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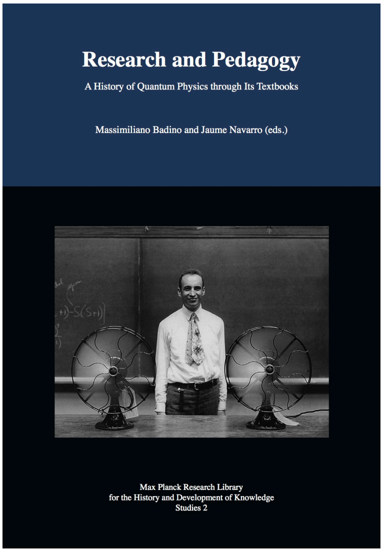
Domenico Giulini:
Max Born’s *Vorlesungen über Atommechanik, Erster Band*



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Chapter 8
Max Born's *Vorlesungen über Atommechanik, Erster Band*

Domenico Giulini

8.1 Outline

A little more than half a year before matrix mechanics was born, Max Born finished his book *Vorlesungen über Atommechanik, Erster Band*, which was a state-of-the-art presentation of Bohr-Sommerfeld quantization.[1] This book is remarkable for its epistemological as well as technical aspects. In this contribution I highlight one aspect from each of these two categories, the first concerning the role of axiomatization in the heuristics of physics, and the second concerning the problem of quantization proper before Heisenberg and Schrödinger.

Max Born's monograph *Vorlesungen über Atommechanik, Erster Band,* was published in 1925 by Julius Springer Verlag (Berlin) as volume II in the Series *Struktur der Materie* (Born 1925). The second volume of the *Vorlesungen* appeared in 1930 as *Elementare Quantenmechanik*, coauthored by Pascual Jordan, and was volume IX in the same series. In the second volume the authors attempt to give a comprehensive and self-contained account of matrix mechanics (Born and Jordan 1930). The word "elementare" in the title alludes, in a sense, to the logical hierarchy of mathematical structures and is intended to mean "by algebraic methods (however sophisticated) only," as opposed to Schrödinger's wave mechanics, which uses (non-elementary) concepts from calculus. Since, by the end of 1929 (the preface is dated 6 December 1929), several comprehensive accounts of wave mechanics had already been published,[2] the authors felt that it was time to do the same for matrixmechanics.

Here I will focus entirely on the first volume, which gave a state-of-the-art account of Bohr-Sommerfeld quantization from the analytic perspective. One might therefore suspect that the book had almost no impact on the post-1924 development[3] of quantum mechanics proper, whose 1925–26 breakthrough did not originate from further analytical refinements of Bohr-Sommerfeld theory.[4] But this would be a fruitless approach to Born's book, which is truly remarkable in at least two aspects: First, for its presentation of analytical mechanics, in particular Hamilton-Jacobi theory and its applications to integrable systems, as well as perturbation theory, and second, for its epistemological orientation. Though it is tempting

[1] As usual, I use the term "Bohr-Sommerfeld quantization" throughout as shorthand for what probably should be called Bohr-Ishiwara-Wilson-Planck-Sommerfeld-Epstein-Schwarzschild … quantization.

[2] Born and Jordan mention the following four books: Arthur Haas's *Materiewellen und Quantenmechanik* (1928), Arnold Sommerfeld's *Atombau und Spektrallinien*, Vol. 2 *(Wellenmechanischer Ergänzungsband)* (1929), Louis de Broglie's *Einführung in die Wellenmechanik* (1929), and Yakov Frenkel's *Einführung in die Wellenmechanik* (1929).

[3] The preface is dated November 1924.

[4] A partial revival and refinement of Bohr-Sommerfeld quantization occurred during the late 1950s, as a tool to construct approximate solutions to Schrödinger's equation, even for non-separable systems (Keller 1958); see also (Gutzwiller 1990). Ever since it has remained an active field of research in atomic and molecular physics.

indeed to present some of the analytic delicacies that Born's book has to offer, it is equally tempting to highlight some of the epistemological aspects, since the latter do not seem to be widely appreciated. Instead, Born's book is most often cited and praised in connection with Hamilton and Hamilton-Jacobi theory, for example in the older editions of Goldstein's book on classical mechanics.[5]

8.2 Structure of the Book

The book is based on lectures Born gave in the winter semester 1923/24 at the University of Göttingen and was written with the help of Born's assistant Friedrich Hund, who wrote substantial parts and contributed important mathematical results (e.g. the uniqueness of action-angel variables). Werner Heisenberg outlined some sections, in particular the final ones dealing with the helium atom. The text is divided into 49 sections, grouped into 5 chapters, and a mathematical appendix, which together amount to almost 350 pages. It may be naturally compared and contrasted with Sommerfeld's *Atombau und Spektrallinien I*, which has about twice the pages. As already stated, Born's text is today largely cited and remembered (if at all!) for its presentation of Hamilton-Jacobi theory and perturbation theory (as originally developed for astronomical problems). Its presentation is considered comprehensive and most concise, though today one would approach some of the material using more geometric methods (compare Arnold (1978) or Abraham and Marsden (1978)).

The chapter contents are as follows:

Introduction: Physical Foundations (3 sections, 13 Pages)
Chapter 1: Hamilton-Jacobi Theory (5 sections, 23 pages)
Chapter 2: Periodic and multiply periodic motions (12 sections, 81 pages)
Chapter 3: Systems with a single valence ('light') electron (19 sections, 129 pages)
Chapter 4: Perturbation theory (10 sections, 53 pages)

Both *Vorlesungen über Atommechanik* volumes were reviewed by Wolfgang Pauli for *Die Naturwissenschaften*. In his review of the first volume, young Pauli emphasized, in a somewhat pointed fashion, its strategy of applying mechanical principles to special problems in atomic physics. He gave the following as essential examples: Keplerian motion and the influence it receives from relativistic mass variations and external fields, general central motion (Rydberg-Ritz formula), diving orbits (*Tauchbahnen*), true principal quantum numbers of optical terms, construction of the periodic system according to Bohr, and nuclear vibrations and rotation of two-atomic molecules. He finally stresses the elaborateness of the last chapter on perturbation theory

> of which one cannot say, that the invested effort corresponds to the results achieved, which are, above all, mainly negative (invalidity of mechanics for the Helium atom). Whether this method can be the foundation of the true quantum theory of couplings, as the author believes, has to be shown by future developments. May this work itself accelerate the development of a simpler and more

[5] In the latest editions (2001 English, 2006 German) (Goldstein, Poole Jr., and Safko 2001) the authors seem to have erased all references to Born's book.

unified theory of atoms with more than one electron, the manifestly unclear character as of today is clearly pictured in this chapter.[6] (Pauli 1925, 488)

STRUKTUR DER MATERIE
IN EINZELDARSTELLUNGEN
HERAUSGEGEBEN VON
M. BORN - GÖTTINGEN UND J. FRANCK - GÖTTINGEN
II

VORLESUNGEN
ÜBER ATOMMECHANIK
VON
DR. MAX BORN
PROFESSOR AN DER UNIVERSITÄT GÖTTINGEN

HERAUSGEGEBEN
UNTER MITWIRKUNG VON
DR. FRIEDRICH HUND
ASSISTENT AM PHYSIKALISCHEN INSTITUT
GÖTTINGEN

ERSTER BAND
MIT 43 ABBILDUNGEN

BERLIN
VERLAG VON JULIUS SPRINGER
1925

Figure 8.1: Cover page.

