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AN INVESTIGATION OF THE MATHEMATICAL FORMULATION OF QUANTUM THEORY AND ITS PHYSICAL INTERPRETATION 1900-1927

by

JOHN LOVAT HENDRY

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ABSTRACT

Between 1900 and 1930 there was a dramatic revolution in the ideology of physics. In 1900 this was objective, causal and structurally consistent, but by 1930, after the establishment of quantum theory and relativity, it had become subjective and acausal, all hope of a structurally consistent description having been abandoned.

This thesis is concerned with the development of quantum theory, and through an analysis of both its mathematical formulation and its physical interpretation an attempt is made to determine the ideological changes that took place, when, how, and, as far as possible, why.

The history of quantum theory as previously written has been mainly concerned with the technical details of the theory and its successful predictions, rather than with the conceptual, methodological and ideological elements and the problems and paradoxes associated with them. The present thesis concentrates on the latter aspects. A large part of the thesis is consequently devoted to certain features of the development of quantum theory that did not appear to have been previously studied in detail. Areas where the existing studies seem to be either inadequate or misleading have also been investigated. Among the original parts of the thesis are critical discussions of Planck's 1900 derivation of the black-body radiation law, the origins and development of the concept of wave-particle duality, the quantum conceptual crisis of 1921-3 (including de Broglie's work and the causality issue), the origins of the Bohr-Kramers-Slater theory and of Heisenberg's quantum kinematics, the joint origins of transformation theory and the Uncertainty Principle, and the development in 1926-7 of different interpretations of quantum theory.

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OR

THE INTRODUCTION OF QUANTUM IDEOLOGY

"Every sentence I utter is to be understood as a question and not as an affirmative"

BOHR

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INTRODUCTION

CLASSICAL PHYSICS, PHILOSOPHY AND METHOD

Nineteenth century physics was characterised, if we are to believe the popular historians, by the universal belief in an objective world, governed by the principle of causality and administered by the laws of Newtonian mechanics. This world was composed of particles, interacting through collisions or through action at a distance, and aetherial fluids, transmitting disturbances through mechanical stresses and strains. The aim of physics was to describe it, consistently, in these terms.

Any simplistic characterisation of the above kind has its faults, the most notable being that the best scientists are often just those who are exceptional, who differ from the norm. But, as a first approximation, the above characterisation is remarkably accurate, especially for the period up to 1860. The electromagnetic and thermodynamic theories then current were those of Poisson and Ampère, of Clausius, Weber, Thomson (later Lord Kelvin), Kirchhoff, Helmholtz and Faraday, and all these physicists seem to have shared the same broad perspective. Within this perspective, however, there were naturally differences, and these were as important for the future as were the common elements: to understand them we must look briefly at the physics of the previous century.

At the beginning of the eighteenth century, there were two major schools of physics, one represented by Newton and his followers, the other by Huygens and the Cartesians. Both schools adopted a corpuscular mechanical world-view, in which matter and aether were supposed to be ultimately particulate, but they disagreed strongly on one crucial point. Newton accepted the existence of a void between the particles, and postulated in accordance with this that they interacted through action at a distance (or of course through collisions).[1] The Cartesians, on the other hand, could abide neither the concept of action at a distance nor that of the void, and they postulated an aether that was, in conjunction with matter, everywhere dense, with transmission of forces only through contiguous action between the particles.

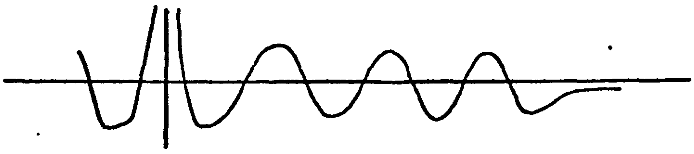
In line with their views on the nature of matter and aether, Newton and Huygens also disagreed on the nature of light. In Huygens' conception, light could not be particulate, for the everywhere dense medium would constantly offer a resistance to its motion that would be incompatible with its enormous speed of propagation. It had therefore to be a manifestation of a disturbance, or wave, propagated through the aether by contiguous action. In Newton's theory, on the other hand, there was no everywhere dense medium to support such a disturbance, and light could only be interpreted as a particulate form or as the manifestation of an action at a distance, like gravitation. But whereas the gravitational attraction between two bodies was a general phenomenon, symmetric and independent of any intervening matter, the propagation of light was directed and highly dependent upon the intervening matter. Newton's theory thus led to the hypothesis that light was of a particulate nature.

The essence of the disagreement on this subject was that to Huygens light was the manifestation of a disturbance in the medium, with no existence apart from the medium, while to Newton it was an independent existent, a 'thing-in-itself'. But the terminology of the debate was that of waves and particles, and although these are not really commensurable terms,[2] and although Newton in fact recognised the need for a wave aspect to light as well as the particulate one, this terminology has been continued by both historians and physicists: I shall therefore follow suit.

As the century progressed, the ultimately particulate nature of matter and aether was universally upheld and the general consensus, following the usual conception of Newton's theory, was that light was also particulate. Newton's treatment of gravitation as action at a distance also became generally accepted, and was developed in an important way by Boscovich. Thus the Cartesian theory was eclipsed by the Newtonian, but not before its last great proponent, Euler, had prepared for a future rebirth by commencing the development of the mathematical theory of contiguous action. Although coming from opposing schools, the developments of Euler and Boscovich in fact had much in common, and since they led on to the physics of the nineteenth century I shall outline them here.

Although the Cartesian theory was ultimately particulate, its mathematical description could only be in terms of fluids: one simply could not analyse the transmission of continuous action by treating each particle individually. One reason for the dominance of Newton's theory was the very practical one that its mathematics was developed, whereas that of fluids was not, and this is partly why Euler's work on the mathematics of fluids was so important: he was the last of the Cartesians, but he also provided the foundation for the wave theories of the nineteenth century. His work was also important in another way, though, for he explicitly distinguished between two assumptions: the metaphysical one of an ultimately particulate medium, and the heuristic one of an idealised fluid medium. He argued that the latter provided a more suitable basis for the solution of many physical problems, and accordingly employed and explored it. This explicit separation, and the resulting concern with an idealised mathematical behaviour, continuously defined for all points in space-time, rather than with a hypothetical mechanical substructure, marks Euler's theory as an early example of a field theory, and as such it had many properties that are often associated with field theories in general. It was a theory of contiguous action, propagated non-instantaneously (the speed of propagation being determined by the properties of the fluid), and the field was actual (as opposed to potential) in that its behaviour was supposed to exist not only mathematically but also physically, in the fluid medium, at all points.

Boscovich's development was also towards the field concept, but within the Newtonian conception of particles in a void and action at a distance. He developed this conception by reducing particles to point-masses and considering the behaviour produced by such particles ("the various motions that arise") rather than the particles themselves, illustrating this behaviour in his famous diagram:



Like Euler he thus concerned himself with the mathematical behaviour at all points rather than with the mechanical substructure (in this case of the particles); like Euler too, he adopted for this an idealised description (i.e. point-masses). But despite these similarities his theory was clearly very different from Euler's. It was a theory of action at a distance, the propagation of action was instantaneous, and the field was potential: Boscovich's behaviour did not define an actual behaviour at all points — indeed there was nothing at most points to behave, only the void — but rather the behaviour that would arise if something were there.[4]

By the end of the eighteenth century, therefore, there were two types of field theory,[5] one corresponding to an idealised Newtonian and one to an idealised Cartesian metaphysic. In the early part of the nineteenth century, Fresnel developed the mathematical theory of fluids sufficiently to allow him to reintroduce the wave theory of light. This was strongly supported by Young's interference experiments, and before our 1860 cut-off date it had been confirmed by the measurements of the speed of light by Fizeau and Foucault.[6] Thus the Cartesian theory, in its idealised form as a theory of fluids, had by 1860 made a comeback, and it stood once again in opposition to the Newtonian theory. For gravitational phenomena the universe was Newtonian, for optics it was Cartesian, and in the realm of electromagnetic theory the two world-views clashed.

