dark—by the phenomena of motion corresponding to it."⁽²⁹¹⁾ For example, he had called for diffraction phenomena when gas molecules pass through small slits provided their wavelength, according to de Broglie, i.e.,

$$\lambda = \frac{h}{mv} \tag{104}$$

(with *m* and *v* denoting the mass and velocity of the molecules, respectively, and *h* Planck's constant) is comparable to the slit dimensions.⁽²⁹³⁾

In Schrödinger's opinion, the task of developing a real undulatory mechanics had not yet been solved by any of the mechanical-optical analogies suggested so far. He therefore emphasized in his second communication: "We must regard the [present] analogy as one between mechanics and *geometrical* optics, and not physical or *undulatory* optics."⁽²⁸⁴⁾ Basically, the idea of "rays," for which Hamilton had worked out his analogy, as well as Fermat's principle, belonged to geometrical optics; further, the *W*-surfaces and the paths of the mechanical systems were only loosely related, because the latter—say a mass point—does not move with the velocity *u*, given by Eq. (98), but rather with a velocity *v* derived directly from Eq. (94), namely,

$$v = \frac{ds}{dt} = \sqrt{2\Im} = \sqrt{2(E - V)} \tag{105}$$

The inequality $v \neq u$ follows naturally from the physical meaning of the W-surfaces: (i) the point velocity of the system is great when grad W is great,

hence the W-surfaces are crowded in the configuration space and their phase velocity is small; (ii) since W is the time integral of the Lagrange function, $\int_{t_1}^{t_2} (\Im - V) dt$, it changes in the time interval dt by $(\Im - V)$, hence the image point (i.e., the moving mass point) cannot remain continuously in contact with the same W-surface.

Apart from these shortcomings, the theory developed up to now was far from being a real undulatory one. Schrödinger emphasized: "Important ideas in wave theory are amplitude, wavelength, and frequency—or, speaking more generally, the wave *form...*, to which exists no mechanical parallel; even of the wave function itself there is no mention beyond that W has the meaning of the *phase* of the waves (and this is somewhat hazy owing to the wave form being undefined)."⁽²⁹⁴⁾ He did not want to stop at that incomplete state of analogy, however, in which one invoked only geometrical or a very primitive form of undulatory optics in the limit of sufficiently small wavelengths—i.e., when the wavelengths were small compared to the dimensions of all paths, as Arnold Sommerfeld and J. Runge had already considered in 1911.⁽²⁹⁵⁾ Such a limiting treatment indicated, in his opinion, already a new, speculative conclusion, which he formulated as follows:

We know today, in fact, that our classical mechanics fails for very small dimensions of the path and for very great curvatures. Perhaps this failure is in strict analogy with the failure of geometrical optics, i.e., "the optics of infinitely small wavelengths," that becomes evident as soon as the obstacles or apertures are no longer great compared with the real, finite, wavelength. Perhaps our classical mechanics is the complete analogy of geometrical optics and as such is wrong and not in agreement with reality; it fails whenever the radii of curvature and dimensions of the path are no longer great compared with a certain wavelength, to which, in q-space, a real meaning is attached. Then it becomes a question of searching for an undulatory mechanics, and the most obvious way is the working out of the Hamiltonian analogy on the lines of undulatory optics.⁽²⁹⁴⁾

Schrödinger's answer to the question, as to whether he had already exhausted the mechanical-optical analogy, was therefore a clear "No." The task of developing a genuine undulatory optics, implicitly posed by Einstein, had not yet been attacked, much less been completed. It fell to Schrödinger to do it.

