OLIVIER DARRIGOL

The origin of quantized matter waves

ONE OF THE basic ingredients of modern quantum field theory is the notion of quantized matter waves. To describe the elementary particles constituting matter such as electrons and quarks, present theorists make use of a formal quantization process applied to relativistic waves, i.e., fields obeying a relativistic wave equation. To explain the interactions between these particles, they introduce another kind of quantized field whose prime example is the electromagnetic field. The notion of a quantized electromagnetic field, the first to appear historically, may be regarded as a natural outcome of the usual quantization rules applied to a classical dynamical system formally analogous to a set of vibrating particles. In contrast, matter-wave quantization seems artificial. In this case the waves to be quantized do not have any classical meaning, and they were unknown in classical physics. Also the relevant quantization process differs essentially from the regular one: it is based on anticommutators instead of commutators. Hence, quantized matter waves represent one of the oddest notions of modern theoretical physics and one of the hardest for a critical mind to assimilate. Of course, from an axiomatic point of view the notion is justified enough as soon as its insertion within a full set of axioms has correct empirical consequences, however indirect these might be. But for many of us, full understanding occurs only if we can imagine

*Equipe REHSEIS du CNRS, 156 avenue Parmentier, 75010 Paris. I am very grateful to John Heilbron for the generous hospitality at Berkeley's OHST and for helpful critical advice. My researches in the history of quantum field theory have also enjoyed the support of Bernard d'Espagnat and Michel Paty.

The following abbreviations are used: *AP, Annalen der Physik; AHES, Archive for history of exact sciences*; AHQP, Archive for history of quantum physics; *BB*, Akademie der Wissenschaften (Berlin), *Sitzungsberichte*; CR, Académie des Sciences (Paris), *Comptes Rendus*; *PB*, Wolfgang Pauli, *Wissenschaftlicher Briefwechsel*, vol. 1, ed. A. Hermann, K. von Meyenn, and V. Weisskopf (New York, 1979), and vol. 2, ed. K. von Meyenn (New York, 1985); *PGV*, Physikaliche Gesellschaft, *Verhandlungen*; *PR, Physical review*; *PRS*, Royal Society of London, *Proceedings*, series A; *PZ, Physikalische Zeitschrift; ZP, Zeitschrift für Physik.*

ourselves as the discoverer, following the historical development. The physicist should be interested in learning the historical roots of quantized matter waves. The historian or philosopher of science should be curious to know the mechanisms of transposition and correspondence that could produce such a strange notion.

The story started in 1923 with a sketchy theory of gases proposed by Louis de Broglie. Both light and material atoms inside a cavity, he said, should be described by "wave-coupled atoms." Their state was entirely determined by a set of integers giving the number of atoms coupled to the various cavity modes. De Broglie considered this notion to be a first hint towards a new kind of reality intermediate between waves and particles.

Four years later, thanks to Pascual Jordan's efforts, the related concept of quantized matter waves was ripe: it was as well defined as the other new concepts of quantum mechanics, it was compatible with Fermi statistics, and it could be generalized to interacting matter. As a positivist champion, Jordan was not tempted to link his new formalism with a new kind of underlying reality. Instead he thought that his continuous fields of discontinuous matrices would get rid of both a continuous wave-like reality and a discontinuous atomistic reality. Clearly, Jordan's and de Broglie's new waves appeared in very different philosophical contexts. I will nevertheless establish their historical connection.

Einstein and Schrödinger were not the only theorists to be inspired by de Broglie's dissertation. Jordan and Dirac both realized how profound the statistical part of this work was, and they extracted from it a formal scheme that would fit into their own attempts to make a quantum field theory. This general scheme consists of characterizing light or gases inside a cavity by a set of integers giving the energy carried by the various cavity modes, whatever the discontinuity in question or the nature of the cavity modes may be.

The historical significance of this scheme was broader than a link between de Broglie's waves and later quantum field theory. Ehrenfest and Debye introduced it early in this century to derive the spectrum of blackbody radiation. In this case the cavity modes were electromagnetic and the discontinuity originated most plausibly from the unknown structure of real sources. The same scheme again can be found in Schrödinger's first paper on matter waves (December 1925) as an elucidation of the mysterious Bose-Einstein counting. There the cavity modes could be either light or matter waves and the discontinuity resulted from the Bohr-Sommerfeld quantization rules applied to these modes.

These examples show that permanent formal schemes allow transfers of knowledge between successive theories even if their basic concepts appear to be incommensurable, even if their inventors' world views conflict. Of course there are other mechanisms for such transfers, some of them also appearing in the history of quantized matter waves. For instance, Jordan borrowed from Einstein and de Broglie a most important heuristic principle: light and matter had to be represented by analogous concepts. This principle engendered de Broglie's waves and wave-coupled atoms; it also explains Jordan's queer insistence on quantizing the already quantal Schrödinger waves.

Again following Einstein's example, de Broglie and Jordan dealt with a most famous and universal puzzle of radiation theory: fluctuations. Einstein thought that the form of fluctuations of macroscopic quantities would give qualitative information about a future theory of radiation. In any case, a proper radiation dynamics would first have to explain the puzzling dual form of energy fluctuations. From 1909 to 1925 Einstein's fluctuation-formula had survived, because its original derivation used only empirical data and well-established theories used inside their range of validity (for instance, statistical mechanics to connect entropy and probability, and diffraction theory for gratings). The result could then be used by de Broglie and Jordan as a permanent standard to be reproduced by any good theory of radiation.

The three examples given of bridges between incommensurable theories—formal schemes, formal heuristic principles, and universal puzzles—share the same kind of permanence since, because of their formal or half-empirical nature, they can be disconnected to some extent from their conceptual environment. One of the important gifts of a good theorist might be the ability to effect this disconnection by a filtering reading of other people's work. This is required not only to inherit past knowledge but also to exploit contemporaneous but different schools of thought. For instance, Dirac could extract noncommutativity from Heisenberg's seminal matrix mechanics paper and the Copenhagen school could assimilate Schrödinger's wave mechanics without sharing the underlying philosophies. I will explain in the same way how Jordan could see a matter-*wave* quantization in a treatment of a Bose assembly, given by Dirac as a *particle*-picture point of view in his radiation paper of 1927.

Once the type of information transferable from one theory to another or from one theorist to another has been identified, it should be easier to characterize the methodological and philosophical character of the physicists or schools under analysis. A sharp contrast appears between the two main creators of quantum field theory, Jordan and Dirac, in regard to their attitudes toward quantized matter waves. Basically these two physicists understood the correspondence method in different ways. Bohr's old correspondence principle connecting quantum models with classical theory for high quantum numbers did not generally give a unique answer to radiation problems, and had to be supplemented by ingenious guessing. It is currently thought that Heisenberg's penultimate sharpening of the correspondence principle led to an unambiguous method of quantizing classical theories. In fact two notable ambiguities remained.

The first one concerns the definition of canonical conjugation. In 1927 Dirac and Jordan independently provided quantum generalizations of classical canonical transformations. Although their results have often been presented as equivalent and labelled under a common name, the transformation theory, they differ more than by notation. Dirac thought that two quantum variables would be conjugate if and only if the Poisson brackets of their canonical counterparts were so. Jordan preferred an axiomatic definition of conjugation (based on the postulate that a variable should be completely indeterminate when its conjugate is exactly known) far more general than Dirac's. For instance the a and a^+ of fermionic second quantization were canonical conjugates for Jordan. Anticommutative quantization was quite conceivable in this scheme.

The other important ambiguity in the new correspondence method originated from the fuzzy word "classical." For Dirac, who had been weaned with classical Hamiltonian mechanics, the classical correspondence basis (the thing to be quantized) could not be anything but the classical theory known to describe the physical world when Planck's constant can be neglected, more specifically the Hamiltonian corpuscular dynamics and Maxwell's electromagnetic theory. For Jordan, any kind of classical *formalism* would do (even Schrödinger's equation considered as a classical wave equation), irrespective of its relevance to the classical world.

This explains why Dirac rejected Jordan's quantized matter waves until the 1950s, and it also sheds some light on their rather unsteady reception among pre-war specialists. For a couple of years the two creators of the first relativistic quantum field theory (1929–1930), Heisenberg and Pauli, shared Jordan's enthusiasm. But when difficulties arose with the famous woeful infinities, when it was known that the relativistic features of the new theory could be expressed as well in the more usual configuration-space formulation, Pauli refused to consider the formulation via quantized matter waves more fundamental, and joined those who had deplored its oddity from the very beginning. It seemed to be safer to keep as close as possible to the classical theory, and Heisenberg, Dirac, and Pauli tried in the early 1930s to give back to electromagnetic radiation the privileged status it had in the old correspondence principle. After these temporary setbacks the discovery of new particles helped to revise conceptions of matter in a way that would ultimately favor matter-wave quantization as the natural formalism for creation and destruction processes.

1. INNOVATIONS

Louis de Broglie's "atomes couplés en onde"

At the Solvay congress held in Brussels in April 1921, Louis de Broglie's elder brother Maurice reported the experiments he had just performed on X-ray diffusion by heavy atoms. Once again the absorption of radiation by finite quanta $h\nu$ was proved, and an old paradox clearly restated: "An atom exposed to light of frequency ν emits a projectile with an energy of the order of magnitude $h\nu$, well before the incident radiation can provide this energy in the form of homogeneous spherical waves."[1] The almost unavoidable conclusion to the corpuscular nature of radiation was drawn by Maurice de Broglie and also, during the coming months, by other leading experimenters including C.D. Ellis and A.H. Compton.[2]

As his brother's collaborator in analyzing experimental results, Louis de Broglie was deeply impressed by the dual character of radiation and carefully studied the old Einstein papers on these matters. In Einstein's paper on light quanta of 1905 the corpuscular structure of light was developed from a formal analogy between gas fluctuations and radiation fluctuations. Pushing this analogy further, Louis de Broglie claimed in early 1922 that Einstein's light quanta, to be fully assimilated to atoms, had to carry a non-zero mass: "The light quantum hypothesis, if one adopts relativistic dynamics, would then lead to the idea of light atoms (of a same very small mass) with speeds depending on their energy (frequency) but all very close to c. One would explain in this way why light seems to be propagated (to within experimental precision) exactly with the speed that plays the rôle of an infinite speed in Einstein's formulas."[3] Considering blackbody radiation to be a gas of such relativistic atoms, de Broglie provided a new derivation of Wien's law. Thus perfecting the analogy between matter

1. M. de Broglie, "La relation $h\nu = E$ dans les phénomènes photoélectriques," in *Atomes et électrons,* 3rd Solvay congress (1921), *Rapports et discussions* (Paris, 1923), 89.

2. Cf. B. Wheaton, *The tiger and the shark: Empirical roots of wave-particle dualism* (Cambridge, 1983).

3. L. de Broglie, "Rayonnement noir et quanta de lumière," *Journal de physique, 3* (1922), 422–428, on 428; cf. M. Jammer, *The conceptual development of quantum mechanics* (New York, 1966), 5.3: "The rise of wave mechanics;" F. Kubli, "Louis de Broglie und die Entdeckung der Materiewellen," *AHES, 7* (1970), 26–68; and Wheaton (ref. 2).

and light, he paved his own way towards a general wave-particle duality, valid both for light and matter.

Meanwhile, to improve his approach to the blackbody puzzle, that is to say, to derive Planck's law instead of Wien's law, de Broglie used the notion of light molecules. He was probably aware of earlier proposals of this kind, obviously suggested by the following power series development of Planck's law: If $\bar{E}=Vd\nu(8\pi\nu^2/c^3)h\nu[\exp(h\nu/kT)-1]^{-1}$, then $\bar{E}=\sum_{n=1}^{+\infty}\bar{E}_n$ with $\bar{E}_n=\frac{1}{n}(8\pi\nu^2Vd\nu/c^3)nh\nu\exp(-nh\nu/kT)$ (with the usual notations of blackbody theory). The first term $\bar{E}_1$ corresponded to Wien's law, and the other terms of the series could be understood as owing to a perfect gas of n-molecules.[4]

De Broglie further noticed that the average squared energy fluctuation could be written as $\overline{\Delta^2}=\sum_n h\nu\bar{E}_n$, to be interpreted as a sum of corpuscular fluctuations due to n-molecules. We recall that the idea of inferring the structure of light from the form of statistical fluctuations calculated from the empirical value of entropy was the central idea of most of Einstein's papers on radiation. The one written in 1909 gave $\overline{\Delta^2}/\bar{E}^2 = 1/(\bar{E}/h\nu)+1/(8\pi\nu^2Vd\nu/c^3)$, the first term being characteristic of a gas of free (atomic) quanta $h\nu$, and the second one of purely classical electromagnetic waves. Comparing Einstein's with his own formula, de Broglie concluded that the interference term had to do with light molecules. These molecules or "agglomerations" could explain more than the statistical properties of light; de Broglie suggested that they might be the key to a corpuscular interpretation of interference:[5]

> In the light-quantum point of view, the interference phenomena seem to be linked to the existence of agglomerations of light atoms whose movements are not independent, but coherent. Therefore, it is natural to suppose that if the theory of light quanta manages to explain interference someday, it will require such agglomerations of light quanta.

Nevertheless, to start his synthesis between wave and particle aspects of light, de Broglie did not use this notion of light molecules. It seems very probable that the basic ingredient was the *finite* mass of light atoms: for such particles there existed a rest frame where the relation $E=h\nu$ became $m_0c^2=h\nu_0$. This last relation, linking the two

4. De Broglie (ref. 3) and "Sur les interférences et la théorie des quanta de lumière," *CR, 175* (1922), 811–813; also A. Ioffe, "Zur Theorie der Strahlungserscheinungen," *AP, 36* (1911), 534–552; G. Krutkow, "Aus der Annahme unabhängiger Lichtquanten folgt die Wiensche Strahlungsformel," *PZ, 15* (1914), 113–136; M. Wolfke, "Einsteinsche Lichtquanta und räumliche Struktur der Strahlung," *PZ, 22* (1921), 375–379; and W. Bothe, "Die räumliche Energieverteilung in der Hohlraumstrahlung," *ZP, 20* (1923), 145–152.

5. De Broglie (ref. 4), 813.

most fundamental formulas written by Einstein in 1905 ($E=h\nu$ and $E=m_0c^2$), suggested the existence of an internal periodic phenomenon of frequency ν_0. The same hypothesis could be extended to any other elementary particles, since these would differ from light quanta only by the order of magnitude of the mass m_0. Here was some hope of perfecting the analogy between matter and light.[6]

The correspondence between rest mass and internal frequency was de Broglie's starting point in the communication "Ondes et quanta," which he submitted to the Paris Academy of Sciences on September 10, 1923. The following paradox had to be solved: according to the general rules of transformation of energy and time in relativity theory, the energy in a reference frame moving at the speed βc became $E=m_0c^2(1-\beta^2)^{-1/2}$ and the internal frequency ν_0 became $\nu_1=\nu_0(1-\beta^2)^{1/2}$ so that $\nu = E/h$ was different from ν_1, and the periodic phenomenon seemed to be disconnected from the corpuscular movement. De Broglie restored the connection by noticing that the internal periodic phenomenon of apparent frequency ν_1 would remain steadily in phase with a wave of frequency ν and speed $V=c/\beta$. The most direct proof of this fact is the one given one year later in de Broglie's thesis. The vibration in the particle's rest frame is steadily in phase with the extended vibration $\sin\nu_0 t_0$ (t_0 being the particle's time). This vibration can be rewritten $\sin[\nu_0(1-\beta^2)^{-1/2}(t-\beta x/c)]$ if the particle moves with the constant speed βc along the x axis of the observer's frame, because according to the Lorentz transformation, $t_0=(t-\beta x/c)\cdot(1-\beta^2)^{-1/2}$. The new sine function represents a plane monochromatic wave of frequency $\nu = \nu_0(t-\beta^2)^{-1/2}$ and speed c/β, as claimed.[7]

In this first communication de Broglie called his waves "fictitious waves" because their speed c/β, higher than c, apparently prevented them from carrying any energy. But a very impressive result followed this hypothesis of matter waves: the Bohr-Sommerfeld quantization

6. This reconstruction is contradicted by a letter from de Broglie to Kubli (1964, in ref. 3) where the generalization of the relation $\lambda=h/p$ (known for light quanta) to material atoms is placed before the idea of a finite mass for light quanta. Such late remembrances can hardly be trusted, and I prefer a plausible reconstruction compatible with de Broglie's first writings. In these the relation $\lambda=h/p$ does not appear before the thesis, whereas massive light quanta follow immediately the first reference to the light-matter analogy (ref. 3). In any case—this is the important conceptual point—de Broglie reached his conclusions by a sharpening of this analogy.