At the end of the eighteenth century, the most important theories of electricity had been Newtonian fluid theories, the fluids being composed of minute particles (interacting at a distance) in the void; but these theories were not amenable to mathematical treatment, and the first quantitatively successful theory arose in the early nineteenth century from the pure Newtonian action at a distance approach of Poisson and Ampère.[6a] By 1860, this approach was still dominant, but it had already run into problems, the most important of which had arisen from the suspicion that electromagnetic forces were propagated non-instantaneously. On the one hand the concept of non-instantaneous action at a distance was thought to be generally unsound, while on the other hand the all-pervading fluid aether, which could carry contiguous action, was anathema to the continental physicists involved with the problem. This problem could not be solved in a physically satisfying way, but it did lead to an important conceptual development. A solution of some kind had to be found, and the procedure chosen was to endow the mathematical field with the necessary properties for non-instantaneous contiguous action, but without supposing these properties to be attached to any physical medium; the metaphysics remained the Newtonian one of action at a distance, but the field theory used to determine the mathematical behaviour was the essentially Cartesian[7] one of contiguous action. Rather confusingly, the mathematical term "field potential" was carried over into the new theory, which was of an actual, not a potential, field. A similar realignment of concepts was also proposed by Faraday. He, like the continental physicists, adopted a contiguous action field concept, and like them also he declined to attribute the field behaviour to an aetherial fluid medium; but whereas they were concerned primarily with the mathematics and only secondarily with a structural visualisation, his priorities were reversed. He therefore adopted the Boscovichian viewpoint that matter should be characterised by its effects rather than by its source, and treated the field as the prime physical existent; the particulate conception of matter was treated as an abstraction, and the aether concept was abolished altogether.

By 1860, to summarise, the field could be seen as a derived concept, potential (Boscovich, gravitation), actual with a substantial support (Euler, Fresnel, optics), or actual with no physical support (the "pure field" approach — continental physicists, electromagnetic theory); or it could be seen as the prime existent (Faraday, electromagnetic theory). Naturally in this situation there were differences of opinion among physicists as to which conception was most important, and as to whether the mathematics or physics was most important, and there was also a divergence of opinion on the question of mechanical hypotheses. The most common methodology of the period, especially among the British physicists, was what might be called the 'suggestive' or 'structural' approach. The results of experiment and observation were used, according to this approach, to suggest a speculative hypothetical mechanism, usually concerned with the substructure of matter or the aether; the implications of this hypothesis were then worked out, and used to suggest further experiments which were used to suggest further hypotheses and so on. The idea was that although the successive hypotheses were themselves discarded, some firm knowledge was added at each stage, and a realistic mechanical description gradually approached.[8] It was a successful and generally popular methodology, but it was not to everyone's liking, especially as the growing importance of field theories stressed mathematical behaviour rather than the mechanical substructure on which the hypotheses were based, and some physicists, most notably Kirchhoff, rebelled against it. They did not give up their mechanical conception of the world, but they restricted their adoption of mechanical hypotheses to those that seemed both reasonable,and necessary for the advancement of science. The mechanical world-view had not been seriously opposed by 1860, but an element of positivism had begun to make itself felt.

My reason for choosing 1860 as the terminal date for the above analysis is that it marks the publication of the first of a series of contributions that revolutionised physics. Their author was Maxwell and he started with kinetic theory which, in his hands, was to provide a new basis for thermodynamics. The most advanced treatment of kinetic theory at the time was that of Clausius, who had treated it as a Newtonian theory of molecular particles moving in an effective void and interacting through collisions; but since he could not analyse the motions of individual particles he had assumed them all to move with the same speed, and this had limited the scope of the theory. Maxwell saw that to develop the theory further it would be necessary to take into account what he saw as obvious, that the speeds of the molecules would change in collisions and vary from one molecule to another. The only way to do this, he saw, was to treat the particles statistically, calculating the distribution of velocities to which a system would finally tend and proceeding from there. This statistical approach was developed both by Maxwell himself and by Boltzmann, who introduced the idea of a most probable distribution, identified with maximum entropy, in place of that of the final one. It proved an extremely successful theory and it was within the mechanical world-view as presented at the start of this analysis, but it raised important problems, and it had its opponents.

Perhaps the strongest opponent of statistical mechanics was Planck, but since the reasoning behind his stance was highly personal I shall consider it only later in the context of his own work. The other principal opponent of the theory was Mach; his objections were, in a general context, more important than Planck's, and they stemmed from a fundamental opposition to the philosophy of science as then practised. It is interesting to note that the physicists of today rarely have any philosophical background, whereas those of the last century were generally well educated not only in philosophy, but also in the arts, in literature, and even in theology: it is interesting because, whereas the physics of today is intimately tied up with serious philosophical problems, that of the last century could be largely divorced from such problems. Thus, as Eddington has remarked, the philosopher-physicists of the last century, up to and including Poincaré, managed to keep their interests quite distinct.[7] Mach was an exception. Like other prominent philosophers of science (Bacon, for example, or Kant) he himself contributed little to science,[9] but as a professional scientist he impressed his viewpoint upon his colleagues, and forced them to consider it.[10] Its main feature was his phenomenalist ontology (and, a fortiori, epistemology), which was quite incompatible with the prevailing mechanical world-view. Mach argued that the kinetic theory, in treating gases as particulate, was making a quite unjustified assumption, for there was no evidence that the particles existed, or even appeared to; the same objection was raised by Ostwald and others, and although these opponents of the traditional world-view were in a clear minority they made their position widely felt.

A second interaction between physics and philosophy concerned the use of statistical methods; this raised the question of causality, a far more basic ingredient of the traditional world-view than either atomism or the use of structural hypotheses. However, although the philosophies of the late nineteenth century were generally unfavourable to causality,[11] only Maxwell himself, the exceptional physicist to whom generalisations do not apply, seems at this time to have considered seriously the possibility of a contingent, as opposed to a deterministic world: and he rejected the possibility.[12] So far as the overwhelming majority of physicists were concerned, physics in general and the individual molecular motions in particular remained fully causal.

Maxwell's second great innovation was his electromagnetic theory, and this had a variety of methodological repercussions. In line with tradition, he adopted the 'suggestive' approach, using Faraday's results and conceptions to suggest a structural mechanism, and this mechanism to suggest further behavioural features and mathematical field equations. But he then dropped part of the mechanism and put the theory across largely in terms of the mathematical equations. Moreover, although any conceptualisation of these equations required the reintroduction of a mechanical framework of some sort (a basis to which descriptive terms could be applied), he did not supply such a framework, and this emphasised the separation of mathematics and metaphysics and the prime importance of the former. This tendency was continued by Hertz, who explicitly reduced to a minimum the role of mechanical structure, and tried to replace the mechanical concept of force with the behavioural one of motion. He had to hypothesise hidden motions, linking the observed cause and effect, and he retained a belief in the ultimately particulate metaphysics, but he was still able to earn Mach's limited approval and this indicates the direction in which he was proceeding. Generally, the work of Maxwell and Hertz encouraged a positivistic attitude to physics, but on a methodological level rather than on the philosophical level of Mach.

Maxwell identified electromagnetic waves with light waves and this was confirmed experimentally by Hertz, so the new electromagnetic theory became one with optics, emphasising the Cartesian side of its admixture. An attempt to synthesise the disparate elements in physics, and in particular in electromagnetic theory, led in the 1890's to the electron theory of Lorentz. Gravitational theory and the old electromagnetic theory depended on the effectively particulate nature of matter, and optics and the later developments of electromagnetic theory on an effectively fluid aether (i.e. on continuous field equations with an actual field), and Lorentz combined these two conceptions. His analysis convinced him that electric charge was necessarily particulate, as in the old electromagnetic theory, and that an all-pervading aether of the type used by Fresnel was necessary for optics, so he postulated these as the structural bases of the physical world, building up a theory in which charged particles appeared as singularities in the Maxwell-Hertz field.