Schrödinger expounded the following physical conclusion, which he called a "physical hypothesis" or, more modestly, a "conjecture." He wrote: "The true mechanical process [in nature] is realized or represented in a fitting way by the *wave processes* in *q*-space, and not by the motion of *image points* in this space."⁽²⁹⁶⁾ If, in dynamics, one only watches out for image points, he claimed, one remains within the description of classical mechanics, which should be viewed as being as approximate a description

of mechanical processes as ray optics provides for optical phenomena. A macroscopic mechanical process can, of course, in a very good approximation, be thought to be confined to the motion of points of geometrical paths; however, "this manner of treatment loses all meaning where the structure of the path is no longer very large compared with the wavelength or indeed is comparable with it." In the latter case, Schrödinger concluded: "We *must* treat the matter strictly on the *wave equation* and not from the fundamental equations of mechanics, in order to form a picture of the manifold of the possible processes."⁽²⁹⁶⁾

These statements form the conceptual basis of undulatory mechanics, the theory that Schrödinger was striving for. In it the old, classical equations of mechanics were "just as useless for the elucidation of the microstructure of mechanical processes as geometrical optics is for explaining the phenomena of diffraction."⁽²⁹⁶⁾ The observation that the old mechanical description had provided "a certain interpretation of this microstructure"-i.e., of several atomic processes-if some new, nonmechanical conditions, such as the quantum conditions in the old quantum theory, were added, could now be easily understood from the new, undulatory point of view. It rested simply on the fact that the particular dynamical method employed-e.g., by Sommerfeld, Schwarzschild, and Epstein—was based on solving the partial differential equation of Hamilton and Jacobi, which is most intimately (of all schemes of classical dynamics) connected with "the true undulatory character of mechanical processes."(297) The old mechanical treatment, insofar as it had been successful, had to be compared with the method of applying the simple Huygens' principle in optics, "supplemented by some rules which are not intelligible in geometrical optics"⁽²⁹⁸⁾—such as Augustin Fresnel's construction of zones-to the problem of diffraction of light. Schrödinger continued: "But we inevitably became involved in irremovable contradictions if we tried, as was natural, to maintain also the idea of *paths of systems* in these processes, just as we find the tracing of the course of a light ray to be meaningless, in the neighborhood of a diffraction phenomenon."(298)

In his third notebook on "Eigensertproblem des Atoms," Schrödinger presented the following arguments. In the case where the wavelength associated with the dynamical process can be considered small compared to the curvature of the orbit, the wave motion constitutes not more than an "anschauliches" tool. He wrote: "However, if we ascribe to it [i.e., to the wave motion] a real significance and consider K to be a constant of nature, namely $h/2\pi$, then it will be, for small, strongly curved orbits—which are admitted according to point mechanics—impossible to construct those 'approximately pointlike'—solutions on the wave theory. In the domain of these orbits therefore [the old] mechanics ceases to be valid, and one must work with the wave-theoretical picture. This seems to be the case for the motions of the electrons in the atom." $^{(299)}$

While the undulatory theory did apparently resolve the essential difficulty of the previous quantum-theoretical description of microscopic, atomic processes, Schrödinger dared to apply the theory to macroscopic phenomena as well. Thus he stated in his notebook:

An interesting consequence of this conception [of undulatory mechanics] is that one also is not allowed any further, if one applies the idea strictly, to imagine the course of the motion of a macroscopic mechanical system as a sequence of sharply defined—in the mechanical sense—states of the system; rather, there exist at any instant a certain vagueness of the state of the system in the sense of classical mechanics. This vagueness is in particular of that kind that a sharply defined state of the system does not only exist at an instant of time but lasts for a little while; on the other hand, at each instant of time a small continuum of states of the system exists simultaneously, namely such states which follow on each other in the course of time, as described by classical mechanics. Evidently this must lead to the consequence that for rapid periodic motions all phases of the motion exist simultaneously.