As an amusing aside, this may be compared with Pauli's review of the second volume, which already showed considerably more of his infamous biting irony. Alluding to Born's as well as Born and Jordan's own words in the introductions to volume 1 and 2 respectively, Pauli's review starts with:

> This book is the second volume of a series, in which each time the aim and sense [*Ziel und Sinn*] of the *n*th volume is made clear by the virtual existence of the (*n*+1)st. (Pauli 1930, 602)

Having given no recommendation, the review then ends with:

> The making [*Ausstattung*] of the book with respect to print and paper is excellent [*vortrefflich*]. (Pauli 1930, 602)

[6]Translations are the author's unless otherwise noted.

Inhaltsverzeichnis.

Einleitung.

Physikalische Grundlagen.

Erstes Kapitel.

Hamilton-Jacobische Theorie.

Zweites Kapitel.

Periodische und mehrfach periodische Bewegungen.

Drittes Kapitel.

Systeme mit einem Leuchtelektron.

Figure 8.2: Table of contents.

Inhaltsverzeichnis IX

Viertes Kapitel.

Störungstheorie.

Anhang.

Figure 8.3: Table of contents.

8.3 Born's Pedagogy and the Heuristic Role of the Deductive/Axiomatic Method

8.3.1 Sommerfeld versus Born

Wilhelm von Humboldt's early nineteenth-century, programmatic vision of an intimate co-existence and cross-fertilization of teaching and research soon became a widely followed paradigm for universities in Prussia, in other parts of Germany, and around the world. And even though it is clear from experience that it cannot be a general rule that the best researchers make the best teachers, or vice versa, Humboldt's program has nevertheless proven extremely successful. In fact, outstanding examples for how to put into action Humboldt's maxim are provided by the Munich and Göttingen schools of Quantum Physics during the post-World-War-I period. Their common commitment to the "Humboldtian Ideal," with actions that speak louder than words, resulted in multiple generations of researchers and teachers of the highest originality and quality. What makes this even more convincing is the impression that this was not achieved on account of individual exceptionality; quite the contrary. Sommerfeld in Munich, for example, is well known to have had an extraordinarily fine sense for the gifts of each individual student and how to exploit these in an atmosphere of common scientific endeavor (Seth 2010). Similar things can be said of Max Born in Göttingen, though perhaps not quite as emphatically. Born's style was slightly less adapted to the non-systematic approaches of scientific newcomers, whereas Sommerfeld appreciated any and all new ideas and tricks, if only for purposes of problem solving. For Sommerfeld, teaching the art of problem solving was perhaps the single most important concern in classes and seminars (Seth 2010). Overly tight and systematic expositions were not suited to that purpose. This point was often emphasized by Sommerfeld, for example, right at the beginning of his classic five-volume "Lectures on Theoretical Physics." The first volume is called "Mechanics," not "Analytical Mechanics," as Sommerfeld stresses in a one-page preliminary note that follows the preface, because

> This name [analytical mechanics] originated in the grand work of Lagrange's of 1788, who wanted to clothe all of mechanics in a uniform language of formulae and who was proud that one would not find a single figure throughout his work. We, in contrast, will resort to intuition [*Anschauung*] whenever possible and consider not only astronomical but also physical and, to a certain extent, technical applications. (Sommerfeld 1977, Vorbemerkung)

The preface itself contains the following programmatic paragraph, which clearly characterizes Sommerfeld's approach to teaching in general:

> Accordingly, in print [as in his classes; D.G.] I will not detain myself with the mathematical foundations, but proceed as rapidly as possible to the physical problems themselves. I wish to supply the reader with a vivid picture of the highly structured material that comes within the scope of theory from a suitable chosen mathematical and physical vantage point. May there, after all, remain some gaps in the systematic justification and axiomatic consistency. In any case during my lectures I did not want to put off my students with tedious investigations of mathematical or logical nature and distract them from the physically interesting. This approach has, I believe, proven useful in class and has been maintained in the printed version. As compared to the lectures by Planck, which

> are impeccable in their systematic structure, I believe I can claim a greater variety in the material and a more flexible handling of the mathematics. (Sommerfeld 1977, v–vi)

This pragmatic paradigm has been taken over and perfected by later generations of theoretical physicists; just think of the 10-volume lecture courses by Landau and Lifshitz, which is still in print in many languages and widely used the world over.

There are many things to be said in favor of this pragmatic approach. For one thing, it takes account of the fact that developing understanding is a cyclic process. Every serious student knows that one has to go over the same material again and again in order to appreciate the details of statements, the hidden assumptions, and the intended range of validity. Often, on one’s nth iteration one discovers new aspects, in view of which one’s past understanding is revealed as merely apparent and ill-founded. Given that we can almost never be sure that this will not happen again in the future, one might even be tempted to measure one’s own *relative* degree of understanding by the number of times this has already happened in the past. From that perspective, the pragmatic approach seems clearly much better suited, since it does not pretend to the fiction of an ultimate understanding. Hence, being able to solve concrete problems sounds like a reasonable and incorruptible criterion.

However, as Thomas Kuhn pointed out long ago, well characterized (concrete) problems, also called “puzzles” by him, must be supplied by the paradigms to which working scientists adhere. But if concrete problems become critically severe, with all hope of eventual solution under the current paradigm fading, further puzzle-solving activities will, sooner or later, decouple from further progress. The crucial question when that occurs is: Where can seeds for further progress be found and how should they be cultivated?

It is with regard to this question that I see a clear distinction between the approaches of Born and Sommerfeld. Sommerfeld once quite frankly admitted to Einstein:

> Everything works out all right [*klappt*] and yet remains fundamentally unclear. I can only cultivate [*fördern*] the techniques of the quanta, you have to provide your philosophy. (Hermann 1968, 97)

Cultivating new seeds could start with establishing simple axioms in a well-defined mathematical framework. But even that might turn out to be premature. Heisenberg was one of the figures who repeatedly expressed the optimistic view that physical problems can be “essentially” solved while still detached from such a framework. In connection with his later search for a unified field theory of elementary particles, he said in the preface to his textbook on the subject:

> At the current status of the theory it would be premature to start with a system of well defined axioms and then deduce from them the theory by means of exact mathematical methods. What one needs is a mathematical description which adequately describes the experimental situation, which does not seem to contain contradictions and which, therefore, might later be completed to an exact mathematical scheme. History of physics teaches us that, in general, a new theory can be phrased in a precise mathematical language only after all essential physical problems have been solved. (Heisenberg 1967, vi)

It seems even more obvious that, in phases of paradigmatic uncertainty, little help can be expected from attempts to establish an axiomatic framework for the doomed theory. And yet, surprisingly, this is precisely what Born did, as we shall see in the next subsection.