Lorentz's synthesis proved very successful in explaining the phenomena, and by the end of the century, after J.J.Thomson's experimental 'discovery' of the electron, it had become the most generally accepted theory of physics. But while the conceptions of 1860 had allowed several variations, those of 1900 were full of them. To the suggestive methodology of Faraday and others and the more cautious approach of Kirchhoff was added the phenomenalism of Mach; to the range of possibilities concerning the aether (the pure field, the reality behind matter, the Newtonian or Cartesian fluid) were added a variety of interpretations of matter, which could be seen as an entity separate from the aether (Lorentz), a structure in the aether (Larmor), or a singularity in the aether as pure field; the aether itself, in its mechanical form, had aquired a wonderful range of possible substructures. Most important, in 1900 the mechanical world-view itself came under attack. Encouraged by the success of his theory, Lorentz suggested that the aim of explaining phenomena such as electromagnetism in terms of mechanical structures and forces might be replaced by that of explaining gravitational forces, still outside his synthesis, in terms of electromagnetic effects; in the early years of the twentieth century this suggestion was developed by Wien and a fairly significant group of physicists, mainly Germans,[13] as the electromagnetic world-view. These physicists (Lorentz soon abandoned their ideals, and seems never to have shared their methodology) retained the concept of material particles, so long as these were charged, but abandoned the mechanical metaphysics of the aether: a natural development of the growing domination of this aspect by the mathematical field theory, which was now taken as self-sufficient.

These were the variations. Having noted them, however, we must return to the similarities, and to the dominant elements of the physical perspective of 1900. First, although it was challenged strongly by the electromagnetic viewpoint, the mechanical perspective still dominated physics. Even the proponents of the electromagnetic world-view, moreover, had to adopt something of the mechanical theory (the concepts of electric particle and aetherial fluid) if they were to give any physical description at all of their equations. The Kirchhoffian methodology, confining the use of structural hypotheses to the provision of a basis for a theory and its description, had increased in popularity, but the 'suggestive' approach was still widely used. Mach's phenomenalism, though widely discussed, was not widely adopted, and the other positivistic theories of the period had made no impact at all on physics in general. This remained, against these philosophies, fully deterministic, and it also retained the two other fundamental aspects of its ideals. That one should be able to describe a situation precisely for a given point in space-time (if not in terms of mechanical substructure, at least in terms of electromagnetic field properties) had not even been questioned; and nor had the possibility of achieving, eventually, a description of the world that was consistently unified, not only mathematically but also in terms of a structural description.

We may conclude that our original characterisation of nineteenth century physics is less true for 1900 than for 1860, but that even at the later date it is nevertheless fundamentally correct. In the next thirty years, however, physics changed dramatically; in the theories of quanta and relativity each aspect of our characterisation came under attack, and each seems to have been defeated. Thus, turning once again to the popular historians, we find that physics by 1927 (and of 1977) was characterised by philosophical and practical subjectivity; a real world independent of the observer, even if supposed to exist, was not supposed to be susceptible to science, the law of causality was no longer supposed to hold, and a consistent structural description of phenomena was no longer believed to be possible.

This remarkable turnabout has been a feature of popular history for many years, but serious historians have only recently approached the problem. Their first investigations, moreover, have necessarily concentrated on relatively narrow areas, such as individual personalities and technical details. What I shall do here is look, for the first time, at the broader issues involved: I shall ask just how the characterisation of physics did in fact change, at what stages in the development of the new theories the individual changes occurred, and what produced them.[14] Since the answering of these questions involves the filling in of many gaps in the detailed history of the subject, as well as the reassessment of some areas already but inadequately treated, I have had to restrict myself in the first place to half of the problem. I shall consider here only the quantum theory, from its inception circa 1900 to its fruition in quantum mechanics, marked by the Solvay Congress of 1927.

I

THE INTRODUCTION AND DEVELOPMENT OF QUANTUM CONCEPTS (1900-20)

I.1. THE INTRODUCTION OF QUANTUM CONCEPTS

One aspect of physical conceptualisation that appeared to be completely unassailable in 1900 was the belief in the wave nature of light. Within five years, however, this had been challenged by no less than three independently conceived theories: Planck's theory of 1900, J.J.Thomson's of 1903, and Einstein's of 1905. Concerning this remarkable turnabout, historians tell us that Planck introduced the concept of energy quanta, that Einstein introduced that of light-quanta, and that Thomson did something rather odd that can be ignored; Einstein's hypothesis was apparently derived from his search for unity and symmetry in physics, and Planck's — well, despite many efforts, they haven't actually told us yet the source of Planck's hypothesis. These theories mark the beginning of quantum physics; they are naturally important. I shall therefore analyse in this section just what hypotheses they involved and how they arose.

Planck[1]

Planck's theory of 1900 has been the subject of many historical treatises; his work prior to that date has also been thoroughly analysed. Due, however, to an apparent barrier between the two subjects, the 1900 theory has never been adequately explained. By means of a sort of historical quantum barrier penetration, I have attempted such an explanation (see Appendix A), and the results are very interesting.

Apart from Mach and his followers, the physicists of the nineteenth century believed in a real objective world, independent of the observer, and in Planck this belief acquired almost religious dimensions. He attributed to the 'real' world a quality of 'absoluteness' and, seeking a reflection of this quality in physical laws, he lighted on the second law of thermodynamics and the concept of entropy. Ironically, this led him to join Mach in opposition to the statistical theory of thermodynamics, for a statistical definition of entropy was quite incompatible with his conception of it as an absolutely increasing entity. He entered into a long debate with Boltzmann on the subject, arguing that all mechanical laws were reversible and that thermodynamics, being concerned with irreversibility, could not therefore be mechanically founded. In the course of this debate, however, he came to realise that his own theory (based on continuous matter) also needed additional assumptions if it were to be irreversible, and that the technique (as opposed to the metaphysics) of statistical mechanics was more potent than that of his own theory. When he was faced in 1900 with a theoretical derivation of his radiation law, which he had deduced empirically, he therefore adopted the statistical technique, and set out to deduce the additional assumptions necessary for irreversibility, in the form of the black-body radiation law, to be achieved. In the course of his investigation he naturally adopted the mathematical assumption of discrete units of energy, and it was this assumption that marked the introduction of quantum concepts to physics.

Although he had adopted the technique of statistical mechanics, however, Planck did not change his metaphysics. Whereas Boltzmann had started with a formula for probability and derived one for entropy, Planck proceeded explicitly in the opposite direction. His choice of probability formula was a deduction as to what assumption would be necessary to derive the radiation law if the statistical technique were adopted, and the same was true of his discrete energy hypothesis: this carried no physical significance whatsoever, for he was still firmly opposed to the whole of the physical interpretation behind the statistical technique. This point is emphasised by the complete mix-up of structural assumptions in his derivation, which was in two parts: first he derived a formula for the energy distribution over resonators in a cavity (Appendix A, eqn.[10]), assuming the energy to be in discrete units treated probabilistically as non-independent, and then he deduced the radiation law (eqn.[11]) on the assumption of the continuous classical theory of light (eqn. [3]). Other features of the derivation compounded this confusion (which was not in fact sorted out for many years) but it appears that Planck was not even aware of the assumptions that he had made. He was concerned neither with the structure of light (light-quanta), nor with that of energy (energy-quanta), nor even with the nature of radiative processes (action-quanta), but only with the mathematical behaviour appropriate to thermodynamic irreversibility and the black-body radiation law.

Even when writing his famous paper, Planck did not apparently realise that he had introduced a discrete energy assumption. When he did realise this, he tried desperately to rectify it, and to reconcile his derivation with the classical electromagnetic wave theory, of which he was in fact one of the strongest adherents. The first introduction of a quantum concept to physics was thus quite accidental and thoroughly regretted, and although it stemmed directly from an opposition to the classical (atomic) conception of matter it's origins were actually in a desire to preserve the classical philosophy of physics, and, indeed, the continuous nature of light.