In the published paper Schrödinger emphasized these consequences as well, although he was not so explicit as in the notes. He also stuck more closely to the "phase wave" picture derived from de Broglie's investigations. Due to his own derivations, which we have discussed and analyzed above, the phase waves define the path of a mechanical object in an unsharp manner only. He wrote:

The path of the point of exact phase agreement will completely lose its prerogative, because there exists a whole continuum of points before, behind, and near the particular point, in which there is almost as complete phase agreement, and which describe totally different "paths." [Hence he denied] real meaning to the phase of electronic motions in the atom; ... that the electron at a definite instant is to be found on any definite one of the quantum paths, specialized by the quantum conditions. [He rather claimed] that the true laws of quantum mechanics do not consist of definite rules for the single path, but that in these laws the elements of the whole manifold of paths of a system are bound together by equations, so that apparently a certain reciprocal action exists between the different paths.⁽³⁰⁰⁾

This interpretation and extension of de Broglie's phase wave concept, he observed, agreed with the ideas that had been expressed earlier by Werner Heisenberg, Max Born, Pascual Jordan, and Paul Dirac in a series of papers, and that he had also found in Niels Bohr's most recent article published in the *Naturwissenschaften*.⁽³⁰¹⁾ Those ideas stated that one must abandon the concepts of "place of an electron" or "path of an electron" in describing atomic processes, because they led to serious contradictions. While he agreed in general with such ideas, Schrödinger definitely did not want to give up altogether a space-time description in the microscopic domain, as Bohr had suggested. He wrote in the second communication: "From the philosophical standpoint, I would consider a conclusive decision in this sense as equivalent to a complete surrender. For we cannot really alter our manner of thinking in space and time, and what we cannot comprehend within it we cannot understand at all."^(302,303) So far there was no need in atomic physics for such a surrender, Schrödinger argued. The previously observed difficulties in the atomic theory were of a similar nature as those, which one would find when one tried to explain diffraction phenomena with the help of the laws of geometrical optics. Schrödinger wrote:

A person versed in geometrical optics might \cdots at last think that the *laws of* geometry are not applicable to diffraction, since he continually finds that light rays, which he imagines as *rectilinear* and *independent* of each other, now suddenly show, even in homogeneous media, the most remarkable curvatures, and obviously mutually influence one another.⁽³⁰²⁾

As wave optics solved the difficulties with optical diffraction phenomena, now undulatory mechanics might solve those in atomic physics. The situation in atoms may provide "logical *contradictiones in adjectis* [inner contradictions] in the old mechanical picture"; however, "they resolve in undulatory mechanics."⁽³⁰⁴⁾

In the beginning of the new atomic theory there stood a wave equation for the specific example of the hydrogen atom. Schrödinger had essentially guessed its structure and form; its derivation or-more adequately-connection with the dynamical equations of the old quantum theory of atomic structure (working with the Hamilton-Jacobi partial differential equation) had been rather artificial and forced. This was soon felt by Schrödinger himself who called, in the second communication, the transformation, Eq. (71), establishing the connection between the old mechanics and the wave equation for the hydrogen atom. "incomprehensible."⁽²⁷²⁾ The question now arose: Did the new formulation of the foundations of undulatory mechanics lead in a less arbitrary and artificial way to wave equations that described atomic systems and processes, notably the successful nonrelativistic hydrogen equations?

Schrödinger opened the discussion of the question by saying: "In what way now shall we have to proceed to the undulatory representation of mechanics for those cases where it is necessary? We must start, not from the fundamental equations of mechanics, but from a wave equation for q-space and consider the manifold of processes possible according to it."⁽³⁰⁵⁾ The most basic and also the safest element for establishing the wave equation was the relation for the phase velocity of propagating waves u,

Eq. (98), which—for a single mass point of mass m is proportional to $(\sqrt{2m(E-V)})^{-1}$ —could be written as

$$u = \frac{ds}{dW} \frac{dW}{dt} = \frac{E}{|\text{grad } W|} = \frac{v}{\left(\frac{2m}{h}\right)(v - V/h)}$$
(106)

with $W = -Et + W_0(x, y, z)$ and v = E/h. The phase velocity u and the mechanical point velocity v were related by the equation

$$v = \frac{dv}{d(v/u)} \tag{107}$$

The relation (106) did not suffice to establish a unique wave equation. Schrödinger admitted: "It is not even decided that it must be definitely of the second order. Only the striving for simplicity leads us to try this to begin with." $^{(305)}$