7. L. de Broglie, "Ondes et quanta," *CR, 177* (1923), 507–510; *Recherches sur la théorie des quanta,* thesis defended at the Sorbonne on 25 Nov 1924, republished by Masson (Paris, 1963). Initially, de Broglie's precise condition was: the phase wave turning around the closed orbit quicker than the electron must catch it again in phase concordance. This condition is of course formally equivalent to a stationarity condition for the phase wave alone, but its expression relates probably to M. Brillouin's earlier notion of a directing field; cf. Wheaton (ref. 2), 291.

could be interpreted as a resonance condition for waves along closed orbits. Two weeks later de Broglie preferred the name "phase wave" to "fictitious wave," and he risked the following prediction: "An electron stream travelling through a small enough aperture would show a diffraction phenomenon. This might be the right direction to look for an experimental confirmation of my ideas."[8] If we add the analogy between Fermat's and Maupertuis' principles and the hint that future wave mechanics would be to classical mechanics what wave optics is to geometrical optics, we get the main points featured in any historical account of de Broglie's contribution to the birth of a new mechanics.

His subsequent approach to interference and to the statistics of gases was neither less profound nor less influential. It was set out for the first time to the French Academy in September 1923 under the title "Quanta de lumière, diffraction et interférences." To explain interference, it was necessary to relate in some way the movement of quanta to the corresponding phase wave. For this purpose de Broglie postulated that "in every point of its trajectory, a freely moving body follows the *ray* of its phase wave in uniform motion. . . .Therefore, I conceive the phase wave as guiding the energy displacements."[9]

A year later he noticed that this postulate (or a more precise one assimilating quanta to wave singularities) was enough to explain interference patterns: high wave-intensity regions were also high ray-density regions.[10] But in September 1923 he added two extra (in fact useless) hypotheses: "The probability of absorbing or emitting a light atom is determined by the resultant of one of the vectors of the phase waves that meet at this atom," and also, "A single phase wave carries with itself a multitude of tiny energy pieces." This last hypothesis, intended as an explanation of coherence, was of course suggested by the older notion of light molecules. It also provided a new picture for Einstein's induced quantum emission: this process had to be interpreted as adding a new quantum to a wave already carrying several quanta. Atoms belonging to the same wave were called by de Broglie wave-coupled atoms ("atomes couplés en onde") to express the kind of special correlation desired.

Although this notion was superfluous for an explanation of interference, it proved indispensable in a new derivation of Planck's law given by de Broglie in October 1923. He first determined the number $n_\nu v d\nu$ of stationary phase waves in a cavity of volume v with

8. L. de Broglie, "Quanta de lumière, diffraction et interférences" (24 Sep 1923), *CR, 177* (1923), 548–550.

9. Ibid.

10. L. de Broglie, "Sur la dynamique des quanta de lumière et les interférences" (17 Nov 1924), *CR, 179* (1924), 1039–1041.

a frequency between ν and $\nu + d\nu$; next he calculated the average number of atoms of energy $h\nu$ attached to *one* of these waves. For the first step de Broglie used an old formula of Rayleigh's:

$$n_\nu d\nu = (4\pi / UV^2)\nu^2 d\nu$$

relating the number of stationary phase waves to the phase speed V and to the group-speed U given by $1/U = d(\nu/V)/d\nu$, which is equal to βc if $V = c/\beta$. The result is $n_\nu d\nu = 4\pi p^2 dp/h^3 = d^3p/h^3$, where p denotes the momentum of the atom. Eventually an extra factor of 2 was supplied to represent the two possible states of polarization of light waves. This formula was very remarkable in itself: it showed that the density of phase waves was the same as the density of quantum states $1/h^3$ in phase space given by Planck. It bridged statistics of waves with statistics of atoms.[11]

At the second step de Broglie made an adventurous use of "Gibbs' distribution law" when giving $\exp(-nh\nu/kT)/\sum_{n'=0}^{+\infty}\exp(-n'h\nu/kT)$ as the probability that a phase wave carries the energy $nh\nu$. Accordingly, the number $N_\nu \upsilon d\nu$ of atoms with frequency within the spectral interval $d\nu$ had to be $n_\nu \upsilon d\nu\,(e^{h\nu/kT} - 1)^{-1}$.

To recover familiar results, de Broglie used two different limits: 1) $m_0 \to 0$ for the light quanta gives $n_\nu d\nu = (8\pi\nu^2/c^3)d\nu$ and $u_\nu = N_\nu h\nu = (8\pi h\nu^3/c^3)(e^{h\nu/kT} - 1)^{-1}$, which is Planck's law; 2) $\beta \to 0$ gives $N_\nu \upsilon d\nu = (\upsilon d^3p/h^3)\exp(-m_0(\beta c)^2/2kT)\exp(-m_0c^2/kT)$, which de Broglie identified as Boltzmann's law for the distribution of the kinetic energy of atoms in a perfect gas.[12]

Two important remarks have to be made about these calculations. The first one, to be found in de Broglie's thesis, concerns the enumeration of the states of a phase wave. To a given number of atoms $h\nu$ corresponds one and only one state of the wave, as follows from de Broglie's "new hypothesis:" "If two or several atoms have perfectly identical phase waves, so that one can say that they are carried by the same wave, their movement can no longer be considered entirely independent and these atoms can no longer be treated as distinct unities in probability calculations."[13]

11. L. de Broglie, "Les quanta, la théorie cinétique des gaz et le principe de Fermat" (8 Oct 1923), *CR, 177* (1923), 630–632; In this communication and in the thesis the group speed was introduced only because it appeared in Rayleigh's formula. The remark added in the thesis was that in any theory of dispersive media the group speed means also the energy speed remained accessory until the later specification of the relation between wave and particle (ref. 8).

12. De Broglie should have been worried by the wrong normalization factor, which is the signature of the misuse of Gibbs' method I will explain later.

13. De Broglie, *Recherches* (ref. 7), 103-104.

It is also clear that the exchange of an atom associated with a given wave with another atom associated with another wave of the same frequency does not change the global state of the system in de Broglie's mind: implicitly but certainly, de Broglie's atoms are *indistinguishable*. Elsewhere in his thesis one finds explicitly the idea of indistinguishability, although in a different and somewhat speculative context. There de Broglie tries to reconcile the description of a gas inside a cavity by stationary waves, on the one hand, and the kinetic molecular picture, on the other hand. Because of the randomness of the molecular motion, the number of atoms deflected per unit time from a given direction is equal to the number of atoms taking this direction after deflection. Accordingly, we can imagine atoms travelling straight from one wall to the other, as soon as we suppose, with de Broglie, that "their identity of structure dispenses us from taking their individuality into account."[14]

The other remark is about de Broglie's use of statistical mechanics. It will be shown later that with his notion of wave-coupled (and implicitly indistinguishable) atoms, a notion isomorphic to the later quantized matter waves, de Broglie had all the necessary ingredients to reach Einstein's theory of a quantum gas. He missed it mainly because of his erroneous application of Gibbs' distribution law. Also, as he was starting something new and bold, he preferred to emphasize the synthetic explanation he could give for well-known phenomena instead of focusing on new consequences of his approach; significantly, he did not repeat in his thesis the prediction concerning matter-wave diffraction.

De Broglie's subtle error may be made clear as follows. Gibbs' canonical method gives the probability p_i that the *global* system is in a state i of energy E_i when it has reached equilibrium with a thermal bath of temperature T: $p_i = \exp(-E_i/kT)/Z$, with $Z = \sum \exp(-E_i/kT)$. In de Broglie's problem, a state of the global system is given by a sequence of integers $n_1, n_2 \cdots n_\alpha, \ldots$, which I shall designate by $(n_\alpha)_\alpha$, α labelling stationary phase waves and eventually their state of polarization. In the case of *light* quanta there is no a priori restriction on the choice of $(n_\alpha)_\alpha$ so that

$$Z = \sum_{(n_\alpha)_\alpha} \exp(-\sum_\alpha n_\alpha h\nu_\alpha/kT)$$

14. Ibid., 98-99; for further information on indistinguishability cf. A. Kastler, "On the historical development of the indistinguishability concept for microparticles," in A. van der Merwe, ed., *Old and new questions in physics, cosmology and theoretical biology* (New York, 1983); M. Klein, "Ehrenfest's contributions to the development of quantum statistics," Akademie van Wetenschappen, Amsterdam, *Proceedings, 62:B* (1959), 41–62; and O. Darrigol, "La genèse du concept de champ quantique," *Annales de physique, 9*

can be factored into $Z = \Pi z_\alpha$, with

$$z_\alpha = \sum_{n_\alpha=0}^{+\infty} \exp(-n_\alpha h\nu_\alpha/kT).$$

Consequently, $p(n_\alpha)_\alpha = \Pi p_\alpha(n_\alpha)$, with $p_\alpha(n_\alpha) = \exp(-n_\alpha h\nu_\alpha/kT)/z_\alpha$. This means that one can apply the canonical method to the various stationary phase waves considered as independent systems. De Broglie's error was to suppose implicitly that this factoring was also valid for quanta of *matter*, which is not true because the constraint $\sum n_\alpha = N$, where N is the fixed number of particles, makes the summation in the expression of Z non-factorable. Taking into account this constraint just provides the results of Einstein's theory of a quantum gas. But de Broglie was trapped by the propensity of his time to apply statistical reasoning to the components of a gas instead of applying them to the whole system.[15]

Comparison to earlier theories

Even if he missed by little a theory of gas degeneracy, de Broglie still contributed greatly to the progress of quantum statistics. His statistics of matter waves, a profound consequence of his strong belief in the analogy between matter and light, was entirely new and promised a brilliant future. Apparently his statistics of light waves was less new; it resembled a much earlier representation of cavity radiation introduced by Ehrenfest and Debye. Ehrenfest's proposal of 1906 was to represent (as had Rayleigh in 1900) cavity radiation by a set of stationary modes formally analogous to harmonic oscillators, and to restrict the energy of these oscillators to integral values of h times their frequency. This hypothesis gave the easiest and most direct proof of Planck's law, without requiring a theory of the interaction process between matter and radiation (contrary to Planck's derivation of 1900). In 1910, Debye published a similar proof (without quoting Ehrenfest), which was often considered the simplest if not the most profound. De Broglie's teacher Paul Langevin liked it very much and mentioned it during the discussions at the first Solvay congress in 1911. De Broglie could not have been ignorant of it.[16]

15. A holistic definition of the states of a gas was indeed one of the issues of the dialogue between Planck and Schrödinger in 1925 about the definition of entropy; cf. P. Hanle, "The coming of age of Schrödinger: His quantum statistics of ideal gases," *AHES, 17:2* (1977), 165–192.

16. P. Ehrenfest, "Zur Planckschen Strahlungstheorie," *PZ, 7* (1906), 528–533; P. Debye, "Der Wahrscheinlichkeitsbegriff in der Theorie der Strahlung," *AP, 33* (1910), 1427–1434; P. Langevin, in *La théorie du rayonnement et les quanta,* 1st Solvay congress (1911), *Rapports et discussions* (Paris, 1912), 118–119.

But neither Debye nor Ehrenfest connected their representation of cavity radiation with Einstein's light quantum. According to Debye, there was no reason to explain the constraint on energy values of wave modes by appeal to a corpuscular nature of radiation: "As for the questions whether elementary quanta must be regarded as an intrinsic property of ether or as an intrinsic property of matter—this latter alternative should be preferred at the moment—our considerations do not provide any key; they can be understood and pursued on the basis of both hypotheses."[17] Ehrenfest made it even clearer that quantization constraints had to originate from the structure of the emitting matter:[18]

> That such [constraints] ("einschränkende Bedingungen") can be physically justified should be clear from the following remark, which I introduce, however, only as an example. Let us admit that every emission of radiation occurring in nature requires in the last analysis the participation of electrons and that these electrons always have a determinate structure. This special structure. . .is in principle sufficient to restrict the possible excitations of the natural modes of our cavity.

In short, radiation had to be considered as entirely classical (obeying Maxwell's equations), and energy quantization was nothing but an external constraint comparable, for instance, to the constraint on the total energy value in the micro-canonical ensemble.

The general hostility to light quanta prevented most theorists from connecting them with quantized waves. Einstein could have explored this direction earlier than de Broglie if he had not been restrained by other considerations: he knew from his very first paper on the subject (1905) that a gas of *independent* light quanta could lead only to Wien's law, whereas Ehrenfest-Debye's approach gave Planck's law. As later noticed by Ehrenfest and Natanson, this had to do with the fact that Planck's or Debye's energy elements were not independent, contrary to Einstein's light quanta: in other words, permutations of equal energy elements did not change the state of a collection of oscillators, whereas permutations of identical light quanta changed the (micro-) state of the gas they constituted.[19]

Since today's quantum physicist plays unselfconsciously with indistinguishable particles, it might be worth stressing that, for a classically trained physicist like Einstein or Ehrenfest, permutable energy

17. Debye (ref. 16), 1434.
18. Ehrenfest (ref. 16), 531.
19. P. Ehrenfest, "Welche Züge der Lichquantenhypothese spielen in der Theorie der Wärmestrahlung eine wesentliche Rolle?" *AP, 36* (1911), 91–118, on 111; L. Natanson, "Über die statistische Theorie der Strahlung," *PZ, 12* (1911), 659–666; cf. M. Klein, *Paul Ehrenfest* (Amsterdam, 1970), 255.

elements could not be identified with real particles. Indeed, two indestructible particles, even if they were perfectly identical, could always be distinguished by mentally following their tracks. Some new impetus was required to cross this logical barrier. Such was the realization that for Einstein's light quanta and for Ehrenfest-Debye's elements the containers were the same: as proved by de Broglie, and later implicitly by Bose, there was a one-to-one correspondence between stationary light waves and quantum states of light atoms inside a cavity.

Through his general connection between wave and particles, Louis de Broglie not only derived this correspondence but was also able to hang groups of atoms on stationary waves. This half-corpuscular scheme was formally equivalent to the old Ehrenfest-Debye scheme, but pointed toward a new synthesis. When de Broglie defended his thesis at the Sorbonne on November 25, 1924, he presented it as a first step toward the identification of a new kind of reality between waves and corpuscles. As he commented much later, "While I was proceeding so boldly into entirely unexplored ground, I was convinced—texts that I published at that time show it clearly—that one had to reach a *true* synthesis of the wave and corpuscle concepts, saving the precise picture of the physical realities always attributed to these two concepts." Further determination of the reality underlying this synthesis was hopeless, as the later history of wave mechanics proved. It was therefore a good strategy to leave this reality fuzzy, as de Broglie termed it in the last words of his thesis: "I intentionally kept vague the definition of the phase wave and of the periodic phenomenon it would somehow express, and also the definition of the light quantum. Therefore, the present theory should be taken as a form whose physical content is not entirely given, rather than as a homogeneous, definitively constituted doctrine."[20]

Some inspired readers of de Broglie's thesis

Einstein

In 1924 Satyendra Nath Bose, an unknown physicist from Dacca, thought he had reached an interesting new derivation of Planck's law and fortunately dared to mail it to Einstein. In his letter he announced his main result in the following words: "You will see that I have tried to deduce the coefficient $8\pi\nu^2/c^3$ in Planck's law independent of the classical electrodynamics, only assuming that the ultimate

20. De Broglie, *Recherches* (ref. 7), preface of the 1963 edition, 4.

elementary region in the phase space has the content h^3."[21] Bose was trying to imitate the theory of a perfect gas that could be found in Planck's lectures on thermal radiation. In this method, one had first to divide the $(\vec{r},\vec{p})$-space into cells of volume h^3, and then to evaluate the number of distributions of a given number of particles among the cells. For a spectral interval $d\nu$, corresponding to a momentum interval dp via the relation $p=h\nu/c$, the number of cells had to be $A=2d^3p/h^3=8\pi p^2dp/h^3=8\pi\nu^2d\nu/c^3$, the factor 2 representing the two possible states of polarization. Now, according to Bose, a macro-state of the gas was characterized by the numbers p_i of cells containing i quanta, and the number W_ν of micro-realizations of this state was $A!/(p_0!p_1!\cdots p_i!\cdots)$, a formula to which I will return. Finally, to get the entropy leading to Planck's law, one had to calculate the value of the sequence of $(p_i)_i$ maximizing the product of the various W_ν for all spectral intervals.[22]

Einstein reacted enthusiastically: he saw that his light quantum hypothesis, by means of Bose's calculation, could lead to Planck's law, and not only, as previously believed, to Wien's law. He translated the paper immediately and sent it to the *Zeitschrift für Physik* with the following comment: "Bose's derivation of Planck's formula is in my opinion a significant breakthrough. The method used here also provides the quantum theory of an ideal gas, as I will show somewhere else."[23] As Bose used only methods of gas theory, his calculation scheme could indeed be adapted to material atoms instead of light quanta. A few days later Einstein communicated to the Berlin Academy the resulting theory of a monoatomic gas. Nernst's two important requirements were satisfied: vanishing entropy and gas degeneracy $(\partial p/\partial T\,|_v\rightarrow 0)$ at vanishing temperatures.[24]

At the end of this communication, Einstein brought up without solving the following paradox, similar to Gibbs' paradox: according to the new theory, the entropy of a mixture of two different gases—no matter how small the differences between molecules—was different from the entropy of a single gas filling the same volume with the same total number of particles. Einstein failed to connect this paradox with Bose's formula for W_ν, which shows he was no more aware than Bose of the indistinguishability involved in this formula.