Thomson [2]

Thomson's introduction of a quantum conception arose (as I have explained more fully in Appendix B) in connection with the probelm of X-ray ionisation. Within the common classical conception of matter as particulate and light as a wave form the discovery of X- and γ-rays caused problems, for they appeared to have both particle and wave properties. The general conclusion was that they must be composed of aether-pulses, of essentially the same nature as light, and spreading out spherically from the source as light did, but consisting of individual irregular vibrations instead of continuous streams of regular ones. This still left the problem, however, of how the rays could selectively ionise, as they did, some of the molecules of a gas and not others, for the ionisation appeared to be independent of the state of the molecules. Guided by this problem, Thomson revived Faraday's suggestion of physical discrete lines of electric force, translated it into one of discrete tubes of force, and combined it with the aether-pulse conception. He supposed that radiation waves (be they light, X-rays or γ-rays) could be transmitted only along the tubes and not through the aether lying between them. X-rays and γ-rays were thus restricted to the intersections of these tubes with the spherical pulses, and, since the tubes stretched outwards from the source of the rays, the effect was to localise the rays in all directions.

Thomson did not postulate a completely localised light-quantum, but he did suppose the light to be localised along the wave front. Since this ensured that it was localised in its impact on matter, and since, for an entire emission process, he took the energy within a tube to be fixed, his idea did however have the most important characteristics of a quantum conception; the light was effectively "done up into bundles", of fixed and immutable energy. Unlike Planck's innovation, Thomson's was conscious and intended, but even more than Planck's it was made within the classical framework. Thomson was a strong adherent of the 'speculative' approach to physics, and his hypothesis was a structural speculation (upon the aether) entirely within this approach, and within the classical philosophy that had always accompanied it. It was of course anathema to adherents of the electromagnetic world-view, and to those of Kirchhoff's non-speculative methodology; moreover, most of those who shared Thomson's methodology objected strongly to this particular hypothesis. But in no way was the hypothesis new in any philosophical or methodological sense, and even in respect of metaphysics it preserved the basic classical characterisation: radiation was still supposed to be essentially wavelike, a disturbance in a medium, and was not afforded any independent existence.

Einstein [3]

In 1905, independent of both Thomson and Planck, but in the knowledge of the latter's work, Einstein introduced explicitly the concept of discrete particles of light. This was the most important of the new quantum concepts, and it was introduced, as has been analysed well by McCormmach,[4] as part of a far-reaching revision of physics.

Einstein's methodology was similar to Kirchhoff's, for he accepted the classical belief in a real objective world that was causal and mechanically visualisable, and also accepted the need for a structural basis for its description, but he did not make use of purely speculative structural hypotheses. He was influenced by Mach, and recognised the limitations of observation, but rejecting Mach's restrictive phenomenalism he saw the aim of physics as being to surmount these limitations. This perspective enabled him to distiguish between observed and real worlds and so penetrate more clearly the nature of the latter, which he strongly believed must be characterised in terms of simplicity and consistency. Looking for these qualities, he was attracted as Planck had been to thermodynamics.

Looking at thermodynamics, Einstein recognised immediately the heuristic value of the kinetic theory, and his published work prior to 1905[5] consisted of the application of a mechanical theory (explicitly analogous to that of gravitation) first to the theory of molecular forces and then to the kinetic theory. However, unlike previous students of that theory, he was so concerned with the problem of consistency that he was not prepared to adopt the atomic hypothesis it entailed, without first justifying it. He felt it necessary to show that the hypothesis was both feasible and consistent, and to this end he investigated,[6] in 1904, the thermal energy fluctuations whose existence was necessary for this to be so. In the course of this investigation, he saw that nothing in the theory of the fluctuations seemed to depend upon the material body possessing the energy, and this prompted him to consider the fluctuations of pure radiant energy, i.e. of black-body radiation. The results indicated a close analogy between material and radiant fluctuations. At about this time,

Einstein had also become interested in Lorentz's theory, and especially in Drude's related work, in which the equipartition law of the kinetic theory was applied to systems of electrons. Encouraged by his fluctuation results, Einstein applied the kinetic theory to black-body radiation and derived Rayleigh's law, which, he realised, must be incorrect. This was not a new result, since it had been reached by both Rayleigh and Lorentz, but Einstein's work was independent. Moreover, his studies had given him a unique view of physics, which, combined with his belief in unity and simplicity, and with his natural brilliance, allowed him to draw revolutionary conclusions.
Most physicists, had they been prepared to face up to the fact that the kinetic theory and the electromagnetic theory were shown to be incompatible, would have abandoned the kinetic theory. But Einstein's studies had given him great faith in the kinetic theory, while he was relatively unfamiliar with the electromagnetic theory.

From this viewpoint, the Lorentz theory was characterised not by its success (as it was for most physicists) but by its intrinsic asymmetries and complications. From a mathematical viewpoint it ran into problems with infinite self-energy (the field energy at the points representing electrons was infinite — a problem with any idealised field theory of particles). Physically, it necessitated the hypothesis of a motion-dependent ('Lorentz') force. Other features, arising in part from the need to explain the negative results of the Michelson-Morley experiments on the effects of the earth's motion through the aether, included the dependence of the mass of an electron upon its speed and the deformability of the electron (Lorentz-Fitzgerald contraction).

Einstein was in the almost unique position of being prepared to doubt the correctness of the electron theory. He was convinced that the kinetic theory was applicable to radiation, but it was clearly not so to classical radiation, so his reaction was to consider modifying the supposed nature of radiation, and this led him to the light-quantum concept. With this concept, he realised, the clash between discrete and continuous elements in physics could be resolved, and with it many of the problems of the electron theory. The stationary aether was no longer necessary, and with this disappeared the [illegible] resulting asymmetries. The properties previously explained by the Lorentz force could be derived from a principle of relativity, and the hypothesis of a universal constant speed, c, for light.

Thus, in 1905, Einstein published a paper on light-quanta,[7] and another on special relativity;[8] another developed the theory of Brownian motion[9] in support of his insistence on the fluctuations of the kinetic theory, and a fourth explored the relationship between mass and energy implied by his hypothesis.[10]

In his paper "On a heuristic viewpoint concerning the production and transformation of light", Einstein found that, in the range of validity of Wien's law (i.e. for high frequencies), "monochromatic radiation of low density behaves ..... in a thermodynamic sense, as if it consisted of mutually independent energy quanta."[11] This hypothesis, suggested by the form of Wien's law, was supported by experimental evidence from the photoelectric and other effects.[12]

Einstein abandoned completely the concept of a substantial aether, and of the three introductions of quantum concepts his was the most at odds with the classical perspective, but it was still basically within the classical philosophy and methodology. Even his introduction of the principles of relativity and the absolute speed of light, although recognising the observer-dependent nature of physical experience, were backed by a firm belief in an observer-independent world, and the whole of his revision of physics entailed an attempt to make the classical theory consistent, rather than a rejection of this theory. Planck had challenged the established conception of matter; Thomson had challenged that of the aether, and Einstein challenged that of light, but they all adhered firmly to the fundamental principles of the nineteenth century.

I.2. THE RECEPTION OF QUANTUM CONCEPTS.

We may conclude from the analysis of the last section that the revolution we are seeking did not arise directly from the introduction of quantum concepts. But for physics to reach beyond these concepts it had first to incorporate them, not merely as possible hypotheses but as necessary — though insufficient — aspects of the description of nature. I shall therefore analyse next how these concepts were received and how, and in what form, they came to be accepted; in this section I shall deal mainly with the first step towards acceptance, the recognition of the concepts as being worthy of serious consideration.