The tradition of writing second-order differential equations to describe wave phenomena in physics existed for some time. Such an equation has the form

div grad
$$\psi - \frac{1}{u^2} \dot{\psi} = 0$$
 (108)

where usually the first term represents the second-order space differential of the wave function

div grad
$$\psi = \left(\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2}\right)\psi$$
 (109)

and the second term the second time derivative, i.e.,

$$\ddot{\psi} = \frac{\partial^2}{\partial t^2} \psi \tag{110}$$

and the wave function ψ obviously depends on the three space variables x, y, z and the time variable t. The equation for the vibrating string considered in the eighteenth century had this form, as well as the equations for the electromagnetic waves derived in 1861 by James Clerk Maxwell. That, in principle, higher-order differential equations might also be used to account for wave processes, Schrödinger knew from Walther Ritz' theory of vibrating rectangles. However, as he said, "striving for simplicity led to the consideration of Eq. (108), in which, of course, "the differential operations

[involving the generalized position coordinates q_k] are to be understood with regard to the line element [(94)]." Still, Eq. (108) was not the only second-order wave equation consistent with the elementary relation, (106), for the wave velocity, Schrödinger emphasized, "for it is possible to generalize by replacing div grad ψ by

$$f(q_k)\operatorname{div}\left(\frac{1}{f(q_k)}\operatorname{grad}\psi\right)$$

where f may be an arbitrary function of the q's, which must depend in some plausible way on E, $V(q_k)$, and the coefficients of the line element."⁽³⁰⁶⁾ Again, Schrödinger had to invoke the simplicity argument, although he considered "in this case that a wrong deduction is not out of the question."⁽³⁰⁶⁾

Once Eq. (108) was taken to be the wave equation in undulatory mechanics, the assumption that the wave has a simple time dependence of the type

$$\psi = \exp(2\pi i v t) \psi_q \tag{111}$$

yielded the wave equation for the time-independent wave function ψ_q (depending only on the position coordinates q_k of the mechanical system)

div grad
$$\psi + \frac{8\pi^2}{h^2} (hv - V) \psi = 0$$
 (112)

or, when the "frequency" v was replaced by the "energy" E via Planck's equation,

div grad
$$\psi + \frac{8\pi^2}{h^2} (E - V) \psi = 0$$
 (113)

With the undulatory description of atomic systems—on the basis of the wave equation (108) or (113)—replacing the usual mechanical description, a serious difficulty threatened to arise, "on account of the multitude of solutions that such an equation possesses."⁽³⁰⁶⁾ For the purposes of describing quantum phenomena the situation looked even worse previously in the old quantum theory of atomic structure one had already had to restrict the manifold of solutions of the classical dynamical theory by the so-called quantum conditions; hence "it seems to be a bad beginning for a new attempt in this direction if the number of possible solutions has been increased rather than diminished."⁽³⁰⁷⁾

Fortunately, however, the actual examples of the undulatory theory

that Schrödinger had already considered up to now with the help of Eq. (113) showed no such unphysical variety of solutions. On the contrary, the equation "carries within itself the quantum conditions,"⁽³⁰⁷⁾ as he had noticed earlier. That is: "It distinguishes in certain cases, and indeed in those where experience demands it, of itself, certain frequencies or energy levels as those which alone are possible for stationary processes, without any further assumption, other than the almost obvious demand that, as a physical quantity, the function ψ must be single-valued, finite, and continuous throughout configuration space."(307) In the case of a one-body problem—as, for example, the nonrelativistic hydrogen atom treated in the first communication—the energy levels could be determined by a much simpler method than in the previous quantum theory of atomic structure. Instead of operating in two steps—(i) the definition of all dynamically possible paths; and (ii) discarding the greater part of these solutions and selecting a few paths by quantum conditions—one defined quantum states in the new theory "at once as the eigenvalues of Eq. (108), which carries in itself its natural boundary condition."⁽³⁰³⁾