21. Bose to Einstein, 4 Jun 1924 (Einstein Estate, Jerusalem), quoted in Jammer (ref. 3), 248.
22. S.N. Bose, "Plancks Gesetz und Lichtquantenhypothese," *ZP, 26* (1924), 178–181.
23. Ibid.
24. A. Einstein, "Quantentheorie des einatomigen idealen gases," *BB* (10 Jul 1924), 261–267.

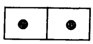
Ehrenfest had given more thought to the problems arising when numbering complexions for light quanta in the same way as for gas molecules. After seeing Bose's paper and knowing about Einstein's theory, he immediately smelled some hidden trick. He knew that any consistent statistical treatment of an assembly of *independent* light particles would lead to Wien's law instead of Planck's. Einstein acknowledged the point in his next communication to the Berlin Academy:[25]

> Mr. Ehrenfest and other colleagues have made the following objection to Bose's theory of radiation and to my analogous theory of ideal gases: in these theories the quanta, or the molecules, are not treated as statistically independent objects, although this circumstance was not made explicit in our communications. The formula expresses indirectly a certain hypothesis of a mutual influence between molecules, presently quite mysterious, which conditions the equiprobability of the cases here defined as complexions.

So Einstein recognized the difficulty, but did not consider it to be fatal for his approach to gas theory. His procedure had to be valid, if only because it was consonant with the spirit of the light-quantum hypothesis, that is, with the analogy between light quanta and atoms. For him the reality of atoms was not endangered by Bose's mysterious counting. But nobody was able to find the puzzling interaction that would save this conception, not even Bose when Einstein charged him with this task.[26]

What made Bose's happy error possible? It is easy to see in a simple example that the combinatory formula $W = A!/(p_0!p_1! \cdots p_i! \cdots)$ implies indistinguishability (according to Gibbs' old terminology) for quanta. The simplest non-trivial case is $A=2$, $p_0=0$, $p_1=2$ and $p_i=0$ for $i \geq 2$. The various configurations are:

- in the case of indistinguishability:

 , conforming to W = 1

25. A. Einstein, "Quantentheorie des einatomigen idealen gases. 2. Abhandlung," *BB* (8 Jan 1925), 3–14, on 5.

26. Cf. J. Mehra and H. Rechenberg, *The historical development of quantum theory,* 4 vols., (New York, 1982), 1:2, on 571, note 872.

• in the case of distinguishability:

Considering that the classical meaning of the word particle forbids indistinguishability, one must conclude that Bose borrowed his combinatorics from some other work without seeing that it did not suit his own problem. Very probably the source was the second edition (1913) of Planck's lectures on thermal radiation or its English translation (1914), where Planck characterized the state of an assembly of A resonators of frequency ν by the sequence of numbers p_i giving the number of resonators carrying i energy-elements $h\nu$.[27]

The kind of counting implied by the formula $W_\nu = A!/(p_0! \cdots p_i! \cdots)$, or by the previous one Planck had used in his derivation of 1900, was certainly clear to him, because he gave tables with concrete examples of distributions of energy-elements, and it was harmless because such abstract things as energy-elements could be indistinguishable. But Bose used the same formula for distributing light-quanta over cells instead of energy-elements over resonators. The confusion was easy to make and indeed had appeared in two earlier papers written by M. Wolfke in 1913 and R. Emden in 1921.[28]

In spite of Ehrenfest's criticism, Einstein went on with his new gas theory. After important empirical predictions like the condensation effect, he derived a formula for the average squared fluctuation of the number n_E of particles inside a partial volume v and an energy interval dE, using exactly the same type of reasoning as he had for blackbody radiation in 1909. The result was $\overline{\Delta n_E^2}/\bar{n}_E^2 = 1/\bar{n}_E + 1/z_E$, with $z_E = (2\pi v/h^3)(2m)^{3/2}E^{1/2}dE$ for the number of cells inside the energy range dE. The existence of a second new term suggested, by analogy with radiation fluctuation, an interpretation in terms of wave interference. Light and matter had to share the same kind of wave-particle duality.[29]

27. This characterization was introduced into blackbody theory by H.A. Lorentz in "Alte und neue Fragen der Physik," *PZ, 11* (1910), 1234–1257; it paralleled closely the one used in 1877 by Boltzmann for gases. Cf. T.S. Kuhn, *Blackbody theory and the quantum discontinuity, 1894–1912* (Oxford, 1978), on 48, 102.

28. M. Wolfke, "Zur Quantentheorie," *PGV, 15* (1913), 1123–1129, 1215–1218; R. Emden, "Über Lichtquanten," *PZ, 22* (1921), 513–517.

29. Einstein (ref. 25).

At that time Einstein had received de Broglie's dissertation, in which he could find the same far-reaching conclusion: if matter and light were analogous, they had to bear the same kind of duality. Einstein could persuade himself that the second term in the fluctuation formula was indeed the one given by interference of de Broglie's waves. Praising the little-known French theorist, he acquainted several physicists with his work. Among them were the professor of theoretical physics at Göttingen Max Born, his colleague, the professor of experimental physics James Franck, and his young student Walter Elsasser, who realized that some experimental results by Davisson and Kunsman and by Ramsauer could be explained as matter-wave effects well before the first experimental proof of electron diffraction (March 1927).[30]

Schrödinger

Another famous physicist, Erwin Schrödinger, got a copy of de Broglie's dissertation. During the years 1924–25 he had given much thought to the quantum theory of gases.[31] According to Sackur, Tetrode, and Planck (1911–1913), quantum theory allowed a determination of the additive constant in the statistical entropy of gases by fixing the size of the quantum cells (h^3) into which the molecules had to be distributed. Although these constants did not have any empirical meaning for a single gas, they were linked to the equilibrium constants of chemical reactions by the well-known Gibbs formula $\Delta H_0 - T\Delta S_0 + RT \ln K_p = 0$, and for this reason were called "chemical constants" by Nernst.[32]

This connection was meaningful and successful, but only if individual entropies were extensive, as required by Gibbs' proof of his formula. Extensivity could be achieved by subtracting somewhat artificially $k \ln N!$ from the entropy of a single gas of N particles, that is, by dividing the probability of a macrostate by $N!$. In 1916 Planck explained this division by the indistinguishability of identical

30. Cf. A. Russo, "Fundamental research at Bell Laboratories: The discovery of electron diffraction," *HSPS, 12:1* (1981), 117–160; Elasser's note was sent on 18 Jul 1925 to *Naturwissenschaften.*

31. Cf. Jammer (ref. 3); V.V. Raman and P. Forman, "Why was it Schrödinger who developed de Broglie's ideas?" *HSPS, 1* (1969), 291–314; and L. Wessels, "Schrödinger's route to wave mechanics," *Studies in the history and philosophy of science, 10* (1979), 311–340.

32. O. Sackur, "Die Anwendung der kinetischen Theorie der Gase auf chemische Probleme," *AP, 36* (1911), 958–980; H. Tetrode, "Die chemische Konstante der Gase und das elementare Wirkungsquantum," *AP, 38* (1912), 434–442, and *AP, 39* (1912), 255–256; M. Planck, *Vorlesungen über die Wärmestrahlung,* 2nd ed. (Leipzig, 1913), on 130–131. Cf. E. Hiebert, *Dictionary of scientific biography,* s.v. "Nernst."

molecules. Ehrenfest, who thought the notion absurd, rather claimed with his student Viktor Trkal that there was no fundamental reason for the entropy of a homogeneous gas to be extensive, since (for implicitly distinguishable particles) the mixture of two identical portions of the same gas could not be achieved by a reversible process. He then gave a new demonstration of the mass-action law dealing directly with the number of complexions for the *mixture*, without any need of extensivity. Only observable entropy variations had to be extensive, not absolute entropies.[33]

In 1924 neither Planck nor Ehrenfest had moved from their original positions. Schrödinger supported Ehrenfest, until he knew about Einstein's attractive new gas theory.[34] In July 1925 he accepted Planck's opinion that "during the counting of permutations the molecules have been overly individualized, whereas the exchange of similar molecules should have no effect on the counting."[35] But he claimed, contrary to Planck, that the division by $N!$ did not entirely express indistinguishability, that one had to switch to Bose-Einstein counting. Hence, Schrödinger adopted Einstein's gas theory although he felt that the mysterious interaction evoked by Einstein might elude further research since it was related to an aberrant indistinguishability. "For the moment," he deplored, "there is not a single possibility to understand the remarkable kind of interaction between molecules that would lead to the elimination of the permutation number from the statistical calculation."[36] But the urge of an explanation was pressing: "To consider this new statistics as something primary, not further explicable, conflicts obviously with natural feeling."[37]

When in autumn 1925 Schrödinger read de Broglie's dissertation, he understood that a profound conceptual change was needed to justify the Bose-Einstein counting: the corpuscular picture of a gas had to be dropped in favor of de Broglie's waves. These could explain not only an interference term in the fluctuation formula, but also the whole new gas theory. Bose's article had been a historical regression; but Bose's letter reached Einstein before de Broglie's dissertation. Schrödinger wrote in his article, "Zur Einsteinschen Gastheorie," sent

33. M. Planck, "Über die absolute Entropie einatomiger Körper," *BB* (1916), 653–667; P. Ehrenfest and V. Trkal, "Ableitung des Dissociationsgleichgewichts aus der Quantentheorie und darauf beruhende Berechnung chemischer Konstanten," *AP, 65* (1921), 609–628.

34. E. Schrödinger, "Gasentartung und freie Weglänge," *PZ, 25* (1923), 41–45.

35. E. Schrödinger, "Bemerkungen über statistische Entropiedefinition beim idealen Gas," *BB* (23 Jul 1925), 434–441, on 440.

36. Ibid.

37. E. Schrödinger, "Zur Einsteinschen Gastheorie," *PZ, 27* (1926), 95–101 (received 25 Dec 1925).

for publication late in 1925:[38]

> The true meaning of Einstein's gas theory is that a gas must be conceived as a system with linear proper oscillations similar to a volume of radiation or to a solid. . . .Following de Broglie one gets the frequency spectrum of the gas by quantizing the stationary phase waves which are allowed in the volume v, much as in the well-known method of Jeans and Debye.

Formally, this statement can be interpreted as an adoption of de Broglie's wave-coupled atoms. But there is a conceptual difference: Schrödinger connected de Broglie's method explicitly to Debye's (contrary to de Broglie) and used Debye's terms, "the quantization of stationary waves," instead of de Broglie's "wave-coupling." Clearly he leaned toward a pure wave-like reality instead of a mixture of two kinds of realities. His early discussion, in the same article, of the problem of the spreading of a wave packet is another indication of this attitude, and marks also the historical shift of his interest from statistical problems to wave mechanics. During the following months he focused on the theory of atomic spectra and left aside the idea of attaching several quanta to a single wave. But his profound insight that the objects of the Bose-Einstein statistics were "energy excitation states instead of true individuals" was not lost forever.[39]

Dirac

A British beginner in quantum theory, Paul Dirac, was also impressed by de Broglie's theory of gases. He had heard about his thesis, probably by reading the extensive summary submitted in February 1924 to the *Philosophical Magazine* by Dirac's supervisor, Ralph Fowler.[40] On August 4, 1925, Dirac gave a talk at the Kapitza club entitled: "Bose's and Louis de Broglie's deduction of Planck's law," where he made the following comment:[41]

> It is a disadvantage of the present theory that the cells play so important a part in it. . . .One can get over the difficulty by adopting the point of view, first proposed by de Broglie, that each particle is associated with a wave, and letting the waves play the part of the cells in the previous theory. Several particles may be associated with the same wave. This point of view is possible only because it turns out that the number

38. Ibid.
39. Ibid., 95, 101.
40. L. de Broglie, "A tentative theory of lightquanta," *Philosophical Magazine, 47* (1924), 446–458.
41. Kapitza Club, "Minutes," AHQP; Cf. P. Dirac, MS draft of a critical presentation of Einstein-Bose statistical mechanics, with notice of de Broglie's work (AHQP).

> of waves associated with a given region of phase space is equal to the number of cells into which that region of phase space was divided in the previous theory. Fundamental assumption of present statistical mechanics now reads: all values for the numbers of atoms or light-quanta associated with a given wave have the same a priori probability.

One can see that Dirac kept closer to de Broglie than Schrödinger: he "adopted" the idea of several particles associated with the same wave, and, contrary to Schrödinger, did not give up the concept of particle in favor of the concept of mechanical wave. But he did not see in de Broglie's idea a possible germ for a new mechanics. On the contrary, he severely criticized de Broglie's guiding principle, the analogy between matter and light. In July 1924 he wrote:[42]

> For the discussion of equilibrium problems, quanta of radiation can not be regarded as very small particles moving with very nearly the speed of light. There are two important points in which this picture is inadequate. In the first place the small particles could not (according to ordinary statistical theory) have any stimulating effect on processes by which they are emitted, and they should therefore be distributed in momentum according to Maxwell's law, which is the same as being distributed in energy (or frequency) according to Wien's radiation law. Secondly the concentration of quanta in thermodynamical equilibrium is not arbitrary, as is the case with all kinds of material particles, but is a definite function of temperature.

Dirac's first critical point fell with the application of Bose's statistics to massive particles, but the second one, expressing the dissymmetry between convertible radiation and conserved matter, was admitted by most physicists until the discovery of new fundamental particles.

Dirac was very little influenced by de Broglie's ideas when, in 1925–26, he built his own quantum mechanics. He preferred to start with the Hamiltonian formalism in which he was already an expert, and he favored the underlying basic classical picture of a set of corpuscles. Not before 1927 and only for electromagnetic radiation did Dirac come back to a notion of a quantized field formally and accidentally related to de Broglie's wave-coupled atoms.

Jordan

Even restricted to its formal skeleton, the notion of wave-coupled atoms had awakened only a very transitory interest on the part of Schrödinger and Dirac. The one who adopted the formal scheme and kept it in mind all through his quantum researches was Pascual

42. P. Dirac, "The conditions for statistical equilibrium between atoms, electrons and radiation," *PRS, 106* (1924), 581–596, on 594.

Jordan. He was the first, in November 1925, to publish an elucidation of Bose's mysterious counting of light-quanta by field quantization (one month before Schrödinger). I will show later that he was already willing to treat material particles on the same footing. Even if he did not publish such a generalization at that time, he believed that he had done so when interviewed in 1963 by Thomas Kuhn![43] His memory was not much more accurate when he wrote in 1927:[44]

> Of course Einstein's theory of light quanta was fundamental and introduced the most important wave particle dualism into physics for the first time. For the several-body problem Einstein's papers on gas degeneracy come into consideration. There, in connection with de Broglie he compared an ideal gas with a system of quantized waves in three-dimensional space.

In spite of the wrong attribution of de Broglie's statistical work to Einstein, this quotation demonstrates sufficiently Jordan's acquaintance with its basis, the new representation of a gas in a cavity. It is even probable that he read de Broglie's dissertation or part of it in 1925. So he remembered, at least, in 1963.[45] His published work of 1925 provides further evidence of de Broglie's influence.

Pascual Jordan and field quantization

Background

Among the creators of quantum mechanics Pascual Jordan is certainly the least known, although he contributed more than anybody else to the birth of quantum field theory. Born in 1902 in Hannover, he wanted first to be a painter, like his father. But physics was more fitted to quench his thirst for fundamental knowledge including biology or pure philosophy. In 1919 he learned relativity from Moritz Schlick's book, and in 1921–22 he was thrilled by Mach's *Mechanik* and *Prinzipien der Wärmelehre*. Throughout his life he considered himself a disciple of Mach and referred constantly to what he thought to be "the essential and decisive principle of the positivistic theory of knowledge: the limitation of scientifically sound propositions to ones that can be proved experimentally." He claimed that this principle helped him to follow Heisenberg in his rejection of unobservable quantities.[46]

43. P. Jordan, interviews by T.S. Kuhn (1963), AHQP.
44. P. Jordan and O. Klein, "Zum Mehrkörperproblem der Quantentheorie," *ZP, 45* (1927), 751–765, on 751.
45. Jordan (ref. 43), 1st session, 6.
46. P. Jordan, *Anschauliche Quantentheorie* (Berlin, 1936), viii.

Except for study of a few excellent physics books, including Sommerfeld's best-seller *Atombau und Spektrallinien*, Jordan was trained as a mathematician. At the Technische Hochschule in Hannover, physics lectures were too mediocre for him; at Göttingen he decided that classes of experimental physics were given too early in the morning. "In this way I am a physicist who never attended a physics course." More exactly, the kind of physics he first learned was very mathematical, being taught by great mathematicians like Hilbert, who gave lectures on statistical mechanics, and Courant, who requested Jordan's collaboration in writing his *Mathematische Methoden der Physik*.[47]

But Jordan's supervisor happened to be Max Born, who made physics at Göttingen captivating. One could profit from the company of brilliant young people like Heisenberg, Pauli, and Fermi; there were great international events like the Bohr Festspiele in June 1922. After collaborating in Born's encyclopedia article on the lattice theory of crystals, Jordan decided to write his dissertation on the quantum theory of radiation (1924). This subject had been suggested to him by the historical transition from 1921 to 1924 in favor of Einstein's light quanta.