First reactions: negative

As I stressed in the last section, the prime concern of Planck's 1900 theory was the problem of thermodynamic irreversibility; within this general problem lay the particular one of the mathematical treatment of black-body radiation. Thus Planck's theory was not concerned at all with the structure of light or energy, and nor, naturally enough, were the first responses to it. The theory was discussed in 1901 by Wien[13] and in 1902 by Burbury,[14] but neither of these authors even mentioned the quantum hypothesis and Burbury went so far as to write that Planck's theory had been modified "in detail only" since 1897. Even Ehrenfest writing in 1905,[15] after Larmor and Lorentz had drawn attention to the quantum hypothesis, asserted that this was "obviously meant to be taken only formally in its present form."[16]

Clearly, if the nature and importance of Planck's hypothesis were to be recognised this hypothesis would have to be examined in some context other than that of irreversibility, and this is in fact what happened.
The first recognition of the nature of the hypothesis seems to have come from Larmor, who in 1902 was commissioned to do a review article[17] on the subject of radiation. In an address to the British Association that year,[18] he discussed Planck's law and its derivation, noting an analogy with Newton's corpuscular theory of optics. He did not however publish his analysis. The following year, Planck's hypothesis was discussed in a published work by Lorentz[19] who, hot in pursuit of an electromagnetic theory of thermodynamics, arrived in

1903 at Rayleigh's law. As I have discussed in appendix A, this law (eqn.[7a]) was in fact appropriate to the classical wave theory of radiation, Wien's law (eqn.[2]) being appropriate to a purely particulate theory and Planck's (eqn.[11]) to a compromise between the two. Comparing Rayleigh's law with Planck's, Lorentz noted that finite energy units appeared to be an essential part of the latter, but his perspective unfortunately led him to concentrate on the similarity between the two laws (their agreement in the low frequency limit) rather than on their differences, and prevented him from penetrating the confusion of structural hypotheses in Planck's work.[20] Thus it was only when Jeans looked at the problem in 1905[21] that the importance of quantum hypothesis in Planck's theory was at last fully recognised.

Jeans' work prior to 1905 had been almost entirely on the kinetic theory.[22] It had culminated in his book on the dynamical theory of gases,[23] and in this he had considered one of the major problems facing the kinetic theory, namely that of specific heats. The kinetic theory predicted that, as a result of the equipartition law, the specific heat of a substance should be proportional to the number of degrees of freedom of its molecules; for high temperatures this seemed to hold, but at lower temperatures the specific heat fell below the predicted levels. Jeans had suggested that this might be due to dissipation in the aether, but Rayleigh[24] pointed out that for a gas in a closed cavity (the model, we may recall, for the black-body radiation law) this would lead to infinite specific heats, as one would have to take into account the infinitely many degrees of freedon of the aether. The situation, he suggested, was just that of Planck's radiation law, in which the infinite radiation predicted by the classical theory in the ultraviolet range did not manifest itself. Prompted by Rayleigh's objection, Jeans published a series of papers on this problem,[25] and thus approached Planck's theory in the context not of irreversibility but of the kinetic theory of a joint matter-aether system; he approached it moreover in the knowledge of Rayleigh's work, of Larmor's analysis, and of Thomson's quantum concept (both Thomson and Jeans being at Trinity College, Cambridge).

Jeans was young, his work to date had been in some ways novel and unconventional,[26] and the continuous theory of light had been in no way fundamental to this work; he was in fact in a far better position than most physicists to accept the new concepts. But, and this gives an indication of the total pervasiveness of the classical electromagnetic theory, he does not seem to have even considered accepting the quantum hypothesis. As a mathematical physicist, distrustful of structural speculation, he ignored Thomson's suggestion, and so far as the black-body radiation law was concerned his argument was simple: if the classical theory, applied to a final state, gave Rayleigh's law, and if Rayleigh's law were empirically wrong, then given the success of the classical theory the obvious conclusion was that a final state did not exist. There was nothing wrong with the classical theory but only with the conditions to which it was being applied, conditions, he suggested, that might well take millions of years to arise.

By 1905, therefore, the only physicists to have seriously considered Planck's hypothesis, Planck himself and Jeans, had both rejected it. Moreover, Thomson's idiosyncratic suggestion had not apparently made any impact and he himself had returned to his speculations on the structure of the atom. That left Einstein, but since he had explicitly rejected (as Planck and Thomson had not) the classical electromagnetic theory, his views were even less liked than those of his colleagues.

Einstein had of course introduced strong supporting evidence for his hypothesis and this meant that it could not be ignored, but the evidence could be, and was, reinterpreted. Of the three phenomena on which he had drawn, that of Stokes' law was relatively obscure and that of X-ray ionisation was inextricably bound up with a continuing debate on the nature of X-rays (not yet generally identified with light); the burden of evidence in respect of the light-quantum hypothesis therefore fell upon the photoelectric effect. Lenard[27] had discovered that when light is played upon a material surface, stimulating the emission of electrons, no emission takes place for light below a critical frequency, and that when electrons are emitted their energy is proportional not to the light frequency itself but to its excess over the critical frequency. Einstein took this as confirmation of the discrete nature of light energy, and the light-quantum hypothesis was indeed the simplest explanation of the phenomenon, but Lenard had suggested a classical explanation according to which the atom contained a sort of trigger mechanism;[28] this was supposed to be releasable by light of sufficient frequency but not by light of insufficient frequency, whatever its intensity, and the idea was taken up and developed by Wien in 1907.[29]

Since there was as yet no experimental evidence to distinguish between these two interpretations of the effect, and since interference phenomena and the general success of the classical electromagnetic theory weighed heavily in favour of a classical explanation, the vast majority of physicists had no reason, in 1907, even to consider Einstein's light-quantum hypothesis. It must indeed have seemed that the quantum concepts had all been thoroughly disposed of.

Rebirth

Fortunately, however, Wien's contribution was not the only one of 1907, for the same year both Einstein and Thomson reentered the arena.

Although Wien's interpretation of the photoelectric effect in terms of atomic behaviour was sufficient for most physicists, it was not so for J.J.Thomson. He himself had followed up Lenard's suggestion of a trigger mechanism, but from his viewpoint this had not been enough: he wanted to know not only the behaviour of an atom but also the structure behind this behaviour and when, in 1907, he had still failed to find such a structure, he decided to revive his conception of light-quanta.[30] In itself, this had no great impact, for the conception continued to be generally dismissed, but the indirect effect was considerable for he put two of his colleagues at the Cavendish onto an experimental investigation of the structure of light, and the results of this programme were important.

Campbell's experiments,[31] aimed explicitly at a decision between the quantum and classical theories of light, were inconclusive, but Campbell was one of the most open-minded and philosophically inclined physicists in England and his interest in the quantum problem was of great value in the years to come. G.E. Taylor's experiments,[32] though largely ignored by historians, were of the utmost importance, for they showed interference effects of light in circumstances in which, according to the quantum theory, only one quantum at a time could be passing through the apparatus. This did not decide conclusively against the light-quantum concept, but it effectively established that light, even on the quantum scale, had an essential wave aspect to its nature, and this was later to prove crucial in the development of the wave-particle duality concept.

In 1907, this concept had not yet been developed, nor the necessity of the particle aspect of light proved, but a step towards these developments was made then by Einstein. While his own theory was being rejected by Wien, Einstein turned his attention to that of Planck.[33] As things stood, Planck's quantum hypothesis could be and generally was ignored. The confusion inherent in the 1900 theory was still the dominating characteristic of that theory, and the only one to have braved it, Jeans, had dismissed the hypothesis. With Einstein's contribution, however, the situation changed dramatically, for having given a simplified derivation[34] of Planck's law, in which the quantum hypothesis (applied to the energy of a resonator) was clearly stated, he applied this law to the problem of specific heats. In marked contrast to the classical theory, the law led, qualitatively, to just the observed behaviour, and this not only confirmed Rayleigh's suggestion that the black-body and specific heat problems were fundamentally the same, but also brought Planck's law and the hypotheses behind it out of the confused context of irreversibility and into that of kinetic theory. The two had been linked already by Jeans' work, but Jeans had proceeded in the opposite direction and his results had been in the confused context; Einstein's results, on the other hand, were clear, acceptable and quantitatively verifiable. Einstein's work thus ensured that Planck's hypothesis could no longer be dismissed as an unclear part of the irreversibility problem, but had to be treated also as an explicit aspect of the theory of specific heats, and a fit subject for the concern of physicists in general. In particular, the most important consequence of the work was that it attracted the attention of Nernst, who followed it through quantitatively and, finding that the results were in perfect accord with experiment,[35] decided to use his influence to convene the first Solvay Congress,[36] in 1911, for the express purpose of considering the quantum conceptions.