In a letter to Paul Ehrenfest from 1925, Einstein divided the community of physicists into the "Prinzipienfuchser" and the "Virtuosi" (Seth 2010, 186).[7] Einstein grouped Ehrenfest, Bohr, and himself in the first category and named Debye and Born as members of the latter. "Virtuosity" here refers to exceptional mathematical and calculational abilities, any encounter with which results in mental depression on the side of the "Prinzipienfuchser," as Einstein concedes to Ehrenfest, who first complained about this effect. However, Einstein adds that the opposite effect exists, too.

This dichotomy is not strictly exclusive. An obvious example of someone who could with equal right be located in both camps is Wolfgang Pauli. But also Born lives in both camps and can be best described, I think, as a "Prinzipienfuchser" amongst the "Virtuosi." The principles with which he is primarily concerned arise within the attempt to find a logical basis from which the physically relevant can be deduced without ambiguity, rather than just applying clever tricks. This difference from the Sommerfeld school was once expressed by Heisenberg in an interview with Thomas Kuhn from 15 February 1963:

> In Sommerfeld's institute one learned to solve special problems; one learned the tricks, you know. Born took it much more fundamentally, from a very general axiomatic point of view. So only in Göttingen did I really learn the techniques well. Also in this way Born's seminar was very helpful for me. I think from this Born seminar on I was able really to do perturbation calculations with all the rigor which was necessary to solve such problems. (Seth 2010, 58)

Let us now turn to how Born himself expresses the heuristic value of the axiomatic method in times of uncertainty.

8.3.2 A Remarkable Introduction

One third of the way through the book, Born recalls the basic idea of 'Quantum Mechanics' in the following way (the emphases are Born's):

> Once again, we summarize the basic idea of Quantum Mechanics, as developed so far: *For a given Model [Modell] we calculate the totality of all motions* (which are assumed to be multiply periodic) *according to the laws of Classical Mechanics* (neglecting radiation damping); *the quantum conditions select a discrete subset from this continuum of motions.* The *energies* of the selected motions shall be the *true* [*wirkliche*] ones, as measurable by electron collision, and the energy differences shall, according to Bohr's frequency condition, correspond [*zusammenhängen*] with the *true* [*wirklichen*] *light frequencies*, as observed in the spectrum. Besides frequencies, the emitted light possesses the observable properties of intensity, phase, and state of polarization, which are only approximately accounted for by the theory (§ 17). These exhaust the observable

[7] As Seth already remarked in note 29 to chapter 6 of (Seth 2010), "Prinzipienfuchser" is nearly untranslatable. Existing compound words are "Pfennigfuchser" (penny pincher) and "Federfuchser" (pedant) (not "Pfederfuchser," as stated in (Seth 2010), which does not exist).

> properties of the motion of the atomic system. However, our computation assigns *additional properties* to it, namely orbital frequencies and distances, that is, the course [*Ablauf*] of motion in time. It seems that these quantities are, as a matter of principle, not accessible to observation.[8] Therewith we arrive at the following judgement [*Urteil*], that *for the time being our procedure is just a formal computational scheme* which, for certain cases, allows us to replace the still unknown quantum laws by computations on a classical basis [*auf klassischer Grundlage*]. Of these true [*wahren*] laws we would have to require, that they only contain relations between observable quantities, that is, energy, light frequencies, intensities, and phases. As long as these laws are still unknown, we have to always face the possibility that our provisional quantum rules will fail; one of our main tasks will be to delimit [*abgrenzen*] the validity of these rules by comparison with experience. (Born 1925, 113–114)

As an (obvious) side remark, we draw attention to the similarity between Born's formulations in the second half of the above cited passage and Heisenberg's opening sentences of his *Umdeutung* paper (Heisenberg 1925).

Born's book attempts an axiomatic-deductive approach to Bohr-Sommerfeld quantization. This might seem totally misguided at first, as one could naively think that such a presentation only makes sense *after* all the essential physical notions and corresponding mathematical structures have been identified. Certainly none of the serious researchers at the time believed these to have been identified for Bohr-Sommerfeld quantization, with Born being no exception, as we have just seen from the passage cited above. So what is Born's own justification for such an attempt? This he provides in his introduction to the book, where he takes a truly remarkable heuristic attitude. I found it quite inappropriate to alter his words, so I quote directly from the introduction:

> The title 'Atommechanik' of this lecture, which I delivered in the winter-semester 1923/24 in Göttingen, is formed after the label 'Celestial Mechanics.' In the same way as the latter labels that part of theoretical astronomy which is concerned with the calculation of trajectories of heavenly bodies according to the laws of mechanics, the word 'Atommechanik' is meant to express that here we deal with the facts of atomic physics from the particular point of view of applying mechanical principles. This means that we are attempting a deductive presentation of atomic theory. The reservations, that the theory is not sufficiently mature [*reif*], I wish to disperse with the remark that we are dealing with a test case [*Versuch*], a logical experiment, the meaning of which just lies in the determination of the limits to which the principles of atomic and quantum physics succeed, and to pave the way which shall lead us beyond those limits. I called this book 'Volume I' in order to express this program already in the title; the second volume shall then contain a higher approximation to the 'final' mechanics of atoms.
>
> I am well aware that the promise of such a second volume is daring [*kühn*]; since presently we have only a few hints as to the nature of the deviations that need to

[8]Here Born adds the following footnote: "Measurements of atomic radii and the like do not lead to better approximations to reality [*Wirklichkeit*] as, say, the coincidence between orbital and light frequencies."

> be imposed onto the classical laws in order to explain the atomic properties. To these hints I count first of all Heisenberg's rendering of the laws of multiplets and anomalous Zeeman effect, the new radiation theory of Bohr, Kramers, and Slater, the ensuing *Ansätze* of Kramers for a quantum-theoretic explanation of the phenomena of dispersion, and also some general considerations concerning the adaptation of perturbation theory to the quantum principles, which I recently communicated. But all this material, however extensive it might be, does not nearly suffice to shape a deductive theory from it. Therefore, the planned '2. Volume' might remain unwritten for many years to come; its virtual existence may, for the time being, clarify the aim and sense [*Ziel und Sinn*] of this book. (Born 1925, v–vi)

Born continues and explicitly refers to (and suggests the reading of) Sommerfeld's *Atombau und Spektrallinien*, almost as a prerequisite for a successful study of his own book. But he also stresses the difference, which lies in part in the deductive approach:

> For us the mechanical-deductive approach always comes first [*steht überall obenan*]. Details of empirical facts will only be given when they are essential for the clarification, the support, or the refutation of theoretical strings of thought [*Gedankenreihen*]. (Born 1925, vi)

But, Born continues, there is a second difference from *Atombau und Spektrallinien*, namely with respect to the foundations of quantum theory, where

> differences in the emphasis of certain features [*Züge*] are present; but I leave it to the author to find these out by direct comparison. As regards the relation of my understanding to that of Bohr and his school, I am not aware of any significant opposition. I feel particularly sympathetic with the Copenhagen researchers in my conviction, that it is a rather long way to go to a 'final quantum theory.' (Born 1925, vi)

It would be an interesting project to try to work out the details of the "second difference," concerning the foundations of quantum theory, by close comparison of Born's text with *Atombau und Spektrallinien*. Later, as we know, Born in principle favored the more abstract algebraic approach (Heisenberg) over the more '*anschauliche*' wave-theoretic picture, quite in contrast to Sommerfeld, who took a more pragmatic stance. Born's feeling that conceptual merit, which is marred by the semi-*anschauliche* picture of waves traveling in (high-dimensional) configuration space, should be given greater consideration is clearly reflected in the second volume, as well as in later publications, such as in the book by Herbert Green (with a foreword by Born) (Green and Born 1965) on matrix methods in quantum mechanics. This split opinion is still very much alive today, though it is clear that, in terms of calculational economy, wave mechanics is usually preferable.