Empirical properties of X and γ rays, including the newly discovered Compton effect (1923), apparently could not be explained without admitting the existence of light particles. This started a new stream of theoretical games, in which quantum hypotheses on the interaction between matter and radiation were checked by "detailed balancing," by balancing emission and absorption processes for matter and radiation at thermal equilibrium, as Einstein had done in his famous theory of radiation of 1916. Probably encouraged by Göttingen's old resistance to light quanta, Jordan tried, with Born's benediction, to maintain detailed balancing (and also the Compton relation) without Einstein's drastic hypothesis of a directed spontaneous emission.[48]

This attempt was rejected by Einstein himself, who could point to an absurd consequence for absorption processes. Jordan became more convinced than anybody else of the necessity of light quanta. He was also the one at Göttingen who knew Einstein's inquiries in this field best.[49]

In the same year, 1924, Einstein impressed his admirer Jordan and his friend Born with his new theory of gases. As already mentioned,

47. Jordan (ref. 43), 1st session, 6.

48. P. Jordan, "Zur Theorie der Quantenstrahlung," *ZP, 30* (1924), 297–319.

49. A. Einstein, "Bemerkung zu P. Jordans Abhandlung 'Zur Theorie der Quantenstrahlung,'" *ZP, 31* (1925), 784–785.

the Göttingen group was the first to realize that there existed experimental evidence in favor of de Broglie's waves. Following this interest Jordan published in July 1925 a new detailed balancing between free electrons and black radiation based on de Broglie's waves. According to the symmetry between light and matter, free electrons had to be treated like Bose-Einstein particles and the interaction probability had to be symmetrical under exchange of electron and light-quantum densities. Jordan's corollary was: "We can understand the elementary scattering process not only as the scattering of light-waves by material corpuscles but also as the scattering of matter-waves by corpuscular light quanta."[50]

In 1927, following Otto Stern's proposal of a cosmic matter convertible into radiant energy, Jordan used his symmetrical expression for the interaction probability between matter and light to provide a new evaluation of energy partition in Stern's idealized cosmos. As pointed out by Bromberg, astrophysical speculations on matter annihilation were not new, but "from Stern's paper to Jordan's, the basis for creation and destruction of matter was transformed. From the mass-energy equivalence, it became the view that light and matter are special cases of a generalized 'particle.'" A later fruit of the light-matter analogy, Jordan's notion of quantized matter waves, would soon formalize these "generalized particles" and the processes of matter creation and destruction. Jordan's attitude was just the reverse of Dirac's. For Dirac empirical evidence of matter conservation excluded the light-matter analogy; for Jordan the light-matter analogy suggested the possibility of creating and destroying matter.[51]

A last example of Jordan's inheritance of de Broglie's and Einstein's faith in the matter-light analogy is his theoretical proof of electron diffraction. Duane had sketched in 1923 how light diffraction could be explained by momentum transfer between incoming light quanta and a quantized grating. This reasoning, Jordan noticed in 1926, could be applied to incoming atoms of *matter*, and implied a diffraction pattern corresponding to the wave length h/p identical to de Broglie's wave length.[52]

50. P. Jordan, "Über das thermische Gleichgewicht zwischen Quantenatomen und Hohlraumstrahlung," *ZP, 33* (1925), 649–655, on 652.

51. O. Stern, "Über das Gleichgewicht zwischen Materie und Strahlung," *Zeitschrift für Electrochemie, 31* (1925), 448–449, and "Über die Umwandlung von Atomen in Strahlung," *Zeitschrift für physikalische Chemie, 120* (1926), 60–62; P. Jordan, "Über die thermodynamische Gleichgewichtskonzentration der kosmischen Materie," *ZP, 41* (1927), 711–717. Cf. J. Bromberg, "The concept of particle creation before and after quantum mechanics," *HSPS, 7* (1976), 161–191, on 184–186.

52. W. Duane, "The transfer in quanta of radiation momentum to matter," National Academy of Sciences, *Proceedings, 9* (1923), 158–164; P. Jordan, "Bemerkung über einen Zusammenhang zwischen Duanes Quantentheorie der Interferenz und den de Bro-

Quantized electromagnetic field

In the summer of 1925 Born was looking for help in the monumental task of developing the consequences of the new multiplication rule just introduced by Heisenberg. Pauli refused, from disdain, he said, of Göttingen's formal mathematical emphasis, and it was Jordan who wrote with Born $PQ-QP=h/2\pi i$ for the first time.[53] This new formalism was the "quantum mechanics" Born had been searching for. In his view, the set of mathematical rules transforming a classical theory into the corresponding quantum theory had to be quite general, and they had also to apply to the classical radiation theory. Jordan's taste for mathematical homogeneity also favored such an extension. Therefore, in the last paragraph of their joint paper, Born and Jordan boldly quantized the electromagnetic field in the same way as the coordinates p and q of a particle. It was an easy task because of the well-known analogy between Maxwell's field and a set of harmonic oscillators. But the two innovators were also too ambitious: they tried to deal with the problem of field emission by a charged particle and got confused in a mess of indices labelling sometimes atom levels and sometimes field-oscillator levels. After reading their work, Heisenberg commented: "In respect of the matrix calculus in electrodynamics I would prefer that we concentrate for the moment on mechanics."[54]

The third basic paper in matrix mechanics was the so-called "Dreimännerarbeit" published in November 1925 by Heisenberg, Born, and Jordan. In spite of Heisenberg's strategic advice, Jordan explored the consequences of field quantization in the last part of this paper. This time he limited himself to the free field under the heading, "Coupled harmonic oscillators. Statistics of wave fields." That Jordan was the author of this part is attested by a letter from Heisenberg to Pauli:[55]

> A third thing that Jordan did for our work is a calculation of the statistical behavior of the natural oscillations of something like a membrane in the new theory. Jordan claims that the interference fluctuations come out right, i.e., both the classical and the Einsteinian terms, and he believes that there is an analogy between our calculations and Bose's statistics. I am rather sorry that I do not understand enough statistics to be able to judge how much sense it makes; but I cannot criticize either, because the problem itself and the subsequent calculations sound meaningful.

---

glieschen Wellen," *ZP, 37* (1926), 376–382.

53. M. Born and P. Jordan, "Zur Quantenmechanik," *ZP, 34* (1925), 858–888. For Pauli's refusal, cf. M. Born, *My life: Recollections of a Nobel laureate* (New York, 1978), on 118.

54. Heisenberg to Jordan, 7 Oct 1925 (AHQP).

55. M. Born, W. Heisenberg, and P. Jordan, "Zur Quantenmechanik. II," *ZP, 35*

Jordan had proceeded in the following way. Maxwell's field inside a cavity or more simply a vibrating string (Heisenberg says a "membrane") can be analyzed into a set of harmonic oscillators. Happily enough the only mechanical systems treated in the matrix mechanics of fall 1925 were harmonic or slightly anharmonic oscillators. The result was constant spacing between consecutive energy levels of an oscillator. Accordingly, the new mechanics led to a description of the different quantum states of the field formally identical to the one given by Ehrenfest in 1906 and by Debye in 1910: a state was defined by a sequence of integers giving the numbers of quanta associated with the field oscillators. This representation was recommended as the shortest way to Planck's law.

Knowing the correspondence between Bose's cells and Debye's cavity modes, Jordan saw that he could relate light quanta to wave excitations, and elucidate Bose's counting as the natural counting for quantized waves. Of course quantization à la Debye was not new; Jordan's innovation was to show that the scheme postulated by Debye became a necessary consequence of the new mechanics applied to Maxwell's field. Furthermore, energy discontinuities in the field appeared as properties of radiation itself, and not of the material emitters. The obstacles previously barring the connection between Debye's scheme and light quanta did not exist for Jordan: he was not hostile to light quanta, which in any case he did not interpret realistically.

This conceptual change was profound but less spectacular than the fluctuation calculation at the end of the "Dreimännerarbeit." Einstein's formula of 1909 for the average squared energy fluctuations in radiation, $\overline{\Delta^2} = h\nu\bar{E} + \bar{E}^2/(8\pi\nu^2/c^3)\nu d\nu$, had first been deduced by purely statistical methods from the semi-empirical Planck law. Not until 1925 did anyone succeed in justifying both terms of the formula by a *dynamical* calculation. Classical theory provided only the second term, light quanta only the first. Ehrenfest had even shown during a seminar attended by Jordan at Göttingen in the summer of 1925 that something intermediate—the classical oscillations of cavity modes plus energy quantization in the fashion of Ehrenfest-Debye—gave only the classical term (although the same scheme was sufficient to prove Planck's law). He also proved that this representation of radiation was incompatible with entropy additivity (which he now advocated) and leaned toward its rejection. In the "Dreimännerarbeit" Jordan mentioned Ehrenfest's analysis as a "valuable help" and drew a clear-cut conclusion: the old quantum theory failed even for the simple problem of the harmonic oscillator.[56]

56. P. Ehrenfest, "Energieschwankungen im Strahlungsfeld oder Kristallgitter bei Su-

With the new quantum mechnanics Jordan could calculate the evolution of the energy matrix E inside a partial volume v and, taking into account non-commutativity, found for time-averages ($\sim$) the relation $\widetilde{\Delta^2} = h\nu\widetilde{E} + \widetilde{E}^2/(8\pi\nu^2/c^3)v d\nu$, in agreement with Einstein's formula, if the diagonal elements of a matrix were interpreted (following Heisenberg) as the observable values of the corresponding classical quantity. The three men could rejoice: by a new mechanics born from atomic theory they had solved the most famous puzzle of radiation theory.

Nevertheless the true nature of this solution was still obscure: in this early period matrix mechanics was nothing but a skeleton, without any articulated interpretation. If matrices corresponding to atomic variables could be related to sets of spectral-line intensities, there was obviously no such interpretation for field matrices. This did not worry Jordan in his formalistic approach, and it was in fact cleared up later when the probabilistic interpretation of quantum mechanics was available. But other people's immediate reactions should be placed in this dark context.

Hostility

Einstein did not welcome the first dynamical proof of his fluctuation formula. He could have rejected it as part of an esoteric matrix mechanics. But in his letter to Jordan of March 6, 1926, he preferred to point out an insufficiency of the quantum field approach: Jordan could give only the average squared fluctuations, whereas Einstein, as early as 1905, knew the probability of any energy fluctuation, for instance $(v/V)^{\bar{E}/h\nu}$ for a fluctuation where all the energy was concentrated inside a volume v within the total cavity volume V.[57] In fact this objection was defeated by a more elaborate interpretation of the quantum mechanical formalism, the later Dirac-Jordan theory of transformations.

A more serious criticism was resurrected by Adolph Smekal from the past history of radiation. The main objection addressed by Planck and Lorentz to Einstein's fluctuation arguments in favor of light quanta was that they supposed applicability of statistical mechanics to pure radiation without any sample of thermalizing matter. Lorentz thought that entropy additivity for the imaginary partial volumes inside a cavity considered by Einstein did not hold, since classical radiation theory suggested spacially extended and correlated

---

perposition quantisierter Eigenschwingungen," *ZP, 34* (1925), 362–373; Born, Heisenberg, and Jordan (ref. 55), 610.

57. Einstein to Jordan, 6 Mar 1926 (AHQP).

electromagnetic oscillations. Planck underlined a qualitative difference between cavity radiation and a gas: for a gas out of equilibrium there existed a thermalizing process inside the volume of the gas due to intermolecular interactions, whereas radiation did not interact with itself.[58]

Some fifteen years later, in the spring of 1926, Smekal insisted again on the necessity of introducing a thermalizing body to analyze radiation fluctuations, not only in Einstein's statistical reasoning but also in the dynamical calculation: "[Jordan's calculation] supposes nothing less than the existence of radiation processes that can occur without the participation of real emitting and absorbing matter; such processes are essentially unobservable. . . .Energy fluctuations in portions of space free of any matter are essentially unobservable."[59]

To Heisenberg's ears this sounded like a familiar melody. Had he not claimed to base his matrix mechanics on a rejection of unobservable quantities? In October 1926 he sent three copies of the same letter to Jordan, Born, and Smekal to damp the quarrel. Jordan's calculations, he wrote, were certainly valid for sound waves inside a solid body, but the question of this applicability to electromagnetic waves had to be reserved until there existed a quantum mechanical treatment of the full interacting system of matter plus radiation. As for Jordan's explanation of Bose statistics, Heisenberg had already written that he could not judge it, "since he did not know enough statistics." This was not only modesty, it was the truth, as proved by his erroneous connection of antisymmetric waves with Bose statistics (June 1926).[60] He concluded his open letter with the following words: "I was actually ready to give up the publication of our three-man-note concerning the fluctuation problem, because any polemics cut me to the quick [this is a rhetorical figure], and I no longer saw any point worth fighting for."[61]

Poor Jordan was left alone with his great idea of quantized waves, and nobody except his closest collaborators knew that he had in mind an even more daring generalization. He thought indeed that matter as well should be represented by quantized waves, more specifically by quantized ψ's when he learned about Schrödinger's ψ-waves.

58. H.A. Lorentz, *Les théories statistiques en thermodynamique*, Conférences au Collège de France notées par L. Dunoyer (Leipzig, 1916), 59; M. Planck, in *La théorie* (ref. 16), 101–102.

59. A. Smekal, "Zur Quantenstatistik der Hohlraumstrahlung und ihrer Wechselwirkung mit der Materie," *ZP, 37* (1926), 319–341, on 322–323.

60. W. Heisenberg, "Mehrkörperproblem und Resonanz in der Quantenmechanik," *ZP, 38* (1926), 411–426.

61. Heisenberg to Born, Jordan, and Smekal, 29 Oct 1926 (AHQP).

Heisenberg's letter cited above bears a trace of this idea, expressed with his usual opportunism: "As for the question of whether Schrödinger's ψ 's are matrices or numbers I have not seen any compelling argument for any of the alternatives. One always has to ask first what can be started in this way." Jordan's path was strewn with difficulties: how to get Pauli statistics instead of Bose statistics, and above all, how to overcome the general indifference to his ideas: "Of course I was handicapped by my speech defect; to spread our ideas, I had to rely on private conversations or discussions in very narrow circles of friends, except for the publication of incomprehensible works."[62]

Jordan did not publish anything on quantized matter waves before 1927. In the summer of that year he told the story to Schrödinger:[63]

> The ideas that I set out in the last paragraph of my work on the Fermi gas [quantized matter waves coupled with light waves] have, as I claimed, an earlier origin; but my earlier conceptions were essentially private communications to Born, Heisenberg, and Pauli. At that time I had given a lot of thought to Einstein's gas theory and I had specified the representation in a way similar to your work in the Phys. Zeits. [on Einstein's gas theory]: The number of atoms in a cell corresponds to the quantum number of a cavity-mode oscillator (we also briefly made this point in the "Dreimännerarbeit"). Then your hydrogen paper gave hope that by following up this correspondence also the non-ideal gas could be represented by quantized waves—that therefore a complete theory of light and matter could be derived in which, as an essential ingredient, this wave field itself operates in a quantum, non-classical way; the need to represent the light field as a quantum mechanically operating wave field was obvious to me after the result of the analysis of the fluctuation properties of quantized waves (cf. "Dreimännerarbeit"). The difficulty on which this hope seemed to flounder at that time was just the validity of Pauli's statistics instead of Einstein's: since this difficulty seemed to be unsuperable, I gradually came to doubt the correctness of the whole representation. Furthermore, Pauli and Heisenberg did not want to hear much about it, while Born was initially very favorable, but later completely withdrew his support.

Jordan also expressed his difficulties in a review article composed in 1926. "Deriving this quantization [of the natural oscillations inside a cavity] from the general principles of quantum mechanics has been impeded by the fact that the present formulations of quantum mechanics concern only systems of individual mass points (electrons,

62. Jordan to Born, 3 Jul 1948 (AHQP). Cf. also Jordan to Bohr, 29 Jul 1926, Franck to Bohr, 9 Jul 1926, and Bohr to Franck, 21 Jul 1926 (AHQP) about funds from the Copenhagen Institute to cure Jordan's stuttering.

63. Jordan to Schrödinger, summer 1927 (original undated) (AHQP).

nuclei), whereas we have to do with an oscillating continuum in the case of the electromagnetic cavity." This comment was not yet in print, however, when Jordan read a new paper by Dirac, which removed the impediment. Jordan added an up-beat footnote: "Dirac's procedure offers. . .a complete justification of Born, Heisenberg, and Jordan's conviction that the difficulties concerning the intensity fluctuations of radiation must be solved quantum-mechanically in quite the same way as the corresponding difficulty in the oscillation energy of crystal lattices."[64] What had Dirac accomplished to overturn judgments on field quantization so quickly?