In the meantime, Lorentz was prompted in 1908 to reconsider both Planck's theory and Jeans' interpretation of it,[37] and although he found both to be unsatisfactory he published in 1910 an analysis of the quantum problem as he saw it[38] and a new derivation of Planck's law.[39] The analysis did not go very far, and the derivation was no more than a simplified 'shorthand' version of Planck's, but both helped to familiarise physicists with the quantum concepts — an important preliminary to any further analysis.

In Cambridge, the combined effect of Einstein's paper and Thomson's renewed considerations prompted Larmor to publish a derivation of Planck's law in 1909.[40] This was, if anything, even more confused than Planck's had been, and by trying to eliminate (through a limiting process) the quantum hypothesis[41] it acted clearly against the acceptance of that hypothesis; but since it was given in a Bakerian lecture it was widely disseminated, and might well have introduced many British physicists to the quantum problem.

Finally, just before he returned to Cambridge from America in 1909, Jeans also decided to re-examine the black-body radiation problem,[42] and although he stuck to his 1905 conclusion, that a final state was not realised, his analysis[43] of the problem nevertheless took it a significant step forward. For Jeans analysed the necessity of the quantum concept in the derivation of Planck's law and concluded (albeit unrigorously) that if one accepted the final state and the (universally assumed) independence of vibrations of different frequencies, then one had necessarily to postulate the quantisation of energy, both for resonators and for the aether.

Duality

Had he accepted Jeans' non-rigorous analysis but rejected his suggestion of a non-existent final state, a Cambridge physicist of 1910 would have been faced on the one hand with a "proof" of the necessity of the quantum hypothesis, including discrete and hence localised absorption, and on the other hand with Taylor's experimental demonstration of the continuous and dissipated absorption of light: light, he would have had to deduce, was somehow both continuous and discrete, both dissipated and localised, both wavelike and particle-like. This conclusion (already, as I have noted in appendix A, implicit in Planck's radiation law) was not in fact accepted, even by a minority group of physicists, for some years, but it had already been propounded for the first time the previous year, by Einstein.

In 1909, as a test of the necessity of the quantum hypothesis, Einstein conducted an investigation into energy fluctuations.[44] Considering the implications of Planck's law, he derived from it a formula for the mean square energy fluctuation that contained two terms,

$$\langle \varepsilon^2 \rangle = \left\{ \frac{c^2 E^2}{8\pi\nu^2 V d\nu} + h\nu E \right\}$$

Classical wave theory gave rise only to the former term, while the application of the law of large numbers to a system of particles led only to the latter.

Following this result, Einstein proposed at a meeting in Salzburg in 1909 a fusion of wave and particle viewpoints,[45] or as he put it "of the wave and emission theories". Planck argued strongly at the meeting that light-quanta could not exist because of the problem of explaining interference phenomena,[46] and Stark, who seems to have been the sole defender of the light-quantum concept, conceded the point but hoped that the concept would soon be able to overcome the difficulty.[47] Einstein, however, did not see any difficulty, "for the following reason: one dare not assume that radiation consists of quanta which do not interact with each other".[48] He admitted that "that would be impossible for the explanation of interference phenomena",[49] and that light-quanta must have some wave-like property:

> "I myself think of a quantum as a singularity surrounded by a large vector field. Through a large number of quanta a vector field could be constructed which would differ little from such as we assume for radiation." [50]

This was the first public mention of an idea Einstein was to struggle with for many years, but he had already suggested something similar privately, several months before the Salzburg meeting, to Lorentz.[51] He had argued then that quanta might be singularities in a field, but that they were quite definitely not independent particles. This conclusion was linked with his attempts to develop a general theory of relativity. Having decided in 1905 to base his unification of physics upon the gravitational rather than upon the electromagnetic model, he had soon realised that instantaneous action at a distance was not a satisfying concept in his new theory, for there was no absolute frame in which to define the instantaneous action, and the Lorentz transformations led to the conclusion that an instantaneous propagation in one frame would appear as a reversal of cause and effect in some others. He therefore decided that a field would have to be introduced for the propagation of gravitational forces with finite speed, and this field was linked conceptually with that supposed to surround (and indeed support) the light-quanta.

Division and controversy

Einstein's fluctuation paper and that he delivered at Salzburg can be seen as the first explicit indication of the dual wave-particle nature of light that appears to characterise the quantum theory, and as the first intimation that physics would have to go beyond the quantum conceptions for the solution of its problems. They also sparked off the first real debate on these quantum conceptions. For the most part, this debate was rather one-sided, in opposition to the conceptions. As Planck put it,

"That seems to me to be a step that in my opinion is not yet called for."[52],

and as Lorentz summed it up the following year, in a survey of the current problems in physics,

"The speaker would not like to quarrel with the heuristic value of this hypothesis, but would like to defend the old theory as long as possible."[53]

Even Einstein himself was too well aware of the subtlety of the problem, and of the need for a dual perspective, to defend the light-quantum concept with any vigour. But it did find one ardent defender in J. Stark, and his efforts acted to stimulate discussion and also to take the quantum problem back to the context of the photoelectric effect and, more importantly, to that of X-ray behaviour.[54]

Stark adopted the light-quantum hypothesis wholeheartedly, partly, it has been suggested,[55] because he wanted to go against the general opinion, but partly also because of his experimental interest in the photoelectric effect and X-ray phenomena. He identified X-rays with light, and although his opponents did not agree with this identification he decided to conduct his argument largely from X-ray evidence, which was generally agreed to reveal an element of discrete behaviour. The result was a three-way controversy on the nature of X-rays. The majority of physicists both in Britain and on the continent accepted the Stokes-Wiechart aether-pulse theory, but there had been considerable opposition to this theory in Britain, where Bragg had advocated a material corpuscular theory of the rays. The aether-pulse theory was defended by Barkla, who had adopted it in 1904[56] and found apparent confirmation of it the following year in the results of experiments showing polarisation of X-rays.[57] Bragg's theory had first been propounded in 1907 as one of γ-rays,[58] which he linked with the "dynamids" (bound states of oppositely charged distant particle pairs) that Lenard had advocated as constituents of the atom, and which he interpreted as bound pairs, disintegrating in the atom, of electrons and positive electrons. Also in 1907, in the context of his quantum conception, Thomson had drawn attention to the extensive similarities between X-rays and γ-rays,[59] and a controversy ensued.[60] Both Barkla and Bragg agreed that the two types of ray were essentially alike, but Barkla insisted on the wave properties of X-rays and Bragg on the particle properties of γ-rays. Although the corpuscular analogy was strengthened by experimental work in 1908,[61] this work also linked the rays with ultraviolet light, which was agreed by both parties to be wave-like, so the controversy continued. Neither Barkla nor Bragg accepted Thomson's light quantum concept in full, but Barkla[62] leant on the authority of Thomson as a supporter of the aether-pulse theory, and Bragg claimed that

"The idea that X- and γ-radiation are both to be regarded as consisting of streams of discrete entities has gained ground steadily in the last year or two.",

again citing Thomson's work.[63]

Barkla and Bragg were agreed, however, on one thing, namely that light (as opposed to X-rays) was a wave form in the aether, just as in the classical theory. Stark, by abandoning the continuous aether in favour of the light-quantum concept, went firmly against this classical view. This landed him in hot debate with Sommerfeld, who preferred, as he said, "to begin with the familiar Wiechart-Stokes view"[64] and who insisted that the classical aether-pulse possessed "absolutely the character of a projectile and no longer differs appreciably in its energy localisation from a corpuscular radiation or from the hypothetical light-quanta."[65] In 1911, the British and continental battles were joined when Bragg, who shared Sommerfeld's views on the heresy of light-quanta, wrote to him advocating the corpuscular theory of X-rays.[66] Sommerfeld, predictably, was not sympathetic.[67]