Born ends his introduction by acknowledging the help of several people, foremost his assistant Friedrich Hund for his "devoted collaboration":

> Here I specifically mention the theorem concerning the uniqueness of action-angle variables which, according to my view, lies at the foundation of *today's* quantum theory; the proof worked out by Hund forms the centre [*Mittelpunkt*] of the second chapter (§ 15). (Born 1925, vii)

Hund is also thanked for the presentation of Bohr's theory of periodic systems. Heisenberg is thanked for his advice and for outlining particular chapters, like the last one on the helium atom. Lothar Wolfgang Nordheim's help with the presentation of perturbation theory is acknowledged as is the work of H. Kornfeld, who checked selected calculations. Finally, Fritz Reiche, H. Kornfeld, and F. Zeilinger are thanked for helping with corrections.

8.4 On Technical Issues: What Is Quantization?

A central concern of Born's book is the issue of quantization rules, that is: How can one *unambiguously* generalize

$$J := \oint p\, dq = nh \tag{8.1}$$

to systems with more than one degree of freedom? The history of attempts to answer this question is interesting, but also rather intricate, and involves various suggestions by Ishiwara (1915), Wilson (1915), Planck (1916), Sommerfeld (1916), Schwarzschild (1916), Epstein (1916a; 1916b), and last but not least, the somewhat singular paper by Einstein from 1917 on "The Quantum Theorem of Sommerfeld and Epstein" (Kormos Buchwald 1987–2005, Vol. 6, Doc. 45, 556–567), to which we turn below. These papers have various logical interdependencies and also differ in subtle and partial ways. Leaving aside Einstein's paper for the moment, the rule that emerged from the discussions looked innocently similar to (8.1), namely

$$J_k := \oint p_k\, dq_k = n_k h \qquad \text{(no summation over k)} \tag{8.2}$$

where $k = 1, 2, \ldots, s$ labels the degrees of freedom to be quantized, which need not necessarily exhaust all physical degrees of freedom, of which there are $f \geq s$, as we shall discuss below.[9] Here we adopt the notation from Born's book, where $(q_1, \ldots, q_f; p_1, \ldots, p_f)$ are the generalized coordinates (configuration variables) and momenta respectively. The apparent simplicity of (8.2) is deceptive though. One thing that needs to be clarified is the domain of integration implicit in the $\oint$-symbol. It indicates that the integration over q_k is to be performed over a full period of *that* configuration variable. In Sommerfeld's words, emphasis in the original:

> *Each coordinate shall be extended over the full range necessary to faithfully label the phase of the system.* For a cyclic azimuth in a plane this range is 0 to 2π, for the inclination in space (geographic latitude θ) twice the range between θ_{min} and θ_{max}, for a radial segment r [*Fahrstrahl*] likewise twice the covered interval from r_{min} to r_{max} for the motion in question. (Sommerfeld 1916, 7)

Another source of uncertainty concerns the choice of canonical coordinates for which (8.2) is meant to hold. Again in Sommerfeld's words of his comprehensive 1916 account:

> Unfortunately a general rule for the choice of coordinates can hardly be given; it will be necessary to collect further experience by means of specific examples.

[9] In (8.2) as well as in all formulae to follow, we never make use of the summation convention.

> In our problems it will do to use (planar and spatial) polar coordinates. We will come back to a promising rule of Schwarzschild and Epstein for the choice of coordinates in § 10. (Sommerfeld 1916, 6)

The rule that Epstein and Schwarzschild formulated independently in their papers dealing with the Stark effect (Epstein 1916a; Schwarzschild 1916)—compared by Epstein in (Epstein 1916b) shortly after Schwarzschild's death—is based on two assumptions. The first is that Hamilton's equations of motion

$$\dot{q}_k = \frac{\partial H}{\partial p_k}, \quad \dot{p}_k = -\frac{\partial H}{\partial q_k}, \tag{8.3}$$

for time-independent Hamiltonians $H(q_1, \ldots, q_f; p_1, \ldots, p_f)$ are solved by means of a general solution $S(q_1, \ldots, q_f; \alpha_1, \ldots, \alpha_f)$ for the Hamilton-Jacobi equation

$$H\left(q_1, \ldots, q_f; \frac{\partial S}{\partial q_1}, \ldots, \frac{\partial S}{\partial q_f}\right) = E\,, \tag{8.4}$$

where $p_k = \partial S/\partial q_k$ and $\alpha_1, \ldots, \alpha_f$ are constants of integration on which the energy E depends. Second, and most important, is that this solution is obtained by separation of variables:

$$S(q_1, \ldots, q_f; \alpha_1, \ldots, \alpha_f) = \sum_{i=1}^{f} S_i(q_i; \alpha_1, \ldots, \alpha_f)\,. \tag{8.5}$$

Note that this implies in particular that $p_k = p_k(q_k; \alpha_1, \ldots, \alpha_f)$, i.e. the k-th momentum only depends on the k-th configuration variable and the f constants of integration $\alpha_1, \ldots, \alpha_f$. This is indeed necessary for (8.2) to make sense, since the right hand side is a constant and can therefore not be meaningfully equated to a quantity that depends nontrivially on phase space. Rather, the meaning of (8.2) is to select a subset of solutions through equations for the α's. However, separability is a very strong requirement indeed. In particular, it requires the integrability of the dynamical system in question, a fact which only Einstein drew special attention to in his paper (Kormos Buchwald 1987–2005, Vol. 6, Doc. 45, 556–567), as we will discuss in more detail below. In fact, integrability is manifest once the $J_1, \ldots, J_f$ have been introduced as so-called 'action variables,' which are conjugate to some 'angle variables' $w_1, \ldots, w_f$; for then the action variables constitute the f observables in involution, i.e. their mutual Poisson brackets obviously all vanish.[10]

But even if we swallow integrability as a *conditio sine qua non*, does separability ensure uniqueness? What is the strongest guarantee of uniqueness one can hope for? Well, for (8.2) to make sense, any two allowed (by conditions yet to be formulated) sets of canonical coordinates $(q_i, p_i)_{i=1\ldots f}$ and $(\bar{q}_i, \bar{p}_i)_{i=1\ldots f}$ must be such that the (J_k/h)'s (calculated according to 8.2) are integers if and only if the $(\bar{J}_k/h)$'s are. This is clearly the case if the allowed transformations are such that among the action variables J_k they amount to linear transformations by invertible integer-valued matrices:[11]

[10] The implication of integrability for separability is far less clear (compare, e.g., Gutzwiller 1990). Classic results concerning sufficient conditions for separability were obtained by Stäckel (see Charlier 1902).