2. CONSTRUCTION

Dirac's radiation theory

"The quantum theory of the emission and absorption of radiation" was communicated by Niels Bohr to the Royal Society, where it was received on February 2, 1927. This theory was elaborated during a four-month stay in Copenhagen by Dirac alone, following his taste for solitary thinking.[65]

During the first two years of quantum mechanics the electromagnetic field had been treated classically in many problems: electron in a classical Coulomb field, scattering, Zeeman effect, and even the Compton effect (!), with a success beyond expectation. Consequently, Jordan's attempt at field quantization could be put aside, in favor of a fairly simple procedure: one introduced the classical imposed field by the substitution $\vec{p} \to \vec{p} - e\vec{A}$, $H \to H + eV$ and calculated the emitted (classical) field in scattering problems from the electric dipole moment of the atom.

But there was a serious flaw in this scheme: spontaneous emission by excited atoms could not be treated without the help of a special correspondence argument. The stationary charge current of Schrödinger's atom could apparently not radiate, and Schrödinger failed in an attempt to solve the purely undulatory coupled problem: Maxwell field plus ψ-field.[66]

64. P. Jordan, "Die Entwicklung der neuen Quantenmechanik," *Die Naturwissenschaften, 15* (1927), 614–623, 636–649, on 642–643.

65. P. Dirac, "The quantum theory of the emission and absorption of radiation," *PRS, 114* (1927), 243–265.

66. E. Schrödinger, "Der Energieimpulssatz der Materiewellen," *AP, 82* (1927), 265–272. The self-energy difficulties encountered by Schrödinger were not incurable. Instead, E.T. Jaynes and others proved in 1956 that a theory of radiation in line with Schrödinger's attempt gives many satisfactory predictions (including the characteristics of spontaneous emission). I thank Claude Cohen-Tannoudji for this remark. More de-

Dirac was the first to explain spontaneous emission by a blind application of quantum mechanics to the dynamical system consisting of atoms interacting with radiation. He could derive Einstein's A and B coefficients without any supplementary hypothesis. This success came as a fruit of his newborn transformation theory, which gave systematic rules to quantize any dynamical system and to interpret the resulting formalism.[67] Everybody was impressed by the new radiation theory. Heisenberg and Pauli soon decided to construct their own quantum electrodynamics on the same basis. Bohr liked it too because it fit well with his philosophy of complementarity: "The renunciation of intuition in space and time that characterizes this treatment of the [radiation] problem gives an impressive indication of the essentially complementary nature of description in the theory of quanta."[68]

Dirac's paper could have been limited to the following topics: quantization of the free field à la Jordan, with an interpretation in terms of light quanta, and a demonstration of the fact that the Hamiltonian of interaction was an operator raising or lowering the numbers of light quanta. In fact there was something more, what Jordan later called second quantization, which had been Dirac's true starting point:[69] "I remember the origin of that work was just playing about with the equations. I was intending to get a theory of radiation at that time. I was just playing about with the Schrödinger equation. I got the idea of applying the quantization to it and worked out what it gave and found that it just gave the Bose statistics."[70] Let us contemplate the odd game the young Cambridge theorist was playing.

He first considered a statistical ensemble of systems described by the wave function ψ obeying to the Schrödinger equation $i\hbar\partial\psi/\partial t = H\psi$ with $H = H_0 + V$, H_0 being the Hamiltonian for free evolution and V representing some kind of interaction. If ψ_r denotes the eigenstate of H_0 with E_r as eigenvalue, the coefficients b_r in the development $\psi = \sum_r b_r \psi_r$ give the number $|b_r|^2$ of systems in the state r (ψ is

tails can be found in E.T. Jaynes, "Survey of the present state of the neo-classical radiation theory," in L. Mandel and E. Wolf, eds., *Coherence and quantum optics* (New York, 1973), 35–81.

67. P. Dirac, "The physical interpretation of the quantum dynamics," *PRS, 113* (1927), 621–641.

68. N. Bohr, in *Electrons et photons*, 5th Solvay congress (1927), *Rapports et discussions* (Paris, 1928), 241–242.

69. The first occurence I found of the expression "Zweite Quantelung" is in P. Jordan, "Zur Methode der zweiten Quantelung," *ZP, 75* (1932) 648–653, and also V. Fock, "Konfigurationsraum und zweite Quantelung," ibid., 622–647. Jordan later confirmed that he was the inventor of this name (ref. 43).

70. P. Dirac, interview by T.S. Kuhn (1962-1963), session 5, on 20.

normalized to the number of systems in the ensemble, and ψ_r to one). On this base the Schrödinger equation can be rewritten:

$$\left.\begin{aligned} i\hbar\dot{b}_r &= \sum_s H_{rs} b_s \\ -i\hbar\dot{b}_r^* &= \sum_s H_{sr} b_s^* \end{aligned}\right\}, \text{ where } H_{rs}^* = H_{sr} ,$$

and this system—here is the trick—can be viewed as the canonical equations for the Hamiltonian

$$F = \sum_{rs} b_r^* H_{rs} b_s \tag{1}$$

the canonical variables being $(b_r, i\hbar b_r^*)$. Then, why not quantize this "classical" theory by imposing $[b_r, i\hbar b_s^+] = i\hbar\delta_{rs}$ and $[b_r, i\hbar b_s] = 0$, or $[b_r, b_s^+] = \delta_{rs}$ and $[b_r, b_s] = 0$? From these conditions it follows that the spectrum of the operator $N_r = b_r^+ b_r$ is $0, 1, 2, \cdots$, and the action of b_r and b_r^+ in the scheme diagonalizing N_r is:

$$b_r \Psi(n_1, \cdots n_r, \cdots) = (n_r)^{1/2} \Psi(n_1, \cdots n_r - 1, \cdots) \tag{2}$$

$$b_r^+ \Psi(n_1, \cdots n_r, \cdots) = (n_r + 1)^{1/2} \Psi(n_1, \cdots n_r + 1, \cdots) \tag{3}$$

Equation (2) indicates destruction, equation (3) creation.

The total number of systems

$$N = \sum_r N_r$$

is conserved during the evolution $i\hbar\partial\Psi/\partial t = F\Psi$, that is to say $[F, N] = 0$. Following the general interpretation rules established in his transformation theory Dirac interpreted $|\Psi|^2(n_1, \cdots, n_r, \cdots)$ as the probability that the number of systems in the state r be n_r. The evolution of this Ψ could be written out by means of equations (1)–(3):

$$i\hbar\frac{\partial}{\partial t}\Psi(n_1, \cdots, n_r, \cdots) = \tag{4}$$

$$\sum_{rs} H_{rs} n_r^{1/2} (n_s + 1 - \delta_{rs})^{1/2} \, \Psi(n_1, \cdots n_r - 1, \cdots n_s + 1, \cdots).$$

A few months earlier Dirac had given the quantum mechanical evolution of an assembly of n particles obeying the Bose statistics.[71] This assembly had to be described by a symmetrical wave function $\phi(\vec{x}_1, \cdots \vec{x}_n)$ in configuration space, or, thanks to the freedom given by the theory of transformations, by the wave function $b(r_1, \cdots r_n)$ giving the probability amplitude that the ith boson is in the r_i eigenstate of H_0. The b, being completely symmetrical, could be re-expressed in terms of the probability amplitude $b'(n_1, \cdots n_r, \cdots)$ that n_r bosons be in the state r:

71. P. Dirac, "On the theory of quantum mechanics," PRS, 112 (1926), 661–677.

$$b(r_1, \cdots r_n) = (n_1! \cdots n_r! \cdots /n!)^{1/2} b'(n_1, \cdots n_r, \cdots),$$

the square root being there to preserve the normalization:

$$\sum_{r_1, \cdots r_n} |b|^2 = \sum_{n_1, \cdots n_r, \cdots} |b'|^2 = 1$$

(To a given sequence $n_1, \cdots n_r, \cdots$ correspond $n!/(n_1! \cdots n_r! \cdots)$ equivalent configurations). This new representation was of course favored by Dirac's earlier notice of de Broglie's work at the Kapitza club.

An easy calculation shows that the evolution of b' induced by the Hamiltonian $\sum_i H_i$, H_i being an exemplar of H, is exactly the one given by equation (4) if one identifies Ψ and b'. The result of the game was a new elegant formulation of something known.

Although this formulation was quite general and could be applied to any kind of bosons, Dirac used it only as a bridge between the light quanta in configuration space and electromagnetic field quantization, which he assimilated to a "wave point of view:" Jordan's field Hamiltonian, to which Dirac added an interaction term, had the same structure as the Hamiltonian F for a set of bosons. Actually this identity could not be quite exact because F conserves the total number of particles, while the quantized electromagnetic interaction term $-\vec{j}.\vec{A}$ raises or lowers excitation numbers by one unit. To save it, Dirac used the freedom he had in interpreting the F-evolution. He noticed that quanta with zero momentum had to be unobservable. Then transitions of normal light quanta from (or toward) these special states could be interpreted as a creation (or a destruction) process. Dirac could now safely conclude: "There is thus a complete harmony between the wave and light quantum descriptions of the interactions."[72]

Dirac's handling of this duality at first appears to differ from Jordan's work: the chronological order of Dirac's researches, as well as the order of the paragraphs in his radiation paper, seem to favor the particle picture. In fact the logical order, re-established in Dirac's introduction, is the one given in the "Dreimännerarbeit." One starts from the classical wave picture of electromagnetic radiation, applies to it the formal rules of quantization, and gets the discontinuous structure of radiation energy. As shown by Jordan, natural statistics applied to these quantized waves were equivalent to Bose statistics applied to a set of light quanta. Dirac further demonstrated that the time evolution of the quantized radiation field was equivalent to the Schrödinger-Dirac evolution of a set of bosons in the symmetrized

72. Dirac (ref. 65), 245.

configuration space. Thanks to the transformation theory, he could also explain why this formal equivalence was not endangered by the contradiction between corpuscular and wave properties: a corpuscular property like the number of particles, and a wave property like the phase, could not be known precisely at the same time because they were represented by canonically conjugate variables.[73]

Regardless of their agreement on the meaning of radiation-field quantization, Jordan and Dirac could not possibly agree on the significance of second quantization. Jordan's reading of Dirac's paper neglected at least two points incompatible with his position. First, the new treatment of a set of Bose particles, later called second quantization, belonged for Dirac to the corpuscular point of view. The true starting point of this treatment was the classical Hamiltonian dynamics of a single particle, which was then submitted to two levels of formal quantization rules. The first level gave the usual Schrödinger equation, now viewed as a formal tool to calculate the evolution of the probabilities of various individual energy states of particles, and not as a mechanical wave equation, which was Schrödinger's original conception. The second level of quantization was then nothing but a convenient way to take Bose statistics into account.

The other point was a warning against confusing the ψ wave associated with a light quantum and the electromagnetic wave. The latter was a real wave whose spectral intensity gave the number of quanta times $h\nu$, the first was a complex wave whose intensity $|\psi|^2$ gave directly the number of quanta $h\nu$. Electrons, Dirac added, had only one type of wave, the Schrödinger wave.[74]

These remarks prove sufficiently that Dirac did not take his second quantization as a derivation of the discontinuity of light. This discontinuity originated in the quantization of electromagnetic waves. Since for electrons or protons there were no analogous waves, the discontinuity of matter had to be a primary, not further deducible, property.

The completion of Jordan's program

When Jordan read the radiation paper of his British rival, he had already his own views on radiation and on quantized Schrödinger equations. In the matter-light analogy he was much closer than Dirac to de Broglie and Einstein. Where Dirac feared a possible source of confusion, Jordan appreciated a powerful heuristic principle, which would give the correct quantum representation of matter as soon as the formal analogy between light waves and matter waves was

73. Ibid., 244.
74. Ibid., 247.

perfected. In a first audacious step de Broglie had brought together light and matter under a single concept of "wave-coupled atoms," to evolve into Schrödinger's and Jordan's "quantized waves." For Jordan, Dirac's new treatment of a Bose assembly was the precise quantum-mechanical formulation of this old idea.

Stimulated by the enthusiastic reception of Dirac's theory, Jordan returned with renewed energy to his researches on quantized matter waves. The main obstacle, the Pauli-Fermi statistics of electrons, still barred the way, although some progress had already been made to circumvent it.

Late in 1926 Pauli had recovered Fermi's theory of a degenerate gas (February 1926) by extending Schrödinger's notion of quantized waves to fermions. He considered the two possible types of representation for a gas: Einstein's (cells in phase space) and Schrödinger's (quantized waves) as "two equally justifiable descriptions." The adaptation of quantized waves to Fermi's statistics was straightforward: one just had to restrict the permitted energy values of a cavity mode of frequency ν to 0 and $h\nu$ instead of taking the whole set of integral multiples: $0, h\nu, 2h\nu, \cdots nh\nu, \cdots$. From the average value $\bar{n}_E(T)$ of the number n_E of particles inside a partial volume v and an energy interval dE, Pauli, returning to Einstein's statistical method, could also derive for the first time the fluctuation formula for fermions: $\overline{\Delta n_E^2}/\bar{n}_E^2 = 1/\bar{n}_E - 1/z_E$, instead of $1/\bar{n}_E + 1/z_E$ for bosons. For vanishing temperatures and inside the Fermi sphere (following modern terminology), where $\bar{n}_E = z_E$, the fluctuations vanish. Pauli commented on this mysterious result: "Perhaps this behavior will be explained by introducing phase relations between the various de Broglie natural oscillations, and it might therefore give a hint for a future physical explanation of the basic hypothesis of Fermi's statistics and of the equivalence rule [exclusion principle] at the same time."[75]

The suggestion, though vague, was encouraging for Jordan. In July 1927 he had reached his goal: a generalization of Schrödinger's wave quantization to Fermi's statistics. For this purpose the "new foundation of quantum mechanics" he had just given was instrumental.[76] This work, generalizing the statistical interpretation of quantum mechanics, closely paralleled Dirac's "Theory of transformations." But it was set into a semi-axiomatic form, and Jordan's axioms were in fact more general than the modern (and Dirac's) version of the quantum formalism. They allowed a broader notion of canonical

75. W. Pauli, "Über Gasentartung und Paramagnetismus," *ZP, 41* (1927) 81–102, on 95–96.

76. P. Jordan, "Über eine neue Begründung der Quantenmechanik, II," *ZP, 44* (1927), 1–25.

conjugation, which Jordan found necessary for associating a conjugate operator with the spin operator S_z. Neither S_z nor N_r (the numbers of particles usėd by Dirac) could have a conjugate in the ordinary sense because they were operators with a discrete spectrum without accumulation point.[77] Jordan's concept of conjugation was thus very different from Dirac's: it was defined axiomatically and did not have a classical counterpart, whereas Dirac could not think of conjugation but in terms of classical Poisson brackets.

In his radiation theory Dirac had used Θ_r, operators conjugate to N_r and obeying the relation $[N_r,\Theta_r] = -i$. The mathematical impossibility of this relation was harmless, as Dirac explained to Jordan: only the meaningful relation $[N_r,e^{i\Theta_r}] = e^{i\Theta_r}$ $(N_r^{1/2}e^{i\Theta_r} = b_r^+$ and $N_r = b_r^+ b_r)$ was required.[78] In his "quantum mechanics of gas degeneracy" published in July 1927, Jordan not only defined Θ_r for Dirac's N_r (with spectrum 0,1,2,....) but also for the natural N_r operator of Fermi statistics (with 0 and 1 for eigenvalues). In the latter case the result was a 2x2 matrix:

$$\Theta_r = \pi/2 \begin{bmatrix} 0 & 1 \\ 1 & 0 \end{bmatrix}_r$$

if

$$N_r = \begin{bmatrix} 0 & 0 \\ 0 & 1 \end{bmatrix}_r,$$

and consequently

$$b_r = e^{-i\Theta_r} N_r^{1/2} = -i \begin{bmatrix} 0 & 1 \\ 0 & 0 \end{bmatrix}_r.$$

Substituting this value into Dirac's Hamiltonian

$$F = \sum_{rs} b_r^+ H_{rs} b_s,$$

he obtained an evolution identical to the one given by antisymmetrical wave functions in configuration space. As in the "Dreimännerarbeit," Jordan submitted his new formalism to the test of fluctuations, this time for the density of an ideal gas. The Einstein and Fermi gases could be created on the same footing, by repeating the kind of dynamical calculation already made for a vibrating string; in this way Einstein's and Pauli's formulas could be recovered.[79]

77. For an operator β with the spectrum $(\beta'_i)_i$ there exists an algebraic function F satisfying $F(\beta'_i) = 0$ and $F'(\beta'_i) \neq 0$ for any i. Then $F(\beta) = 0$ and for any operator α the relation $[\alpha,\beta]F'(\beta) = [\alpha,F(\beta)] = 0$ excludes $[\alpha,\beta] = i\hbar$. Ref. 76, 2.