Schrödinger did not exclude the possibility that for more general dynamical systems further conditions might be required; however, he definitely expected that these would "no longer be of such a strange and incomprehensible nature as the previous 'quantum conditions,'" but rather "will be of the type that we are accustomed to find in physics with partial differential equations as initial or boundary conditions."⁽³⁰⁷⁾

As Schrödinger emphasized, the undulatory mechanics thus allowed one to simplify the solution of problems of atomic dynamics considerably bevond the previous methods. Earlier one had to integrate the equations of motion of a given problem, then select a solution by the quantum conditions, and finally determine the energy states. The use of the Hamilton-Jacobi methods-invoked by Schwarzschild, Epstein, and Sommerfeld-had already simplified the evaluation: a real calculation of all possible orbits was avoided and the complex momentum integral was directly obtained, from which the energy states could then be derived. In undulatory mechanics one immediately attempts to get the energy states: one may begin first by solving the wave equation without the boundary and the continuity conditions, and soon find that applying the latter simplifies the solution enormously-as Schrödinger had shown, for example, in the case of the nonrelativistic hydrogen equation, where the general solution for the wave function, Eqs. (78) and (82), was then reduced to the very simple form, Eq. (86).

Moreover, it did not seem possible that more direct methods for calculating the energy eigenvalues existed or would come to exist in the future. These methods, Schrödinger wrote, should work especially in the case of the low-order eigenvalues, which were of special interest for spectroscopy and atomic physics. In conclusion, Schrödinger said:

From the idea, definitely outlined, that every problem of eigenvalues allows itself to be treated as one of maxima or minima without reference to the differential equation, it appears to me very probable that direct methods will be found for the calculation, at least approximately, of the eigenvalues, as soon as urgent need arises. At least it should be possible to test in individual cases whether the eigenvalues, *known* numerically to all desired accuracy through spectroscopy, *satisfy* the problem or not.^(309,310)

To decide this and similar problems and to complete the establishment of the new theory, he had to treat further examples of microscopic quantum systems, which he would amply do in the following weeks and months.

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- 235. There is absolutely no doubt that the story displacing the investigation on the relativistic wave equation into the summer or early fall of 1925 cannot be justified by the existing documents, especially the letter of November 3 to Einstein. We would also like to exclude the possibility that Schrödinger wrote the *Memorandum* before November 16, because, if he was then already considering the problem, he would have mentioned it to Alfred Landé rather than talk about complicated geometrical constructions with phase waves—which is clearly a preliminary step. Even if he had, by November 16, already noticed the failure of the relativistic hydrogen equation, he should have made a remark in the letter to Landé hinting at the negative result. After all, Schrödinger was never a person to hide disappointing conclusions.
- 236. D. M. Dennison, AHQP Interview, January 30, 1964.
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- 259. E. Schrödinger, Ref. 244, English translation, p. 1.
- 260. E. Schrödinger, Ref. 244, p. 362.
- 261. E. Schrödinger, Ref. 244, p. 363, footnote 1.
- 262. E. Schrödinger, Ref. 244, pp. 363-367.
- 263. E. Schrödinger, Ref. 244, pp. 363-364.
- 264. For details, see J. Mehra and H. Rechenberg, Ref. 245, Chapter III, pp. 472–473 and pp. 492–493.
- 265. E. Schrödinger, Ref. 244, p. 367; English translation, p. 5.
- 266. E. Schrödinger, Ref. 244, p. 368; English translation, p. 6.
- 267. For details, see J. Mehra and H. Rechenberg, Ref. 245, p. 473.
- 268. E. Schrödinger, Ref. 244, p. 370; English translation, p. 7.
- 269. The number of surface harmonics associated with the index n is 2n + 1, since one has to count every function of the form $P_{\pi}^{m}(\cos \theta) \exp(\pm im\phi)$, with

$$P_n^m(x) = (1 - x^2)^{m/2} \frac{d^n}{dx^m} P_n(x), \qquad m \le n,$$

where $P_n(x)$ is the spherical harmonic of index n.