[11] Note that the inverse matrices must also be integer-valued; hence the matrices must have determinant equal to ± 1.

$$\bar{J}_k = \sum_{l=1}^{f} \tau_{lk} J_l \quad (\tau_{lk}) \in \mathrm{GL}(f, \mathbb{Z}) \,. \tag{8.6a}$$

Here $\mathrm{GL}(f, \mathbb{Z})$ is the (modern) symbol for the group of invertible $f \times f$ matrices with integer entries. The most general transformations for the angle variables compatible with (8.6a) are

$$\bar{w}_k = \sum_{l=1}^{f} \tau^{-1}_{kl} w_l + \lambda_k(J_1, \dots, J_f) \,, \tag{8.6b}$$

where the λ_k are general (smooth) functions.[12]

The task is now to carefully amend the Epstein-Schwarzschild condition demanding separability by further technical assumptions under which the transformations (8.6) will be the *only* residual ones. The solution of this problem is presented in § 15 of Born's book, where Born acknowledges essential help with this task from Friedrich Hund.

Born also states that the technical conditions under which this result for multiply periodic systems can be derived were already given in the unpublished thesis by Johannes M. Burgers (Burgers 1918), who is better known for his works on the adiabatic invariants. The arguments to show uniqueness in Burger's thesis are, according to Born, technically incomplete. The conditions themselves read as follows:

A The position of the system shall periodically depend on the angle variables $(w_1, \dots, w_f)$ with primitive period 1.

B The Hamiltonian is transformed into a function W depending only on the $(J_1, \dots, J_f)$.[13]

C The phase-space function:

$$S^* = S - \sum_{k=1}^{f} w_k J_k \,, \tag{8.7}$$

considered as function of the variables (q, w), which generates the canonical transformation $(q, p) \mapsto (w, J)$ via

$$p_k = \frac{\partial S^*}{\partial q_k} \qquad J_k = -\frac{\partial S^*}{\partial w_k} \,, \tag{8.8}$$

shall also be a periodic function of the w's with period 1.

A and B are immediately clear, but the more technical condition C is not. And, as Born remarks, A and B do not suffice to lead to the desired result. In fact, a simple canonical transformation $(w, J) \mapsto (\bar{w}, \bar{J})$ compatible with A and B is

$$\bar{w}_k = w_k + f_k(J_1, \dots, J_f) \,, \quad \bar{J}_k = J_k + c_k \,, \tag{8.9}$$

[12]Our equation (8.6b) differs in a harmless fashion from the corresponding equation (7) on p. 102 of (Born 1925), which reads $w_k = \sum_{l=1}^{f} \tau_{kl} \bar{w}_l + \psi_k(J_1, \dots, J_f)$, into which our equation turns if we redefine the functions through $\psi_k = -\sum_{l=1}^{f} \tau_{kl} \lambda_l$.

[13]We follow Born's notation, according to which the Hamiltonian, considered as function of the action variables, is denoted by W.

where the c_k are arbitrary constants. Their possible presence disturbs the quantization condition, since J_k and $\bar{J}_k$ cannot, in general, both simultaneously be integer multiples of h. Condition C now eliminates this freedom. After some manipulations the following result is stated:

> **Theorem (Uniqueness for non-degenerate systems)** If, for a mechanical system, variables (w, J) can be introduced satisfying conditions A-C, and if there exist no commensurabilities between the quantities
>
> $$\nu_k = \frac{\partial W}{\partial J_k}\,, \tag{8.10}$$
>
> then the action variables J_k are determined uniquely up to transformations of type (8.6a) [that is, linear transformations by $\mathrm{GL}(f, \mathbb{Z})$]. (Born 1925, 104)

For the proof, as well as for the ensuing interpretation of the quantization condition, the notions of *degeneracy* and *commensurability* are absolutely essential: An f-tuple $(\nu_1, \dots, \nu_f)$ of real numbers is called r-fold degenerate, where $0 \le r \le f$, if there are r but not $r+1$ independent integer relations among them, that is, if there is a set of r mutually independent f-tuples $n_1^{(\alpha)}, \dots, n_f^{(\alpha)}$, $\alpha = 1, \dots, r$ of integers, so that r relations of the form

$$\sum_{k=1}^{f} n_k^{(\alpha)} \nu_k = 0, \quad \forall \alpha = 1, \dots, r \tag{8.11}$$

hold, but there are not $r+1$ relations of this sort. The f-tuple is simply called degenerate if it is r-fold degenerate for some $r > 0$. A relation of the form (8.11) is called a commensurability. If no commensurabilities exist, the system is called non-degenerate or incommensurable.

It is clear that a relation of the form (8.11) with $n_k^{(\alpha)} \in \mathbb{Z}$ exists if and only if it exists for $n_k^{(\alpha)} \in \mathbb{Q}$ (rational numbers). Hence a more compact definition of r-fold degeneracy is the following: Consider the real numbers $\mathbb{R}$ as a vector space over the rational numbers $\mathbb{Q}$ (which is infinite dimensional). The f vectors $\nu_1, \dots, \nu_f$ are r-fold degenerate if and only if their span is s-dimensional, where $s = f - r$.

Strictly speaking, we have to distinguish between *proper* (*eigentlich*, Born) and *improper* (or contingent) (*zufällig*, Born) degeneracies. To understand the difference, recall that the frequencies are defined through (8.10), so that each of them is a function of the action variables $J_1, \dots, J_f$. A proper degeneracy holds identically for all considered values, $J_1, \dots, J_f$, (the set of which must contain at least an open interval of values around each considered value, J_k), whereas an improper degeneracy only holds for singular values of the J's. This distinction should then also be made for the notion of r-fold degeneracy: a proper r-fold degeneracy of frequencies is one that holds identically for a whole neighborhood of values $J_1, \dots, J_f$ around the considered value.

The possibility of degeneracies and their relevance for the formulation of quantization conditions was already anticipated by Schwarzschild (1916), who was very well acquainted with the more refined aspects of Hamilton-Jacobi theory, e.g. through Charlier's widely read comprehensive treatise (Charlier 1902, 1907). Schwarzschild stated in § 3 of (Schwarzschild

1916) that if action-angle variables could be found for which some of the frequencies, ν_k vanished, say $\nu_{s+1}, \dots, \nu_{s+r}$ where $s + r = f$, then no quantum condition should be imposed on the corresponding actions $J_{s+1}, \dots, J_{s+r}$. The rationale he gave for that description was that defining equation (8.10) for the frequencies showed that the energy W was independent of $J_1, \dots, J_k$. In his words (but our notation):

> This amendment to the prescription [of quantization] is suggested by the remark, that for a vanishing mean motion ν_k, the equation $\nu_k = \partial W / \partial J_k$ shows that the energy becomes independent of the variables J_k, that therefore these variables have no relation to the energetic process within the system. (Schwarzschild 1916, 550)

From that it is clear that the independence of the energy W from the J_k for which $\nu_k = 0$ is only given if the system is *properly* degenerate; otherwise we just have a stationary point in W with respect to J_k at one particular J_k value. So Schwarzschild's energy argument only justifies not quantizing those action variables whose conjugate angles have frequencies that vanish identically in the J_k (for some open neighborhood).