78. Cf. Jordan (ref. 76), 3.

79. P. Jordan, "Zur Quantenmechanik der Gasentartung," *ZP, 44* (1927), 473–480.

Although Jordan knew he was on the right track, his paper was only a sketch, full of misprints and imprecisions. The draft received by Alfred Landé resembles a bad student paper overcorrected by the professor. Jordan had omitted the signs introduced by creation operators when acting on fermionic states. These essential signs were determined exactly by Jordan and Wigner late in 1927. There appeared for the first time anticommutation relations, which became the formal symbol of the so-called "Wigner-Jordan second quantization:" $a_r a_s + a_s a_r = 0$, $a_r a_s^+ + a_s^+ a_r = \delta_{rs}$, the a_r differing from the b_r by the introduction of signs.[80]

Jordan expressed the essential meaning he gave to second quantization even before this rigorous formulation. His paper on gas degeneracy ended with the following words:

> The results we have reached hardly leave any doubt that—in spite of the validity of Pauli's statistics instead of Bose's for electrons—a quantum-mechanical wave theory of matter can be developed that represents electrons by quantized waves in the usual three-dimensional space. The natural formulation of the quantum theory of electrons will be attained by conceiving light and matter as interacting waves in three-dimensional space. The basic fact of electron theory, the existence of discrete electric particles, appears in this context as a characteristic quantum phenomenon; indeed it means exactly that matter waves occur only in discrete quantum states.

That the discontinuity of both matter and light could derive from formal wave quantization was not just a remarkable mathematical result in Jordan's mind. It was imbedded in a positivistic view of the world implicating the liquidation of materialism, as underlined by Cini. In his abundant philosophical writings Jordan advocated the rejection of atomism assimilated to a kind of materialism:[81]

> The new concepts, resulting from the experiences of quantum physics and their intellectual interpretation, mean a far-reaching liquidation of the classical western world picture developed by natural science from the Greek materialistic philosophy. . .In Democritus' representation each individual atom had a definite destiny and possessed in its indestructibility and invariability the permanent guarantee of its lasting identity; while the electrons and other elementary particles of the modern physicist, aside from their destruction and conversion properties, possess no individuality. . .Finally, the existence of atoms is no longer a

80. Reprint of the preceding paper annotated by A. Landé, Office for History of Science and Technology, University of California, Berkeley.

81. P. Jordan, *Physics in the 20th century* (New York, 1944), 144, 146, partly quoted in M. Cini, "Cultural traditions and environmental factors in the development of quantum electrodynamics (1925–1933)," *Fundamenta scientiae, 3* (1982), 229–253, on 248.

> primary basic fact of nature; it is only a special part of a much more general and comprehensive phenomenon—the phenomenon of quantum discontinuities.

The coherence of Jordan's philosophical attitude might be questioned: is it possible that the choice (or possibility of the choice) of a special kind of formalism in a physical theory might "justify and confirm" positivism? Is it not true that this special choice was guided by a heuristic principle, the light-matter analogy, which should be completely arbitrary for a positivist, and which was indeed introduced by anti-positivistic thinkers like Einstein and de Broglie? In any case Jordan was convinced of the superiority of his wave quantization over the usual Schrödinger-Dirac multi-dimensional configuration space. If it was not truer, it was at least more economical, since, as Jordan stressed, it made use of the usual three-dimensional space instead of an abstract multi-dimensional space. Jordan had now in hand the scheme that would frame future quantum electrodynamics: light and matter described as mutually interacting quantized waves in three-dimensional space.

To build a relativistic theory of coupled quantized fields was too difficult a task to be done in a single stroke. Jordan took the first step in Hamburg with Pauli, who hurried more than his Göttingen friends towards relativistic generalizations of quantum mechanics. The result of this collaboration was a fully relativistic theory of the *free* quantized electromagnetic field.[82] During a subsequent stay in Copenhagen at the end of the summer of 1927, Jordan met someone who had also read Dirac's radiation paper in a personal way and was trying to generalize second quantization. Oskar Klein, then Bohr's assistant in Copenhagen, had been searching for a solution of the relativistic several-body problem in his five dimensional theory. As he later explained, "I had still that left over from the five-dimension approach that I wanted to regard, so to say, the wave equation as a space-time thing." After meditating over Dirac's radiation paper he wrote to him in March 1927:[83]

> Thank you very much for the reprint of your last paper, which I read with great interest. I think it has quite convinced me that the quantum field theory ought to come on the lines you trace there. I tried sometime ago if it were possible to generalize your result concerning the

82. P. Jordan and W. Pauli, "Zur Quantenelektrodynamik ladungsfreier Felder," *ZP, 47* (1928), 151–173; the main results were given in a letter from Pauli to Bohr of 6 Aug 1927 (*PB,* 168); the publication was delayed several months probably because the authors were hoping for some further development.

83. O. Klein, interview by T.S. Kuhn, J.L. Heilbron, and L. Rosenfeld (1962), AHQP, session 2, 4, and session 6, 19–20; Klein to Dirac, 24 Mar 1927 (AHQP).

quantization of the amplitudes of the Schrödinger waves in taking the electrostatic reaction of the waves on themselves into account.

Where Dirac saw only a quantization of particle numbers, Jordan saw a wave quantization analogous to light-wave quantization, and Klein saw a space-time description of a set of particles, something more appropriate to a relativistic theory of interacting matter. In July 1927 Bohr wrote to Pauli:[84] "With such efforts [a statistics of ideal gases without multi-dimensional space] Klein also was busy for sometime, and he hoped to reach a relativistic treatment of the several-body problem." Klein planned a first exploratory step with a wave treatment of the *non-relativistic* Coulomb interaction between charged bosons. But he worried, as he wrote Dirac, that self-energy difficulties would ruin the project:[85]

> I got a formula corresponding to your formula 17 [quantized Schrödinger equation: $i\partial\Psi/\partial t = F\Psi$], where the general term agreed with the general term in the corresponding formula for the symmetrical eigenfunctions of a multitude of electrons acting on one another with electrostatic forces. But those terms where several indices were alike (i.e. the self-energy terms) did not agree and the formula I got did not lead back to the ordinary equation for $N=1$. So this only stresses the difference between light and material particles.

The last sentence must certainly have pleased Dirac, who kept warning against over-playing the light-matter analogy.

Together in Copenhagen, Klein and Jordan could reconsider the problem, and found out that the self-energy difficulty was very easily circumvented by choosing the right order (now called normal ordering) for products of quantized waves $\psi(\vec{r}) = \sum_\alpha b_\alpha (\vec{r} \mid \alpha)$. Specifically, one had to use the following expression for the new F-operator generalizing Dirac's F-Hamiltonian:

$$F = \int d^3r \psi^+(\vec{r})(-\hbar^2/2m)\Delta\psi(\vec{r})$$
$$+(e^2/8\pi)\int \psi^+(\vec{r})\psi^+(\vec{r}') \mid \vec{r} - \vec{r}' \mid^{-1} \psi(\vec{r})\psi(\vec{r}')d^3r d^3r',$$

with all the creators ψ^+ on the left side.

The Schrödinger evolution given by F was equivalent, as desired, to the Schrödinger evolution in configuration space given by the Hamiltonian

$$\sum_i (-\hbar^2/2m)\Delta_i + \sum_{i<j} (e^2/4\pi) \mid \vec{r}_i - \vec{r}_j \mid^{-1}.$$

84. Bohr to Pauli, 15 Jul 1927 (*PB*, 167).
85. Klein (ref. 83).

Normal ordering eliminated the extra infinite self-energy terms $(e^2/4\pi)\,|\vec{r}_i - \vec{r}_i|^{-1}$. Thanks to this success Jordan was very optimistic for a while about the future of quantum electrodynamics. He even hoped that the normal-ordering trick could be generalized in such a way that the well-known self-energy difficulties of *classical* electrodynamics would be solved. Jordan and Klein's paper was sent for publication in October 1927. Within a year, matter-wave quantization had become a clean and powerful tool, with a very direct formal expression (for Bose statistics):[86]

$$[\psi(\vec{r}), \psi^+(\vec{r}')] = \delta_3(\vec{r} - \vec{r}').$$

A few days later Jordan sent off another paper from Copenhagen: "On waves and corpuscles in quantum mechanics," in which he summarized his successes and, using his transformation theory, generalized second quantization to calculate the probability that the single particle states α_i are occupied $N(\alpha_i)$ times knowing that the single particle states β_j are occupied $N(\beta_j)$ times, that is to say $|(N(\alpha_1), N(\alpha_2) \cdots \mid N(\beta_1), N(\beta_2) \cdots)|^2$ in Dirac's notation. He was now armed to answer Einstein's criticism against his derivation of radiation fluctuations: taking the energy for the β_j and the position for the α_i one could calculate the probability of any density fluctuation in a stationary state of an ideal gas, as in Einstein's light-quantum paper of 1905. Proudly, Jordan announced with Bohr's blessing "a very far-reaching and clear explanation. . .of the mathematical relations which are basic for the quantum-mechanical dualism between corpuscles and quantized waves." This was Jordan's contribution to the interpretation debate animating Copenhagen in 1927.[87]

3. ASSIMILATION

The early reception of quantized matter waves

Bohr and Heisenberg

Part of the reactions to matter wave quantization were conditioned by the debate on uncertainties and complementarity held in Copenhagen during the first semester of 1927. The two main protagonists were Bohr and Heisenberg, though Klein often took part to

86. P. Jordan and O. Klein, "Zum Mehrkörperproblem der Quantentheorie," *ZP, 45* (1927), 751–765.

87. P. Jordan, "Über Wellen und Korpuskeln in der Quantenmechanik," *ZP, 45* (1927), 766–775.

and probably found opportunities to support his and Jordan's views on wave quantization. As is well known, there was initially a strong opposition between Bohr and Heisenberg about the meaning of the proofs of the famous uncertainty relations $\Delta q \Delta p \geq h$. Before sending his uncertainty paper for publication, Heisenberg converted himself to Bohr's opinion, but rather reluctantly and only partially. Amazingly, the Jordan-Klein paper was welcomed by the two protagonists, and might even have helped damp the leftover discord.

Heisenberg had provided two proofs of the uncertainty relations. The first one was just a formal derivation using the commutator $[q,p]$, the second one was a semi-classical derivation, starting from the usual classical description of an imaginary γ ray microscope and correcting it semi-quantitatively by the introduction of the quantum discontinuity in the exchange of energy between γ rays and the observed electron. According to Heisenberg, the fundamental feature leading to uncertainty relations was, in both proofs, the quantum discontinuity introduced either formally through quantum mechanics or empirically as an extra limitation imposed on the classical analysis of a thought experiment.

Bohr strongly opposed this point of view: for him any physical discussion had to start from the underlying internal logic of the theory, which could be reduced neither to formalism nor to an empirical discussion of measurement possibilities. The logic of the new quantum mechanics had to be found in the wave-particle duality and the relevant Einstein-de Broglie formulas $E = h\nu$, $\vec{p} = \hbar\vec{k}$ relating wave and particle concepts. Uncertainty relations then meant a quantitative estimate of the degree of mutual exclusion of these two concepts. Heisenberg could not deny that his thought experiments indeed made use of the wave-particle duality merely to evaluate the minimal discontinuous exchange of energy between γ rays and the observed particle.[88]

When he knew about the Jordan-Klein quantization, Heisenberg interpreted it as a formal bridge between his original point of view and Bohr's. Wave-particle dualism, taken as a start by Bohr, could be derived by formal quantization applied either to the wave picture or to the particle picture. Heisenberg later remembered: "This [matter wave quantization] I liked very much because now I could see, 'All right. There is an entirely different picture to start with, and if I quantize that picture—that is if I make this picture open to the same restrictions as the particle picture—then the two pictures become

88. Cf. J.L. Heilbron, "The Copenhagen spirit in quantum physics and its earliest missionaries," *Revue d'histoire des sciences* (1986), 194–230, and L. Rosenfeld, "Men and ideas in the history of atomic theory," *AHES, 7* (1971), 69–90.

equivalent.'"[89]

On his side Bohr, in spite of his dislike of formalism, welcomed Dirac, Jordan, and Klein's quantized waves. Being neither waves nor particles they illustrated the necessity of banishing the classical intuition of causal spatio-temporal descriptions of phenomena, in accordance with the philosophy of complementarity. At the Solvay congress of October 1927, he praised the "symbolic procedure" of Jordan and Klein, a procedure "analogous to the deep treatment of the radiation problem developed by Dirac," where he could enjoy the same "renunciation of the intuition of space and time. . ., which shows impressively the essentially complementary nature of description in quantum theory."[90]

Schrödinger

When Schrödinger discovered wave mechanics in 1926, he thought that he could save physics from the abstract algebra of discontinuity proposed by the Göttingen school. He hoped that his ψ waves could be interpreted as mechanical waves, the squared modulus $|\psi|^2$ representing some kind of density of electronic matter. The various proofs of formal equivalence between matrix and wave mechanics did not erase this fundamental interpretative difference. Obstacles to a realistic interpretation of the ψ wave, raised by the expert in criticism Pauli, did not extinguish Schrödinger's hope of a future victory. Among these obstacles stood the abstract multi-dimensional space that Schrödinger had been forced to introduce to deal with several-body problems.

Jordan's quantized matter waves removed this barrier by re-establishing three-dimensional physical space in this type of problem. Also they resembled the quantized waves used by Schrödinger himself in December 1925 to explain Bose statistics, and to re-define particles as wave excitations. This was a first convergence point for the ideas of the two quantum theorists.

Meanwhile, Schrödinger had needed only ordinary (unquantized) waves interacting with a Coulomb nuclear potential for his spectacular derivation of atomic spectra. Guided by his mechanical interpretation, he had attempted to take into account the Coulomb self-interaction energy of a ψ wave carrying the charge density $e\psi\psi^*$. But the corresponding wave equation,

$$i\hbar\partial\psi/\partial t = (-\hbar^2/2m)\Delta\psi + (e^2/4\pi)\int \psi^*(\vec{r}\,')\psi(\vec{r}\,')\,|\vec{r}-\vec{r}\,'|^{-1}d^3r'\psi(\vec{r}),$$

89. W. Heisenberg, interview by T.S. Kuhn and J.L. Heilbron (1963), AHQP, session 8, 21.

90. N. Bohr (ref. 68), 241, 238.

led to incorrect results. Still worse was the effect of the radiation reaction computed by introducing the source terms $\rho = e\psi^*\psi$ and $\vec{j} = (e/m)\mathrm{Re}\, \psi^* - (i\vec{\nabla} - e\vec{A})\psi$ in Maxwell's equations: it gave absurd infinite energy shifts.[91]

Now Jordan's plan for a future quantum electrodynamics started with this kind of coupling between matter and electromagnetic waves; and the Jordan-Klein F-Hamiltonian gave canonical equations identical to Schrödinger's equation for the self-interacting ψ. The only difference was the extra condition added by Jordan: $[\psi(\vec{r}),\psi^+(\vec{r}')] = \delta_3(\vec{r}-\vec{r}')$.

When he read the conclusion of Jordan's paper on gas degeneracy, Schrödinger recognized his first idea of quantized waves, and also the idea of interacting matter and light waves. On July 28, 1927, he wrote to Jordan:

> I am particularly interested in your remark [on coupling quantized light- and matter waves]. Indeed, as far as I understand it, it is also my opinion. I had thought that this point of view was strictly excluded by Göttingen and Copenhagen. Now I am happy that there are more prospects, that we are coming together again. Occasionally it would be useful to me to know where you expressed similar views right after the publication of my first papers (as expressed in your footnote).

Jordan explained himself extensively in the answer quoted already, and in his paper with Klein he referred for the first time to Schrödinger's paper on Einstein's gas theory as related to his own ideas.

Nevertheless this convergence was very superficial: there was too much abstract algebra in Jordan's idea to please Schrödinger's taste for intuitive physics. In October, at the Solvay congress, he protested that Jordan's way was not his or nature's: "One must first communicate in physical language. For the moment I cannot see an answer to a physical question in the assertion that some variables are submitted to a non-commutative algebra, especially when these variables are supposed to represent numbers of atoms."[92] Only much later, and still refusing allegiance to the Copenhagen interpretation, did he suggest that quantized waves might provide a convenient basis for a realistic and non-probabilistic interpretation of quantum mechanics. Although he opposed Jordan's positivistic perversion of complementarity even more than its original Copenhagen version, he agreed at least on one point: "We must not be afraid of losing time-honoured atomism. It has its counterpart in the level-scheme (of second quantization) and

91. Schrödinger (ref. 66).
92. Schrödinger, in *Electrons et photons* (ref. 68), 208.

nowhere else."[93]

Dirac

Dirac's rejection of matter-wave quantization has a special interest because it conditioned to a large extent the attitude of other specialists in early quantum field theory. The scientific authority of the one who invented both a new radiation theory and a new relativistic equation for electrons was such that second quantization was sparsely used before the war, even where it could have been most helpful, in the positron theory. Before the 1950s Dirac systematically avoided the Jordan-Wigner quantization although he could not ignore it, and although he had himself given the first example of second quantization. But he made no formal attacks: he did not like spending time criticizing other people's work, and preferred to concentrate on his own research. His only written criticism I could find was reported at the Solvay congress of 1927:[94]

> Mr. Dirac argues that this modification [transition from Dirac's second quantization to Wigner-Jordan's] is very artificial from a general point of view. Fermi statistics are not really established on the same basis as Einstein-Bose statistics, since the natural way of quantizing waves leads precisely to the latter statistics for the particles associated with these waves. To get the Fermi statistics, Jordan had to introduce a singular quantization method, designed on purpose to reach the desired result.