New ideas

The entry of Sommerfeld to the quantum debate was an important one, for although he advocated the classical viewpoint he was sufficiently open-minded, and sufficiently respectful of Einstein (he was in fact one of the first converts to relativity theory), not to dismiss the quantum conceptions out of hand. He was, moreover, an excellent mathematical physicist and possibly the most brilliant physics teacher of the century, so he was able to contribute to the quantum problem both his own talents and those of a long chain of students; this last point was to be of particular importance in the long term. More immediately, Sommerfeld made two other contributions to the quantum problem. First, in response to Einstein's Salzburg paper of 1909, he suggested that Planck's constant 'h', although necessary on empirical grounds, should not be interpreted as applicable to energy ($h\nu$) but to action (h);[68] on this view neither the energy of radiation nor that attached to a resonator should be quantised, but only the action over a complete (non-instantaneous) emission or absorption process, the classical nature of light being preserved. Secondly, he interested his assistant Debye in the problem, and this resulted in a new derivation of Planck's law, by Debye, in 1910.[69] This did not represent any fundamental conceptual advance over Planck's derivation, but like those of Larmor and Lorentz it familiarised the notation and took the problem to a wider audience. It also had one distinct advantage over Planck's derivation and the others to date in that it dealt directly with the radiation, and not with the resonators supposed to create it. This still left the derivation with a mass of non-explicit structurally contradictory hypotheses, but it did at least remove the most glaring of the contradictions, the use of Planck's classically derived formula linking radiation and resonators.

Summary

In the course of all the above developments, the quantum concepts grew from being silently rejected in 1907 to being noisily rejected in 1910, and this was a great advance: the concepts were, at last, worthy of serious consideration. When it came, this consideration centred around the Solvay Congress of 1911, and this will be the subject of my next section; but first let us summarise the state of affairs as of 1910.

Thomson's conception of the light-quantum had still failed to make any significant impact, and Thomson himself had returned to his speculations on the structure of the atom. Also speculating in this direction was Haas,[70] who suggested in 1910 an atomic structure based on Thomson's but explicitly incorporating Planck's constant; this suggestion did make quite an impact,[71] especially upon Lorentz who analysed it at some length,[72] but the model was completely ad hoc, and it turned out to be without any predictive value. The main fruit of Thomson's quantum conception was the experimental result of Taylor, and this, together with the general success of the classical electromagnetic theory, weighed heavily against any light-quantum hypothesis; moreover, although the photoelectric effect was most simply explained in terms of light-quanta, there was no convincing reason as yet to dismiss the classical interpretation of this effect. The balance of experimental evidence was thus heavily against the light-quantum concept and this was reflected in opinion, for only Stark and Einstein actively propounded this conceptualisation, and Einstein qualified it considerably.

In fact, Einstein's conception of quanta was, in 1910, much closer in some ways to that of opponents of the conception than to that of the original, 1905 conception. In 1905 he had conceived of light as localised quanta, independent of each other and also independent existents in an ontological sense; in 1910 he conceived of it as fundamentally a wave form (an aspect that was shared by Thomson's conception) , manifesting itself in localised particles, but dependent both on each other and, ontologically, on the medium. This medium was no longer the classical aether, and it was hardly substantial at all, being no more than a mathematical field; but Einstein had not, as we shall see later, changed his methodology. He still recognised the need for some structural basis for the description of a behaviour and he seems to have seen the space-time field as in some sense substantial.

All this was too complex for most people, however , and the conceptions of Einstein, Thomson and Stark (who seems to have stuck to Einstein's 1905 view) were identified in the minds of their opponents. These were led by Wien, who still sought an electromagnetic description of nature, Planck, whose basic philosophy was equally opposed to any suggestion of discrete radiation, Lorentz, whose preference , although not nearly so dogmatic as those of his colleagues, was naturally enough for the classical theory that he had helped create, and Sommerfeld.

These physicists wielded considerable authority and they all preferred, as Planck explained in 1909, to look for an explanation of quantum phenomena in the properties of matter, rather than in the structure of the aether.

In general any adherent of the electromagnetic world-view, or of any other fundamentally behavioural methodology, could do no other at this stage than reject the light-quantum hypothesis. For the most part, physicists in the tradition of suggestive structural speculation were similarly placed, for Taylor's results seemed to dictate against the hypothesis while the complexities of matter and (classical) aether offered great scope for structural speculation of the quantum phenomena. Only those who stood between these two positions (such as Einstein, Nernst, Jeans, Lorentz or Stark) could seriously consider the light-quantum hypothesis, and they too had to face up to the enormous problem posed for this hypothesis by the evidence of interference phenomena. In respect of X-rays, where the three views that I have outlined stood irreconcileably opposed, the evidence for a particulate nature was greater, but it was still not conclusive and the preference was almost universally for a classically based theory.

Finally, in respect of the black-body radiation law, the situation was still utterly confused. Einstein's application of Planck's law to the problem of specific heats had served the purpose of revitalising the quantum conceptions, but in principle it did not take the problem beyond that of black-body radiation itself. Thanks to Einstein's derivation of Planck's law, the hypothesis of energy quantisation for the resonators had been made quite explicit, and Jeans had even claimed it as necessary in the context for both resonators and aether; but there was still total confusion as to the particle and wave hypotheses in the derivation of the law, and the relationship between the hypothesis of quantised energy content of the resonators and that of light-quanta had not been sorted out. Einstein had shown that both wave and particle aspects seemed to follow from the law, but this had only shown up the confusion; it had not cleared it.

I.3. THE FIRST RECOGNITION OF QUANTUM CONCEPTS: NECESSARY BUT INSUFFICIENT

In 1911, thanks to the efforts of Nernst, the first Solvay Congress was held in Bruxelles.[73] Of the physicists present most were opposed to the concept of light-quanta; but, with the possible exception of Wien, they all recognised that there was a quantum problem worthy of their serious consideration. Through the discussions at the Congress and their repurcussions over the next few years the precise nature and full extent of this problem were established. To the Congress itself, Einstein, Sommerfeld and Planck all contributed important theoretical analyses, while there were significant contributions to the discussions from several other physicists, including Jeans, Poincaré and Langevin.

The case for the light-quantum concept.

The case in support of the light-quantum concept was put by Einstein,[74] who felt that to attribute the quantum effects as his opponents did to the atom was only to postpone the problem. Any atomic mechanism, he felt, would be bound to involve the assumption of localised quanta at some stage. Having introduced the light-quantum concept in the context of his fluctuation formula, Einstein admitted that this concept seemed "irreconcileable with the phenomena of diffraction and interference".[75] But if it were to be avoided, he could see only one alternative, namely that

> "One must resort to abandoning the law of conservation of energy in its present form, giving it for example only a statistical kind of validity, as one does already for the second principle of thermodynamics." [76]

In the particular case of energy fluctuations, he concluded that [77]

> "In this case I do not see that one could introduce a hypothesis of accumulation and one can only choose between the structure of radiation and the negation of an absolute validity for the law of conservation of energy.",

and in general he found it necessary "to introduce a hypothesis such as that of quanta alongside the indispensable Maxwell equations." [78]

Since Einstein was quite right when he declared in the ensuing discussion that "in the first place we would agree on conserving the energy principle",[79] this was a very strong argument. It did not convince Planck, Sommerfeld, or Wien, but Einstein did gain some support for his conceptions at the congress. Lorentz, who took the chair had already stressed the failure of the classical theory in his opening paper, and although he still had reservations about the light-quantum hypothesis he defended Einstein's contentions in discussion. Jeans also seems to have been gradually coming over to Einstein's viewpoint. His main contribution to the Congress had been an analysis showing that