Now, it is true that for a r-fold degenerate system (proper or improper) a canonical transformation exists such that, say, the first $s = f - r$ frequencies $\nu_1, \dots, \nu_s$ are non-degenerate, whereas the remaining r frequencies $\nu_{s+1}, \dots, \nu_{s+r}$ are all zero (only for the particular values of J's in the improper case). The number s of independent frequencies is called the *degree of periodicity* of the system (Born 1925, 105). Hence Schwarzschild's energy argument amounts to the statement, that for *proper degeneracies* only the s action variables $J_1, \dots, J_s$ should be quantized, but not the remaining $J_{s+1}, \dots, J_{s+r}$. If the degeneracies are improper, similar systems with arbitrarily close values of the J_k would have these variables quantized, so that it would seem physically unreasonable to treat such singular cases differently, as Epstein argued in reaction to Schwarzschild (Epstein 1916b).

Born now proceeds to generalize the uniqueness theorem to degenerate systems. For this, one needs to find the most general transformations that preserve conditions A-C and, in addition, preserve the separation into s independent and r mutually dependent (vanishing) frequencies. This can indeed be done, so that the above theorem has the following natural generalization:

> **Theorem (Uniqueness for degenerate systems)** If, for a mechanical system, variables (w, J) can be introduced satisfying conditions A-C, then they can always be chosen in such a way that the first s of the partial derivatives
>
> $$\nu_k = \frac{\partial W}{\partial J_k}, \tag{8.12}$$
>
> i.e. the $\nu_1, \dots, \nu_s$ are incommensurable and the others $\nu_{s+1}, \dots, \nu_{s+r}$, where $s + r = f$, vanish. Then the first s action variables, $J_1, \dots, J_s$, are determined uniquely up to transformations of type (8.6a) [that is, linear transformations by $\mathrm{GL}(s, \mathbb{Z})$]. (Born 1925, 108)

In the next section (§ 16), Born completes these results by showing that adiabatic invariance holds for $J_1, \dots, J_s$ but not for J_k for $k > s$, even if the degeneracy is merely improper (Born 1925, 111). He therefore arrives at the following

> **Quantization rule:** Let the variables (w, J) for a mechanical system satisfying conditions A-C be so chosen that $\nu_1, \ldots, \nu_s$ are incommensurable and $\nu_{s+1}, \ldots, \nu_{s+r}$ $(s + r = f)$ vanish (possibly $r = 0$). The stationary motions of this systems are then determined by
>
> $$J_k = n_k h \qquad \text{for} \quad k = 1, \ldots, s. \tag{8.13}$$
>
> (Born 1925, 112)

Born acknowledges that Schwarzschild already proposed exempting those action variables from quantization whose conjugate angles have degenerate frequencies. But, at this point, he does not distinguish sufficiently clearly between proper and improper degeneracies. This issue is taken up again later in chapter 4, on perturbation theory, where he states that the (unperturbed) system, should it have improper degeneracies, should be quantized in the corresponding action variables (cf. Born 1925, 303).

8.4.1 A Simple System with (Proper) Degeneracies

To illustrate the occurrence of degeneracies, we present, in a slightly abbreviated form, the example of the 3-dimensional harmonic oscillator, which Born discusses in § 14 for the same purpose. Its Hamiltonian reads

$$H = \frac{1}{2m}\left(p_1^2 + p_2^2 + p_3^2\right) + \frac{m}{2}\left(\omega_1^2 x_1^2 + \omega_2^2 x_2^2 + \omega_3^2 x_3^2\right). \tag{8.14}$$

The general solution to the Hamilton-Jacobi equation is $(i = 1, 2, 3)$:

$$x_i = \sqrt{\frac{J_i}{2\pi^2 \nu_i^2 m}}\ \sin(2\pi w_i), \tag{8.15a}$$

$$p_i = \sqrt{2\nu_i m J_i}\ \cos(2\pi w_i), \tag{8.15b}$$

where

$$\nu_i = \frac{\omega_i}{2\pi} \quad \text{and} \quad w_i = \nu_i t + \delta_i. \tag{8.15c}$$

The δ_i and J_i are six integration constants, in terms of which the total energy reads

$$W = \sum_{i=1}^{3} \nu_i J_i. \tag{8.16}$$

Now, a one-fold degeneracy occurs if the frequencies ν_i obey a single relation of the form

$$\sum_{i=1}^{3} \tau_i \nu_i = 0, \tag{8.17}$$

where $\tau_i \in \mathbb{Z}$. This happens, for example, if

$$\omega_1 = \omega_2 =: \omega \neq \omega_3 \,, \tag{8.18}$$

in which case the Hamiltonian is invariant under rotations around the third axis. The energy then only depends on J_3 and the sum $J_1 + J_2$. Introducing coordinates x_i' with respect to a system of axes that are rotated by an angle α around the third axis,

$$x_1' = x_1 \cos\alpha - x_2 \sin\alpha \,, \tag{8.19a}$$
$$x_2' = x_1 \sin\alpha + x_2 \cos\alpha \,, \tag{8.19b}$$
$$x_3' = x_3 \,, \tag{8.19c}$$

under which transformation the momenta transform just like the coordinates.[14] The new action variables, J_i', are given in terms of the old (w_i, J_i) by:

$$J_1' = J_1 \cos^2\alpha + J_2 \sin^2\alpha - 2\sqrt{J_1 J_2}\, \cos(w_1 - w_2) \sin\alpha \cos\alpha \,, \tag{8.20a}$$
$$J_2' = J_1 \sin^2\alpha + J_2 \cos^2\alpha + 2\sqrt{J_1 J_2}\, \cos(w_1 - w_2) \sin\alpha \cos\alpha \,, \tag{8.20b}$$
$$J_3' = J_3 \,. \tag{8.20c}$$

As Born stresses, the J_i''s depend not only on the J_i's, but also on the w_i's, more precisely on the difference $w_1 - w_2$, which is a constant, $(\delta_1 - \delta_2)$, along the dynamical trajectory according to (8.15c) and (8.18), as it must be (since the J_i''s are constant). It is now clear that, for general α, the conditions $J_{1,2} = n_{1,2} h$ and $J_{1,2}' = n_{1,2}' h$ are mutually incompatible. However, (8.20) show that the sums are invariant

$$J_1' + J_2' = J_1 + J_2 \tag{8.21}$$

hence a condition for the sum

$$J_1' + J_2' = J_1 + J_2 = nh \tag{8.22a}$$

together with

$$J_3' = J_3 = n_3 h \tag{8.22b}$$

makes sense.