Recently in Varenna Dirac discussed his reaction to Jordan's initiative:[95]

> At first I did not like the work of Jordan and Wigner, and I think I can attribute this dislike to my mind being an essentially geometrical one and not an algebraic one. In the case of the Bose statistics and the second quantization which was connected with it one had a definite picture underlying the basic equations, namely the picture that the theory would be applied to an assembly of oscillators. There was no such picture available with the Fermi statistics, and I felt that was a serious drawback. I did not appreciate therefore the importance of this other kind of second quantization.

One might rightly be surprised that the inventor of q-numbers and of γ_μ matrices thought he had a geometrical mind. Elsewhere, in the

93. Cf. E. Schrödinger, "The meaning of wave mechanics," in *Louis de Broglie physicien et penseur* (Paris, 1953), 16–32, on 24, 28.
94. Dirac, *Electrons et photons* (ref. 68), 270.
95. P. Dirac, "Recollections of an exciting era," in Charles Weiner, ed., *History of twentieth century physics* (New York, 1977), 109–146, on 140, (International school of physics "Enrico Fermi," course 57).

foreword to the first edition of his *Principles of quantum mechanics* he wrote: "Fundamental laws do not govern the world as it appears in our mental picture in any very direct way, but instead they control a substratum of which we cannot form a mental picture without introducing irrelevancies."[96] This statement might echo Eddington's belief in an unknowable, unpicturable background, or Copenhagen's rejection of visual atomic models. In any case the geometrical pictures called upon by Dirac could not be a representation of his "substratum" the way fields and positions could be in classical physics.[97]

By mental pictures one might mean any representation that had proved to be useful in standard problems, for instance oscillators, point-like particles, spinning particles, or any of the esoteric drawings used by mathematicians to illustrate and guide their demonstrations. What Dirac meant at Varenna was much more specific: there is a special privileged class of pictures, and a precise relation linking the quantum formalism to this class. These pictures are the basic concepts of classical mechanics, like material points and waving fields. The connection with quantum formalism is provided by the quantization rules of a Hamiltonian system, for instance, substituting commutators for Poisson brackets.

In his first paper on quantum mechanics, Dirac had stressed the permanence of classical equations in Heisenberg's new theory, and therefore implicitly the importance of classical pictures:[98]

> In a recent paper Heisenberg puts forward a new theory which suggests that it is not the equations of classical mechanics that are in any way at fault, but that the mathematical operations by which physical results are deduced from them require modification. *All* the information supplied by the classical theory can thus be made use of in the new theory.

Dirac's formulation of quantum mechanics, as noticed by de Maria and la Teana, "represented an *extension* rather than a *destruction* of the conceptual world of classical physics." Dirac sharpened the correspondence principle more than Heisenberg himself, who believed that his introduction of a "new kinematics" destroyed the conceptual framework of classical physics, however needed it was as a basis for the correspondence method. This difference in weighing the importance of the classical basis faded as quantum formalisms and their interpretations were further developed.[99]

96. P. Dirac, *The principles of quantum mechanics* (Oxford, 1930), preface.
97. Cf. Bromberg (ref. 51), 190, and H. Kragh, "Cosmophysics in the thirties: Towards a history of Dirac cosmology," *HSPS, 13:1* (1982), 69–108, on 87–89.
98. P. Dirac, "The fundamental equations of quantum mechanics," *PRS, 109* (1925), 642–653, on 642.
99. M. de Maria and F. la Teana, "Dirac's unorthodox contribution to orthodox

But Dirac's and the Copenhagen-Göttingen understandings of the new correspondence method did not quite fuse together: the Copenhagen-Göttingen school quantized classical *formalisms* where Dirac quantized classical *pictures*. I mean that Dirac's classical basis had to be precisely the one used to describe the system under study in the known classical limit. To obtain a quantum theory of electrons, Dirac had to start with the corpuscular picture known to be valid in the classical limit, and not with another kind of classical formalism. In the same situation Jordan took the freedom to quantize waves.

Not only the choice of the correspondence basis, but also the quantization process, was wider for Jordan. As I have mentioned, Dirac's quantization was reached through a reinterpretation of classical canonical conjugation. Instead, Jordan's "singular method of wave quantization" required a non-classical axiomatic generalization of conjugation even if it was not "designed on purpose to reach the desired result."[100]

Dirac's disdain for the Jordan-Wigner quantization is not the only illustration of his conception of correspondence. Another one, even more spectacular, is given by his route to an equation for the relativistic electron. Whereas the continental school (for instance Kramers and Pauli) was seeking a relativistic generalization of Pauli's equation for the spinning electron, Dirac started from the *true* classical picture, the point electron, without the spin property; for spin, being proportional to Planck's constant, was known to be non-classical. In his approach this property arose as a by-product of a relativistic quantization. Finally, to solve the negative-energy difficulty of this otherwise wonderful equation, Dirac again avoided Jordan's quantization. He used the freedom he had in connecting his equation to observation, and introduced an unobservable sea of negative energy electrons, analogous to the zero-momentum light quanta of his radiation theory.[101]

Mathematical physicists

Whereas Jordan, Bohr, Heisenberg, and Schrödinger judged matter-wave quantization according to its epistemological bearing and Dirac judged it according to a pre-established methodology, other theorists underlined its mathematical advantages in particular problems. Wigner had probably found some interesting symmetry in

---

coni," Università di Roma, INFN, Sezione di Roma, p. 16.

100. Dirac, *Electrons et photons* (ref. 68), on 270.

101. Cf. H. Kragh, "The genesis of Dirac's relativistic theory of the electron," *AHES, 24* (1982), 31–67.

Jordan's attempt to quantize fermion waves and so helped him to a rigorous formulation. A young Russian visitor to Göttingen, Vladimir Fock, also liked the mathematics of second quantization. He introduced what is now called Fock's representation (where the annihilators are diagonal), and he generalized the correspondence between second quantization and configuration space (even for operators changing particle numbers). He did not state any preference for either of the two formalisms, although he could show that the Jordan-Wigner formalism simplified the Hartree-Fock method (for approximate solutions of the n-body problem). Fock's demonstrations, being remarkably clear and elegant, were often reproduced in subsequent accounts of second quantization.[102] .

A lesser known, but interesting case is Hermann Weyl's involvement in Dirac's hole theory. In the second edition (1931) of his *Group theory and quantum mechanics* he wrote: "Nothing of course prevents us from quantizing matter waves in a way analogous to electromagnetic wave quantization. . . .[And] nothing prevents us from replacing the number N_{PS} [of occupation of a state of 4-momentum P and spin S] by $N^-_{PS} = 1-N_{PS}$ if $P_0<0$ and to keep $N^+_{PS} = +N_{PS}$ if $P_0>0$." Thanks to this new notation, the second quantized Hamiltonian appeared to be invariant under charge-conjugation. And the holes first interpreted by Dirac as protons acted exactly as positively charged electrons. Amazingly, Dirac was immediately convinced by this argument, although it made use of the Jordan-Wigner quantization.[103]

Retrospectively we can say that Weyl's insight was very deep: he not only proved the symmetry between holes and electrons, but he also showed the need of *anticommutative* charge conjugation to get a charge-symmetric quantum electrodynamics. He noticed that coupled Dirac and Maxwell equations were invariant by the substitution $\psi \rightarrow C\psi^*$ and $e \rightarrow -e$ (C being a 4x4 number matrix, and * being a complex conjugation) only if the components ψ and $\bar{\psi}$ anticommuted, so that the coupling current became invariant. Weyl's discussion antedated both Fock's charge-symmetric formulation of positron theory (1933) and Belinfante's proof of the spin-statistics theorem (1939), all because he early recognized the usefulness of Jordan's quantized matter waves.[104]

102. V. Fock, "Verallgemeinerung und Lösung der Diracschen statistischen Gleichung," *ZP, 49* (1928), 339–357; V. Fock, "Konfigurationsraum und zweite Quantelung," *ZP, 75* (1932), 622–647.

103. H. Weyl, *Gruppentheorie und Quantenmechanik*, 2nd ed. (Leipzig, 1931), section IV C 12.

104. Ibid., 263-264; V. Fock, "Zur Theorie der Positronen," Akademiia Nauk, *Doklady,* (1933), 267–271; F.J. Belinfante, "Undor calculus and charge-conjugation," *Physica,*

The use of quantized matter waves

Heisenberg and Pauli's program

Enchanted by Dirac's radiation theory, Pauli soon proposed to Heisenberg a program for constructing a fully relativistic quantum electrodynamics. In his answer of February 1927, Heisenberg made clear his point of view:[105]

> I agree very much with your program concerning electrodynamics, but not quite concerning the analogy: quantum mechanics/classical mechanics = quantum electrodynamics/classical Maxwell theory. That one must quantize the Maxwell equations to get light quanta and so on à la Dirac, I believe already; but perhaps the de Broglie waves will also have to be quantized in order to get charge, mass, and statistics (!!) of electrons and nuclei.

Apparently, Pauli and Heisenberg did not agree about the classical basis to be quantized. Pauli, like Dirac, wanted to quantize the usual classical electrodynamics; Heisenberg, following Jordan, preferred to quantize only waves, even for matter. When second quantization for fermions was available, Heisenberg reacted enthusiastically:[106]

> Dear Jordan, Science owes you much for having melted under the sun of your romantic formulae the Damocles sword of Fermi statistics, which had been menacing for so long as a dark cloud above the horizon of particle electrodynamics. (Style freely adapted from de Broglie's *Recherches sur la théorie des quanta*, Leipzig 1927).

When Bohr, probably repelled by the dryness of Jordan's formalism, inquired about Pauli's opinion, he got the usual sharp reply. Pauli conceded some value to Jordan's conception, but only in the "phenomenological point of view," i.e., if one neglected the particle's

6 (1939), 849–869, and "The undor equation for the meson field," ibid., 870–886; Weyl's reasoning was the following. Since the matrix C is chosen so that $C^{+}\gamma_\mu C = \gamma_\mu{}^{*}$, charge conjugation applied to j_μ gives:

$$j_\mu^C = (-e)\overline{C\psi^*}\gamma_\mu C\psi^* = -e(C\psi^*)^{+}\gamma^o\gamma_\mu C\psi^* =$$
$$-e\,{}^t\psi C^{+}\gamma^o CC^{+}\gamma_\mu C\psi^* = -e\,{}^t\psi\gamma^{o*}\gamma_\mu{}^*\psi^*.$$

Since j_μ^C is a scalar in respect of spinorial indices, it remains unchanged by transposition if ψ and ψ^+ commute, and changes its sign if ψ and ψ^+ anticommute. In the latter case only, one has:

$$j_\mu^c = -{}^t j_\mu^c = +e\psi^+\gamma_\mu^+\gamma^{o+}\psi = j_\mu^+ = j_\mu.$$

105. Heisenberg to Pauli, 23 Feb 1927 (*PB*, 154).
106. Heisenberg to Jordan, 12 Dec 1927 (AHQP).

nature, charge, and electromagnetic interactions. For him, Jordan's suggestion of basing electrodynamics on interacting matter waves was just a foolish excursion beyond this limited range of validity.[107]

Fortunately, Pauli's criticism, albeit sharp, was not obstinate. He was converted by Jordan and Klein's "truly beautiful paper," which proved that matter-wave quantization could also subsume electromagnetic interactions. At the Solvay congress of October 1927 Pauli was the one who brought forward second quantization when Einstein recalled the natural defect of configuration space: "In this representation two configurations of a system that differ only by the permutation of two particles of the same type are represented by two different points (in configuration space), which is not in agreement with the new results of statistics." Let us mention that Pauli's reply to this did not convince Einstein, who later declared to Klein: "Second quantization, that is sin squared."[108]

Now in agreement about the use of quantized waves in quantum electrodynamics, Heisenberg and Pauli intensified their collaboration, but encountered severe technical difficulties linked with gauge invariance and various infinities. Nonetheless, after distressing interruptions, they finally published in 1929–30 two enormous difficult papers on "the quantum theory of wave fields" giving a general relativistic theory of quantum fields. Quantum electrodynamics was just a particular case where electromagnetic radiation was described by the quantized Maxwell field, and matter by Dirac's field (i.e. a field described by Dirac's equation of 1928 for the relativistic electron) submitted to Jordan-Wigner quantization.[109]

The introduction to the first paper emphasized the advantages of treating matter and radiation on the same footing, and rejected the configuration-space approach for being hardly compatible with relativity and retarded interactions. Indeed, a configuration, being a set of positions at a single time, was not a relativistically invariant concept, as Klein had noticed; and it fit too well with action at a distance, as Einstein had said at the Solvay congress: "The property of forces to act only at small distances has a less natural expression in configuration space than in 3- or 4-dimensional space."[110]

107. Bohr to Pauli, 15 Jul 1927 (*PB,* 167), and 6 Aug 1927 (*PB,* 168).

108. Pauli to Kronig, 22 Nov 1927 (*PB,* 175); A. Einstein, W. Pauli, *Electrons et photons,* (ref. 68), 256–257; "Sin squared" is in Klein (ref. 83), session 2, 4.

109. W. Heisenberg and W. Pauli, "Zur Quantendynamik der Wellenfelder," *ZP, 56* (1929), 1–61, and "Zur Quantentheorie der Wellenfelder, II," *ZP, 59* (1930), 168–190. Cf. Darrigol (ref. 14).

110. Einstein (ref. 108).

With their all wave-like theory, Heisenberg and Pauli were able to give a general, though very heavy proof of relativistic invariance. But to what extent did their use of quantized matter waves bring new predictions or new possibilities? If one considers the general quantum field theory they developed, the answer is largely positive: new particles, new fields to be discovered later, with any spin value, could fit into their framework. Above all, processes annihilating or creating matter were allowed. One could, for instance, as noticed by Heisenberg and Pauli in their second paper, add to the usual Hamiltonian of quantum electrodynamics a term ${}^t\psi_p\ \sigma_{\mu\nu}\psi_e F^{\mu\nu}$ + Herm. conj. giving matter annihilation following the process $e+p \rightarrow \gamma$ introduced by Eddington and Jeans to save the energy balance of stars.[111]

But Heisenberg and Pauli were wrong when they hoped that their theory, once restricted to pure quantum electrodynamics, brought new predictions differing from Dirac's radiation theory. Pauli even thought that he could give a new estimate of secondary γ-ray emission in β decay explaining the continuity of the β spectrum.[112] Illusions were soon dispelled. A young American visitor, Robert Oppenheimer, helped Pauli to apply the mathematical transformation that led back to electron configuration space, which was more suitable to describe atomic systems. Then Oppenheimer eliminated the "longitudinal photons" from the Heisenberg-Pauli formalism to reach a new form perfectly equivalent to Dirac's original radiation theory, except for the use of Dirac's electron. The incompleteness ascribed by Heisenberg, Pauli, and Fermi to Dirac's theory proved to be only apparent.[113]

111. Heisenberg and Pauli (ref. 109), 170; Bromberg (ref. 51).

112. Heisenberg and Pauli (ref. 109), 55, 60–61; also Pauli to Klein, 18 Feb 1929 (*PB*, 216). The probability for secondary γ-ray emission during β decay is proportional to $(e^2/hc)(v/c)^2$, v being the speed of the electron inside the nucleus. This speed has to be relativistic to confine the electron inside the nucleus, which is not the case for α particles. According to Pauli this explained why α spectra were sharp and β spectra continuous.

113. R.J. Oppenheimer, "Note on the theory of the interaction of field and matter," *PR, 35* (1930), 461–477; Dirac's radiation theory seemed to be incomplete because, Heisenberg and Pauli wrote, "it did not give a uniform treatment of [static] interaction and radiation forces" (ref. 109, 55). Fermi expressed the same criticism: "Dirac's electrodynamics is incomplete because it considers only the electromagnetic field of radiation;" Cf. E. Fermi, "Sopra l'elettrodinamica quantistica," Accademia dei Lincei, *Rendiconti, 5* (1929), 881–887, on 881. In fact the electromagnetic degrees of freedom not included in Dirac's radiation field (i.e. the field derived from the vector potential $\vec{A}_1$ in the Coulomb gauge div $\vec{A}=0$) are entirely carried over to the Coulomb interaction, which Dirac takes into account. Then it is clear that the elimination at the quantum level of the longitudinal and time-like photons (every "particle" which is not created by the transverse field $\vec{A}_\perp$) as performed by Oppenheimer and Fermi (ref. 115), leads back to Dirac's theory. For a general proof, cf. O. Darrigol, *La naissance de la théorie quantique des champs, 1925–1948*, Thèse de 3ème cycle (unpublished; Paris, 1982).