- 270. E. Schrödinger, Ref. 244, p. 371.
- 271. E. Schrödinger, Ref. 244, English translation, p. 9.
- 272. E. Schrödinger, Ref. 256, p. 489; English translation, p. 13.
- 273. The name "wave mechanics" first occurred in Schrödinger's third communication: Quantisierung als Eigenwertproblem. (Dritte Mitteilung: Störungstheorie, mit Anwendung auf den Starkeffekt der Balmerlinien), Ann. Phys. (4) 80, 85-138 (1926); English translation, Ref. 244, pp. 62-101. He introduced the name there without much ado and preparation.
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- 280. E. Schrödinger, Ref. 256, p. 489 ff.
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- 286. Equation (103) was, as Einstein showed, always satisfied when the Hamilton function H of the system did not depend on the position coordinates. Clearly, the requirement is not that H does not involve the position coordinates q_k —say, via the potential V—but rather that H satisfies the equation

$$\frac{\partial H}{\partial q_i} + \sum_{k=1}^{i} \frac{\partial H}{\partial p_k} \frac{\partial p_k}{\partial q_i} = 0$$

for all *l* degrees of the system.

- 287. A. Einstein, Ref. 285, p. 90.
- 288. L. de Broglie, Ref. 218, p. 63.
- 289. Louis de Broglie used the Einsteinian formulation of the quantum conditions in Chapter III of his thesis, entitled "Les conditions quantiques de stabilité des trajectoires" ("The Quantum Conditions of the Stability of the Trajectories," L. de Broglie, Ref. 218, p. 62). He stressed in particular its invariance under coordinate transformations. If applied to the Sommerfeld treatment of Kepler orbits in atoms, and if combined with the idea of electron phase waves, it yielded the resonance condition, i.e., the length of the orbit has to be an integral multiple of the wavelength of the phase wave (*loc. cit.*, p. 65).
- 290. Einstein's J and J^* for the characteristic functions have to be replaced by Schrödinger's W—in the time-dependent and the time-independent form, respectively.
- 291. A. Einstein, Ref. 194, p. 10.
- 292. E. Schrödinger, Ref. 256, p. 497; English translation, p. 18.
- 293. Einstein had found that for thermal gas molecules the condition— λ being of the order of magnitude of the slit dimensions—could never be satisfied, while it seemed possible for hydrogen and helium molecules at very low temperatures (Einstein, Ref. 194, p. 10).
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- 304. E. Schrödinger, Ref. 299, p. 15.
- 305. E. Schrödinger, Ref. 256, p. 509; English translation, p. 27.
- 306. E. Schrödinger, Ref. 256, p. 510; English translation, pp. 27-28.
- 307. E. Schrödinger, Ref. 256, p. 54; English translation, p. 28.
- 308. E. Schrödinger, Ref. 256, p. 512; English translation, p. 29.
- 309. E. Schrödinger, Ref. 256, p. 513; English translation, p. 29.
- 310. It is interesting to remark at this point that, several months earlier, Max Born in Göttingen had contemplated a similar abbreviation of the computational methods in matrix mechanics, in order to obtain directly the energy eigenvalues of quantum mechanical systems—just as Schrödinger had done—but without immediate success. [See the three-man paper: M. Born, W. Heisenberg, and P. Jordan, Zur Quanten-mechanik. II, Z. Phys. 35, 557-615, 581 (1926)].