But what about coordinate changes other than just rotations? To see what happens, Born considers instead of (8.19) the transformation to cylindrical polar coordinates (r, φ, z) with conjugate momenta (p_r, p_φ, p_z) (cf. footnote 14):

$$x_1 = r\cos\varphi \qquad p_r = p_1 \cos\varphi + p_2 \sin\varphi \,, \tag{8.23a}$$
$$x_2 = r\sin\varphi \qquad p_\varphi = -p_1 r \sin\varphi + p_2 r \cos\varphi \,, \tag{8.23b}$$
$$x_3 = z \qquad p_z = p_3 \,. \tag{8.23c}$$

[14] Generally, the momenta, being elements of the vector space dual to the velocities, transform via the inverse-transposed of the Jacobian (differential) for the coordinate transformation. But for linear transformations the Jacobian is just the transformation matrix and it being an orthogonal matrix implies that its inverse equals its transpose.

The transformation equations from the old (w_i, J_i) to the new action variables (J_r, J_φ, J_z) are:[15]

$$J_r = \tfrac{1}{2}(J_1 + J_2) - \sqrt{J_1 J_2}\,\sin\bigl(2\pi(w_1 - w_2)\bigr), \tag{8.24a}$$
$$J_\varphi = 2\sqrt{J_1 J_2}\,\sin\bigl(2\pi(w_1 - w_2)\bigr), \tag{8.24b}$$
$$J_z = J_3. \tag{8.24c}$$

The total energy expressed as a function of the new action variables reads:

$$W = \nu(2J_r + J_\varphi) + \nu_z J_z, \tag{8.25}$$

where here and in (8.24) $\nu := \omega/2\pi$ and $\nu_z := \omega_3/2\pi$ (cf. 8.18). Again it is only the combination $2J_r + J_\varphi$ that enters the energy expression, and from (8.24) we see immediately that

$$2J_r + J_\varphi = J_1 + J_2. \tag{8.26}$$

Again, conditions of the form $J_r = n_r h$, $J_\varphi = n_\varphi h$, and $J_z = n_z h$ would pick out different "quantum orbits" [*Quantenbahnen*, Born] than those corresponding to $J_i = n_i h$. The energies, however, are the same.

8.5 Einstein's View

By 1917 Einstein had already taken up the problem of quantization in his long neglected[16] paper "On the Quantum Theorem of Sommerfeld and Epstein" (Kormos Buchwald 1987–2005, Vol. 6, Doc. 45, 556–567). Einstein summarized this paper in a letter to Ehrenfest dated 3 June 1917 (Kormos Buchwald 1987–2005, Vol. 8, Part A, Doc. 350, 464–6), in which he also makes a number of interesting comments, as we shall see below. For discussions of its content from a modern viewpoint see, e.g., (Gutzwiller 1990; Stone 2005).

In this paper Einstein suggested replacing the quantum condition (8.2) with

$$\oint_\gamma \sum_{k=1}^{f} p_k dq_k = n_\gamma h, \qquad \forall\gamma. \tag{8.27}$$

First of all one should recognize that here the sum forms the integrand, rather than each individual term $p_k dq_k$ as in (8.2). Second, (8.27) is not just one but many conditions, as many as there are independent paths (loops) γ along which the integrand is integrated.

Let us explain the meaning of all this in a modernized terminology. For this, we first point out that the integrand has a proper geometric meaning, since

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[15]There are two errors in Born's book in the formulae corresponding to (8.24a) and (8.24b), resulting from an erroneous factor of ν^{-1} in his formula (21) in § 14 of Chapter 2. My formulae correct Born's formulae on his p. 98.

[16]Einstein's paper was cited by de Broglie in his thesis (de Broglie 1925), where he spends slightly more than a page (pages 64–65 of Section II in Chapter III) discussing the "interpretation of Einstein's quantisation condition," and also in Schrödinger's "Quantisation as Eigenvalue Problem", where in the Second Communication he states in a footnote that Einstein's quantization condition "amongst all older versions stands closest to the present one [Schrödinger's]." However, after matrix and wave mechanics settled, Einstein's paper seems to have been largely forgotten until Keller (1958) reminded the community of its existence.

$$\theta = \sum_{k=1}^{f} p_k dq_k \tag{8.28}$$

is the coordinate expression of a global one-form on phase space (sometimes called the Liouville form),[17] quite in contrast to each individual term $p_k\, dq_k$, which have no coordinate-independent, geometric meaning. Being a one-form it makes unambiguous sense to integrate it along paths. The paths γ considered here are all closed, i.e. loops, hence the $\oint$-sign. But what are the loops γ that may enter (8.27)? For their characterisation it is crucial to assume that the system be integrable. This means that there are f (= number of degrees of freedom) functions on phase space, $F_A(q,p)$ $(A = 1,\dots,f)$, the energy being one of them, whose mutual Poisson brackets vanish:

$$\{F_A, F_B\} = 0. \tag{8.29}$$

This implies that the trajectories remain on the level sets for the f-component function $\vec{F} = (F_1,\dots,F_f)$, which can be shown to be f-dimensional tori $T_{\vec{F}}$ embedded in $2f$-dimensional phase space. From (8.29) it follows that these tori are geometrically special (Lagrangian) submanifolds: The differential of the one form (8.27), restricted to the tangent spaces of these tori, vanishes identically. By Stokes's theorem this implies that any two integrals of θ over loops γ and γ' within the same torus T coincide in value (possibly up to a sign, depending on the orientation given to the loops) if there is a 2-dimensional surface σ within T whose boundary is just the union of γ and γ'. This defines an equivalence relation on the set of loops on T whose equivalence classes are called homology classes (of dimension 1). The homology classes form a finitely generated Abelian group (since the level sets are compact) so that each member can be uniquely written as a linear combination of f basis loops (i.e. their classes) with integer coefficients. For example, if one pictures the f-torus as an f-dimensional cube with pairwise identifications of opposite faces through translations, an f-tuple of basis loops is represented by the straight lines-segments connecting the midpoints of opposite faces. Each such basis is connected to any other by a linear $\mathrm{GL}(f,\mathbb{Z})$ transformation.

Now we can understand how (8.27) should be read, namely as a condition that selects, out of a continuum, a discrete subset of tori $T_{\vec{F}}$, which may be characterized by discretized values for the f observables F_A. In light of the last remark of the previous paragraph, it does not matter which basis for the homology classes of loops one chooses to evaluate (8.27). This leads to a quantization condition independent of the need to separate variables.

What remains undecided at this stage is how to proceed in cases where degeneracies occur. In the absence of degeneracies, the torus is *uniquely* determined. It is the closure of the phase space trajectory for all times. If degeneracies exist, that closure will define a torus of dimension $s < f$, the embedding of which in a torus of dimension f is ambiguous

[17] In the terminology of differential geometry, phase space is the cotangent bundle T^*Q over configuration space Q with projection map $\pi : T^*Q \to Q$. The one-form θ on T^*Q is defined by the following rule: Let z be a point in T^*Q and X_z a vector in the tangent space of T^*Q at z, then $\theta_z(X_z) := z(\pi_*|_z(X_z))$. Here the symbol on the right denotes the differential of the projection map π, evaluated at z and then applied to X_z. This results in a tangent vector at $\pi(z)$ on Q on which $z \in T^*_{\pi(z)}Q$ may be evaluated. In local adapted coordinates $(q_1,\dots,q_f;p_1,\dots,p_f)$ the projection map π just projects onto the qs. Then, for $X = \sum_k(Y_k\partial_{q_k} + Z_k\partial_{p_k})$ we have $\pi_*(X) = \sum_k Y_k\partial_{q_k}$ and $z(\pi_*(X)) = \sum_k p_k Y_k$, so that $\theta = \sum_k p_k\, dq_k$.

since the latter is not uniquely determined by the motion of the system. This we have seen in Born's examples above. Even simpler examples would be the planar harmonic oscillator and planar Keplerian motion (cf. Arnold 1978, sec. 51). In such cases one has to decide whether (8.27) is meant to apply only to the s generating loops of the former, lower-dimensional torus or to all f of the latter, thus introducing an $(f-s)$-fold ambiguity in the determination of the "quantum orbits" [*Quantenbahnen*, Born].