Worse than that, Pauli and Oppenheimer demonstrated that the self-energy infinities that had been met several times on the way to quantum electrodynamics were not of a purely formal nature; they definitely spoiled physical things like line spectra. In July 1929 Pauli wrote to Bohr:[114]

> I am *not* very satisfied by the whole theory of Heisenberg and me (although I think that it carries "certain features" of a future correct theory). Particularly the self-energy of electrons makes much more serious difficulties than Heisenberg initially thought [Heisenberg believed that Jordan and Klein's normal ordering trick could be generalized]. Also the *new* results to which our theory leads are mostly dubious and the danger lies near that this whole business will gradually lose contact with physics and degenerate into pure mathematics.

So it became clear to Pauli that matter-wave quantization had not brought anything new in his quantum electrodynamics, and it had not helped to circumvent infinities. His pessimism was contagious for the younger theorists who were already repelled by the overly abstract Jordan-Wigner quantization and by the tedious demonstrations of the Heisenberg-Pauli papers. Oppenheimer later said: "The whole Heisenberg-Pauli approach to electrodynamics is a monstrous 'boo-boo.'" Weisskopf remembered that all these papers using second quantization were "just impossible" for him. And Fermi had earlier decided to build his own quantum electrodynamics independently without any use of quantized matter waves.[115] The simplicity of his approach convinced most people; one of the young men who attended Fermi's lectures at Ann Arbor in 1935, Henrik Casimir, recalled:[116]

> Fermi impressed everyone very much by his very clear lectures on the quantum theory of the electromagnetic field; of course, before that the Heisenberg-Pauli theory and so on had been reported at the Copenhagen conference, but I would say that people found it difficult to work with. . .and I think this Fermi publication turned it into a sort of tool which you could easily solve problems with.

In the early thirties, following the general scepticism against wave quantization, several papers dealt with quantum electrodynamics in configuration space. The most extreme cases were Landau and

114. Oppenheimer (ref. 113); Pauli to Bohr, 17 Jul 1929 (*PB*, 231).

115. R.J. Oppenheimer, interview by T.S. Kuhn (1963), AHQP, session 2, 1; V. Weisskopf, interview by T.S. Kuhn and J.L. Heilbron, AHQP, 7; E. Fermi (ref. 113) and "Nota II," ibid., *12* (1930), 431–435; E. Fermi, "Quantum theory of radiation," *Reviews of modern physics, 4* (1932), 87–132.

116. H. Casimir, interview by T.S. Kuhn and L. Rosenfeld (1963), AHQP, session 2, 18.

Peierls' and Oppenheimer's attempts to come back to configuration space even for light quanta. As noticed already by Heisenberg and Pauli in their second paper, one could always translate wave quantization into configuration-space language, even when particle numbers were not conserved; this could be achieved by introducing a sequence of configuration spaces with any possible number of particles. The procedure was artificial, as Peierls later admitted, and did not throw any new light on infinities.[117]

Coming back to the correspondence principle

Since blind formal developments had proved unsuccessful, both Heisenberg and Pauli decided to come back to the spirit of Bohr's correspondence principle: they decided to keep as close as possible to the classical theory. In February 1931 Heisenberg gave an apparently new version of quantum electrodynamics with the introduction: "In the following I describe a method for the treatment of radiation problems, which relates much more closely than the previous ones to the *intuitive* representations of classical theory and the mechanics of waves, and which consequently directly provides in most cases the result expected from correspondence arguments."[118]

In fact Heisenberg's new theory was equivalent to Heisenberg-Pauli's quantum electrodynamics. From a modern point of view it is obvious that the only change he introduced was the use of "Heisenberg's picture" instead of Schrödinger's. In Heisenberg's picture the operators, here the field operators, evolve classically, and so many classical properties of the field, like propagation and interference, are more obvious than in Schrödinger's picture. This formalism was closely related, as Heisenberg himself noticed, to a pre-Dirac approach to radiation problems invented by Klein, "Electrodynamics and wave mechanics from the point of view of the correspondence principle," where the properties of emitted classical electromagnetic fields were connected by an extra correspondence rule to the Klein-Gordon expression for the electric current: $j_\mu = (\hbar/2mi)\cdot(\psi^*\partial_\mu\psi - \psi\partial_\mu\psi^*)$. But contrary to Klein, Heisenberg was dealing with quantized electromagnetic and ψ fields.[119]

When Pauli, a year later, wrote his magistral handbook article on quantum theory, he traveled back in time so far as to give a detailed

117. Heisenberg and Pauli (ref. 109), 190; L. Landau and R. Peierls, "Quantenelektrodynamik im Konfigurationsraum," *ZP, 62* (1930), 188–200; R.J. Oppenheimer, "Note on the lightquanta and the electromagnetic field," *PR, 38* (1931), 725–746.

118. W. Heisenberg, "Bemerkung zur Strahlungstheorie," *AP, 9* (1931), 338–346, on 338.

119. O. Klein, *ZP, 41* (1927), 407–442.

and perfected exposition of Klein's method. Infinities had made field quantization questionable, but a more intuitive albeit less precise method could still be useful. Consequently, Pauli introduced his quick exposition of the Jordan-Wigner method with a few cautious words:

> This method arose from considering the analogy between material particles with symmetrical states on the one hand, and the light quanta of radiation on the other hand. I doubt this is really a far-reaching physical analogy, and it could also be shown that every result of wave mechanics can also be reached without using this method. As a calculating device at least we must introduce it.

Matter waves, added Pauli, differed essentially from light waves, "because the functions ψ and ψ^* are symbolic quantities, not themselves directly observable, and they contain the quantum of action."[120]

This last remark meant a return to Dirac's viewpoint: only classical observable quantities could constitute a genuine basis of correspondence. Dirac's position had also evolved since the self-energy difficulties. Because he had no doubt about the pictures underlying the correspondence basis (point particles and electromagnetic waves), he came to question the Hamiltonian treatment of the field. Although this treatment was formally equivalent to Maxwell's equations, it had seldom been used in classical physics, where it was unnecessary and cumbersome; now Dirac looked for some fundamental reasons to exclude it.

In 1932 he published "a relativistic quantum mechanics" starting with the following criticism:[121]

> These authors [Heisenberg and Pauli] regard the field itself as a dynamical system amenable to Hamiltonian treatment and its interactions with the particles as describable by an interaction energy, so that the usual methods of Hamiltonian quantum mechanics can be applied. There are serious objections to these views, apart from the purely mathematical difficulties to which they lead. If we wish to make an observation on a system of interacting particles, the only effective method of procedure is to subject them to a field of electromagnetic radiation and see how they react. Thus the only rôle of the field is to provide a means for making observations.

This criticism touched not only Heisenberg and Pauli's quantum electrodynamics, but also Dirac's own radiation theory of 1927. Five

120. W. Pauli, "Die allgemeinen Prinzipien der Wellenmechanik," in *Handbuch der Physik, 24:1* (Berlin, 1933), 83–272, section 15: "Korrespondenzmässige Behandlung der Strahlungsvorgänge," 201–210, on 198, 200.

121. P. Dirac, "Relativistic quantum mechanics," *PRS, 136* (1932), 453–464, on 454.

years had passed, and Dirac now realized that, in spite of his prudence, he himself might have conceded too much to the matter-light analogy by giving the same Hamiltonian status to particles and radiation. He went on:

> *The very nature of an observation requires an interplay between the field and the particles.* We cannot therefore suppose the field to be a dynamical system on the same footing as the particles and thus something to be observed in the same way as the particles. The field should appear in the theory as something more elementary and fundamental.

This was probably an echo of Bohr's long-lived strategy in the old quantum theory: early atomic models could be developed only by stipulating that the radiation observed in spectral measurements obeyed Maxwell's equations in free space. Dirac now reproduced exactly this basic feature of the correspondence method, although he certainly hated to come back to the arbitrariness of old-fashioned correspondence rules in Klein's manner.

In his new approach he described particles by relativistic wave equations (with a different time for every particle to insure relativistic invariance) coupled with an imposed *free* quantized Maxwell field. The wave satisfying this set of equations, being a function of this field, was itself an operator whose matrix elements between field states could be interpreted as probability amplitudes for transitions from an incoming field state to an outgoing one. That the new theory focused on such observable scattering probabilities was, Dirac emphasized, a satisfying departure from Heisenberg-Pauli's theory, where the evolution of the field during a scattering process, though unobservable, could be calculated: "the Heisenberg-Pauli theory thus involves many quantities that are unconnected with the results of observations and that must be removed from consideration if one is to obtain a clear insight into the underlying physical relations." This utterance recalls the strategy of the inventor of matrix mechanics. Heisenberg had rejected unobservable classical trajectories inside atoms, Dirac now rejected any dynamical description, even symbolic quantized ones, for field evolution during interactions. Later on, Heisenberg would justify his scattering matrix approach to particle physics by the same kind of philosophy.[122]

It did not take more than a month for Léon Rosenfeld to publish a proof that Dirac's fundamental criticism had generated a theory equivalent to Heisenberg-Pauli's. In modern terminology we would say that Dirac had introduced the "interaction picture" for the

122. W. Heisenberg, "Die 'beobachtbaren Grössen' in der Theorie der Elementarteilchen," *ZP, 120* (1943), 513–538, 673–702.

electromagnetic field, whereas previous quantum electrodynamicians had used Schrödinger's or Heisenberg's picture. Nevertheless, to assist in the further development of the theory (in the 1940s Tomonaga and Schwinger were helped by Dirac's multiple times), Dirac found it worthwhile to simplify Rosenfeld's proof and to complete his new form of quantum electrodynamics with the help of Fock and Podolsky. Thanks to the introduction of Fermi's method of the quantization of the Maxwell field, Dirac reached a very "beautiful" set of equations:

$$\begin{cases} i(\partial^i_\mu - e_i A^i_\mu)\gamma^\mu\psi = m_i\psi \\ \partial^2 A_\mu = 0 \end{cases} \qquad \begin{cases} [\partial A - \sum_i e_i \Delta(x - x_i)]\psi = 0 \\ [A_\mu(x), A_\nu(y)] = i g_{\mu\nu}\Delta(x-y) \end{cases}$$

where every symbol represented intrinsically relativistic concepts, a precious flower of Dirac's esthetics. A look at the equations was enough to show that the corresponding theory was relativistically invariant.[123]

In this formalism the relativistic extension of the notion of configuration was decisive: a set of positions $\vec{x}_i$ had to be replaced by a set of 4 vectors x_i including multiple times t_i. Ironically, matter-wave quantization thus proved to be useless even for a relativistic formulation of quantum electrodynamics. Starting from the opposite viewpoint Dirac had produced an amazingly simplified version of this theory, although one still poisoned by implicit infinities.

New particles

In 1932–33 the discovery of two new particles, the neutron and positron, brought some new blood to the sick quantum field theory. Dirac reworked the hole theory he had ceased to believe in the year before the positron's discovery, and started to calculate vacuum polarization effects considered as displacements in the virtual infinite sea of negative energy electrons. These phenomena changed the properties of quantum electrodynamics qualitatively, and not only when actual positrons were present. Following Dirac, both the German school with Heisenberg and his students Euler, Kockel, and Weisskopf, and the American school with Oppenheimer and his students Furry,

123. L. Rosenfeld, "Über eine mögliche Fassung des Diracschen Programms zur Quantenelektrodynamik und deren formalen Zusammenhang mit der Heisenberg-Paulischen Theorie," *ZP, 76* (1932), 729–734; P. Dirac, V. Fock, and B. Podolsky, "On quantum electrodynamics," *Physikalische Zeitschrift der Sowjetunion, 2:6* (1932), 468–479, on 468.

Uehling, and Serber worked out systematic methods to fish physical results from the infinite Dirac sea. There was even some hope that the vacuum polarization infinities might cancel self-energy infinities.[124]

A large part of this work, again following Dirac's example, used the picture of a set of negative electrons. When used, second quantization functioned as a mathematical tool for dealing with the many-electron problem first treated by Dirac in configuration space. The Dirac field, when quantized, was developed into a sum of annihilators of electrons with both energy signs, and the positrons were still interpreted as lacunae in the "normal" vacuum state, with all negative energy states filled up.

But there was also a more progressive trend: Oppenheimer, evidently influenced by Bohr, started his own version of positron theory (with Wendell Furry) by a criticism of the notion of configuration. Knowledge of the number of electrons and positrons was complementary, in Bohr's sense, to detailed knowledge of their space configuration, because too precise a localization of a particle creates parasite pairs around it. In true physical situations the number of particles was not completely determinate, and the old concept of a configuration of a fixed number of particles was inapplicable. Furry and Oppenheimer wrote in December 1933: "This circumstance means that, again as in the case of the electromagnetic field, the formal aspects of the theory are best studied by abandoning the wave functions defined in configuration space and by using the methods of quantized waves in actual space."[125]

In November 1933 Fock showed how to develop the quantized ψ into annihilators of electrons and *creators* of positrons with positive energy signs, and Heisenberg, inspired by his earlier theory of incomplete atomic shells, used the same formalism in the last part of his big paper on positrons of 1934. Apparently, only Fock and his Russian collaborators realized at that time that this was not only a formal rewriting of Dirac's concepts, but a way to dry up the infinite electron sea altogether.[126]

Even Pauli apparently missed the point and continued to disparage what he called "subtraction physics." In 1934 he welcomed an idea of his assistant Victor Weisskopf's: the spinless Klein-Gordon wave,

124. P. Dirac, "Théorie du positron," in *Structure et propriétés du noyau atomique*, 7th Solvay congress (1933), *Rapports et discussions* (Paris, 1934), 203–212.

125. W. Furry and R. Oppenheimer, "On the theory of electron and positive," *PR, 45* (1934), 245–262, on 246.

126. V. Fock (ref. 104); W. Heisenberg, "Bemerkungen zur Diracschen Theorie des Positrons," *ZP, 90* (1934), 209–231; also Heisenberg to Pauli, 17 Jul 1933 (*PB,* 316).

when properly quantized, could give rise to pair-creation and vacuum-polarization effects. Pauli called his paper with Weisskopf the "anti-Dirac paper" because pair creation appeared there as a consequence of matter-wave quantization, and no longer as a jump out of an infinite sea. That this theory was very similar to Fock's version of positron theory was then noticed by the Russian school. Nevertheless, negative energy particles survived even in Wentzel's famous textbook on quantum field theory of 1943. In this context the use of quantized electron waves was still subordinate to a many-particle picture supposed to be more fundamental.[127]

Fermi was the one who struck the strongest blow, in 1933, against this kind of classical picture of conserved particles. To be sure, he disliked abstract formalisms, he was repelled by the Jordan-Wigner quantization, and he discarded it when building his own quantum electrodynamics. But the situation had changed after the neutron's discovery and Pauli's neutrino proposal. Then β decay appeared as an elementary process in which the neutron was annihilated and three other particles created, a proton, an electron, and a neutrino. Energy conservation was secured by the unobserved neutrino, but particle-number conservation was violated by supposing, as Bohr and Fermi did, that the electron (and the neutrino) did not pre-exist inside the nucleus. In Fermi's hands, wave quantization became a necessity linked to the non-conservation of particle number. He was not concerned with the methodological or epistemological roots of Dirac's and Jordan's conflicting choices, but with the efficiency of a given formalism in solving a particular problem. His treatment of β decay, one of the most durable physical theories of the century, represented the first paradigmatic use of matter-wave quantization. And this time nobody tried to find for electron-neutrino creation what Dirac's sea was to electron-positron creation.[128]

Present textbooks on quantum field theory, if they try to justify matter-wave quantization, prefer Fermi's physical argument of an unconserved matter or Weyl's and Fock's technical charge-symmetry argument. Jordan's claim to have derived the very existence of

127. W. Pauli and V. Weisskopf, "Zur Quantisierung der skalaren relativistischen Wellengleichung," *Helvetica physica acta, 7* (1934), 709–731; "Subtraction-physics," in Weisskopf to Heisenberg, 3 May and 16 May 1934 (AHQP); "Anti-Dirac," in Pauli to Heisenberg, 14 Jun 1934 (*PB,* 373), and Weisskopf (ref. 115); D. Ivanenko and A. Sokolov, "Bemerkungen zur zweiten Quantelung der Dirac-Gleichung," *Physikalische Zeitschrift der Sowjetunion, 11* (1937), 590–596; G. Wentzel, *Einführung in die Quantentheorie der Wellenfelder* (Vienna, 1943).

128. E. Fermi, "Tentativo di una teoria dell'emissione dei raggi 'beta,'" *Ricerca scientifica, 4* (1933), 491–495; "Tentativo di una teoria dei raggi β," *Nuovo cimento, 2* (1934), 1–19; "Versuch einer Theorie der β-Strahlen, I," *ZP, 88* (1934), 161–171.

particles is usually left out, perhaps as too “philosophical.” Still, the most profound conceptual change brought up by early quantum field theory and prepared by Louis de Broglie might well be the substitution of wave excitations for classical, individually identifiable particles.