The geometric flavor of these arguments is clearly present in Einstein's paper, though he clearly did not use the modern vocabulary. Einstein starts from the f-dimensional configuration space whose coordinates are defined by the q's and regards the p's as certain 'functions' on it, defined through an f-parameter family of solutions. Locally in q-space (i.e. in a neighborhood or each point) Hamilton's equations guarantee the existence of ordinary (i.e. single-valued) functions $p_k(q_1, \dots, q_f)$. However, following a dynamical trajectory that is dense in a portion of q-space the values p_k need not return to their original values. Einstein distinguishes between two cases: either the number of mutually different p-values when the trajectory returns to within a small neighborhood U around a point in q-space is finite, or it is infinite. In the latter case, Einstein's quantization condition does not apply. In the former case, Einstein's considers what he, in the letter to Ehrenfest, called the Riemannisation ("*Riemannisierung*") of q-space, that is, a finite-sheeted covering. The components p_k will then be a well-defined (single-valued) co-vector field over the dynamically allowed portion of q-space (see (Stone 2005) for a lucid discussion with pictures).

In a most interesting, one and a half page supplement added as proof, Einstein points out that the first type of motion, where q-space trajectories return with infinitely many mutually different p-values, may well occur for simple systems with relatively few degrees of freedom, e.g. that of three point-like masses moving under the influence of their mutual gravitational attractions, as was first pointed out by Poincaré in the 1890s to whom Einstein refers. Einstein ends his supplement (and the paper) by stating that, for non-integrable systems, his condition also fails. In fact, as discussed above, it cannot even be written down.

Hence one arrives at the conclusion that the crucial question concerning the applicability of quantization conditions is that of *integrability*, i.e. whether sufficiently many constants of motion exist; other degrees of complexity, like the number of degrees of freedom, do not directly matter. As we know from Poincaré's work, non-integrability occurs already at the 3-body level for simple 2-body interactions. But what is the meaning of "Quantum Theory" if "quantization" is not a universally applicable procedure?[18]

In the letter to Ehrenfest mentioned above, Einstein stresses precisely this point, i.e. that his condition is only applicable to integrable systems, and ends with a truly astonishing statement:

> As pretty as this may appear, it is just restricted to the special case where the p_ν can be represented as (multi-valued) functions of the q_ν. It is interesting that this restriction just nullifies the validity of statistical mechanics. The latter presupposes that upon recurrence of the q_ν, the p_ν of a system in isolation assume all values by and by which are compatible with the energy principle. *It seems to me, that the true [wirkliche] mechanics is such that the existence of the integrals (which exclude the validity of statistical mechanics) is already assured by*

[18]Even today this question has not yet received a unanimously accepted answer.

> *the general foundations.* But how to start??[19] (Kormos Buchwald 1987–2005, Vol. 8, Part A, Doc. 350, 465, my emphasis)

Did we just witness Einstein contemplating the impossibility of any rigorous foundation of classical statistical mechanics?

8.6 Final Comments

In his book, Born also mentions Poincaré's work and cites the relevant chapters on convergence of perturbation series and the 3-body problem in Charlier's treatise (1907), but he does not seem to make the fundamental distinction between integrable and non-integrable systems in the sense Einstein made it. Born never cites Einstein's paper in his book. He mentions the well-known problem (since Bruns 1884) of small denominators (described in Charlier 1907, chap. 10, sec. 5) and also Poincaré's result on the impossibility of describing the motion for even arbitrarily small perturbation functions in terms of convergent Fourier series. From that Born concludes it is impossible to introduce constant J_k's and hence impossible to pose quantization rules in general. His conclusion from this is that, for the time being, one should adopt a pragmatic attitude:

> Even though the mentioned approximation scheme does not converge in the strict sense, it has proved useful in celestial mechanics. For it could be shown [by Poincaré] that the series showed a *type of semi-convergence*. If appropriately terminated they represent the motion of the perturbed system with great accuracy, not for arbitrarily long times, but still for practically very long times. *From this one sees on purely theoretical grounds, that the absolute stability of atoms cannot be accounted for in this way*. However, for the time being one will push aside [*sich hinwegsetzen*] this fundamental difficulty and make energy calculations test-wise, in order to see whether one obtains similar agreements as in celestial mechanics. (Born 1925, 292–293)

Ten pages before that passage, in the introduction to the chapter on perturbation theory, Born stressed the somewhat ambivalent situation perturbation theory in atomic physics faces in comparison to celestial mechanics: On one hand, 'perturbations' caused by electron-electron interactions are of the same order of magnitude than electron-nucleus interactions, quite in contrast to the solar system, where the sun is orders of magnitude heavier than the planets. On the other hand, the quantum conditions drastically constrain possible motions and could well act as regulator. As regards the analytical difficulties already mentioned above, he comments in anticipation:

> Here [convergence of Fourier series] an insurmountable analytical difficulty seems to inhibit progress, and one could arrive at the opinion that it is impossible

[19]So hübsch nun diese Sache ist, so ist sie eben auf den Spezialfall beschränkt, dass die p_ν als (mehrdeutige) Funktion der q_ν dargestellt werden können. Es ist interesstant, dass diese Beschränkung gerade die Gültigkeit der statistischen Mechanik aufhebt. Denn diese setzt voraus, dass die p_ν eines sich selbst überlassenen Systems bei Wiederkehr der q_ν nach und nach alle mit dem Energieprinzip vereinbaren Wertsysteme annehmen. *Es scheint mir, dass die wirkliche Mechanik so ist, dass die Existenz der Integrale, (welche die Gültigkeit der statistischen Mechanik ausschliessen), schon vermöge der allgemeinen Grundlagen gesichert ist.* Aber wie ansetzen??

> to gain a theoretical understanding of atomic structures up to Uranium. (Born 1925, 282–283)

However,

> The aim of the investigations of this chapter shall be to demonstrate, that this difficulty is not essential. It would indeed be strange [*sonderbar*] if Nature barricaded herself behind the analytical difficulties of the n-body problem against the advancement of knowledge [*das Vordringen der Erkenntnis*]. (Born 1925, 282–283)

In the course of the development of his chapter on perturbation theory very interesting technical points come up, one of them being connected with the apparent necessity to impose quantization conditions for the unperturbed action variables conjugate to angles whose frequencies are *improperly* degenerate. But the discussion of this is quite technical and extraneous to Born's approach to the quantization procedure.

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