> "No extension of the Maxwell-Boltzmann theory in the sense that will be indicated [i.e. in which the canonical equations are conserved] can account for the phenomena of radiation",[80]

and although he concluded with his familiar suggestion that a final state might not be reached in reality, he only mentioned this as a possibility, whereas he had previously advocated it as the probable solution. Since there was clearly no support for the suggestion from the other participants at the Congress,[81] he did not return to it in discussion, and although he was not yet fully convinced of the need for a quantum theory at all he lent his support to the light-quantum hypothesis, presumably in accordance with his conclusion of 1910 that any quantisation of energy must apply to the aether, as well as to resonators.[82] On these grounds, Jeans objected in particular to Planck's theory, arguing that it was after all radiation, and not matter, that was supposed to be the subject of Planck's law.[83]

The most significant conversion to the light-quantum concept was that of Poincaré. Poincaré was new to the quantum theory, but throughout the Congress he contributed the most pertinent questions to the discussions and in the concluding debate he stressed his conviction that something had to be done , remarking that all the theories presented appeared to combine classical and quantum elements in a way that did not appeal to his sense of consistency:

> "What has struck me during the discussions we have been hearing is to see the same theory resting now on the principles of classical mechanics and now on the new hypotheses which are their negation; one must not forget that there is no proposition one could not easily demonstrate if only one introduced into the demonstration two contradictory premises." [84]

This observation prompted him to go further even than Einstein and to suggest that something basic had to change, not only in the fundamental mechanical principles but also in the laws expressing them: with characteristic boldness he asked whether one could "still express these laws in the form of differential equations?" [85]

Poincaré repeated this query in a semi-popular article on the quantum hypothesis in 1912,[86] and added to it then the suggestion that one might introduce some sort of atoms of time.[87] He felt that the quantum hypothesis had called into question the fundamental concept of continuity, and he was not afraid to draw the consequences.[88] His suggestions had no issue, since he died soon after making them and they were too revolutionary, and too difficult to implement, to be taken up by anybody else. Moreover they were the suggestions of a mathematician rather than a physicist, with no immediate consequences in the way of experimental predictions or even structural conceptualisations. But despite all this there can be no denying the immense authority Poincaré held at this time, as a philosopher, as a mathematician, and indeed as a physicist, and the strong emphasis he placed upon the seriousness of the quantum problem must have had a considerable effect. If Poincaré took the problem seriously, it could hardly be ignored, and his article must have brought it, moreover, to the attention of a wider audience than ever before.

The defense of the classical concept of light

Of those who opposed Einstein's views on the nature of light, the most aware of the existence of a quantum problem were probably Sommerfeld and Planck, both of whom profounded theories aimed at solving this problem without recourse to the light-quantum concept.

Having failed to eliminate the implications of his 1900 theory, Planck had kept away from the quantum problem, concentrating on what he saw as the far more important question of relativity. The convening of the Congress brought him back to the subject for the first time,[89] and his contribution was a new derivation of his radiation law[90] (for the details of which see Appendix C), the essential features of which were the assumption of continuous absorption, a description of emission (supposed continuous) in terms of action rather than energy, and in terms of atomic behaviour rather than radiative structure, and a hypothesis of elementary regions of equal probability. These features were all of considerable importance, and they all seem to have stemmed from a single basic motive, the avoidance of the light-quantum hypothesis. As we have noted, Planck had always preferred a behavioural approach to physics, and had always opposed the structural hypothesis of atomism, even for matter;[91] this hypothesis applied to light seemed to him a heresy, and he dismissed it out of hand:

> "It goes without saying that such hypotheses are irreconcileable with Maxwell's equations. ... When one thinks of the complete experimental confirmation which Maxwell's electrodynamics has received from the most delicate phenomena of interference .... one feels some repugnance at ruining its basis from the outset." [92]

Planck did not distinguish between the different conceptions of light-quanta, and it is not even clear that he recognised the dual nature of Einstein's conception; although he referred to the paper in which this was introduced, and although he had taken part in the discussions following this paper's presentation at Salzburg, he nowhere discussed the possible existence, suggested in it, of singularity solutions to something approximating to Maxwell's equations. The light-quantum concept struck him as so heretical that he could not conceive of such a compromise.

Having rejected the concept of light-quanta, Planck could not see how light, although it might be emitted as some sort of pulse, could possibly be absorbed in discrete units, since the pulse would spread out spherically. He insisted that

> "In all cases where an oscillator is supposed to vary in a discontinuous manner, it is impossible to understand where the energy absorbed by an oscillator comes from when ... its energy increases sharply from 0 to $h\nu$.... <u>The phenomenon of absorption of free radiation is essentially continuous</u>."[93]

This assumption seems to have led directly to the hypothesis of regions of equal probability. Planck must have been familiar with Einstein's 1907 derivation of his radiation law, and he must also have known, as we shall see, of an analysis of the law by Ehrenfest in 1911; the assumptions behind his law, as phrased by these authors, were incompatible with continuous absorption, and he therefore rephrased these assumptions so as to allow a continuum of energy values. To Einstein, to Ehrenfest, and later to Poincaré, a resonator could have discrete energy values only, equivalent in the case of a linear harmonic oscillator to the p and q values lying on discrete ellipses in the phase space. To Planck, the values did not lie <u>on</u> discrete ellipses, but <u>between</u> pairs of such, as is indicated in figure 1.

Figure 1.

These regions were associated, as the Einstein ellipses had been, with equal a priori probabilities, and they were related to a "hypothesis of elementary quantities of action"[94] by the formula $\iint dp\,dq = h$, the integration being taken over a single region.

This was clearly an important change, for there was nothing, given Planck's interpretation of the quantum behaviour, to prevent the energy of the resonators from changing continuously throughout the regions: all that was required was that the regions somehow attained equal probabilities, perhaps through some sort of atomic mechanism. Unfortunately, as we may deduce from Ehrenfest's and Poincaré's analyses,[95] Planck's hypothesis does not in fact work, for there is a difference between giving unit probability to a region as a whole and giving it to the average value of the region. This was pointed out by Poincaré, who added a note to the Congress report that

> "In reality, ... , the probability is zero throughout, except at certain isolated points: it is the same at these various isolated points, and there is only one point in each region.",[96]

but he had only worked this out after the Congress itself, and there was no criticism of Planck's theory on these grounds during the reported discussions: a clear perspective of the situation was still universally lacking.

Planck's new hypothesis brought him into line with Sommerfeld, who had earlier suggested an action interpretation of the quantum hypothesis. It also tied in well with his own general ideas, action being a purely behavioural concept with no structural significance at all, and he laid great stress on the fact that

> "Above all, one must insist on the fact that the hypothesis of quanta is not properly speaking a hypothesis of <u>energy</u>, but could rather be called a hypothesis of <u>action</u>."[97]

According to this viewpoint, Planck naturally linked the discontinuous emission of radiation with the behaviour of the resonator, and he did this by postulating that, on the occasion of the absorbed energy reaching a multiple of $h\nu$, there should be a predetermined probability that the resonator should emit all of the accumulated energy. No mechanism was suggested for this total collapse, but it seemed to be a simpler type of behaviour to account for than a partial emission would have been.

Sommerfeld[98] also stressed that it was the action and not the energy that must be quantised, and that this quantisation should be related to the atom rather than to the radiation. He suggested that

> "The general properties of all the molecules or atoms which determine the phenomena of radiation do not consist of the intervention of particulate elements of energy, but in this, that the manner in which the changes of energy are ... produced is given by a universal law.",[99]

this law being that

> "in all purely molecular phenomena, the atom gains or loses a universally determined quantity of action of the amount $\int_0^\tau H\,dt = h/2\pi$.",[100]

H being the classical Hamiltonian, and the integration being over the duration of the process. Basing his treatment upon this law, Sommerfeld considered those phenomena that suggested a light-quantum hypothesis, including γ- and X-ray effects as well as the photoelectric effect.

Sommerfeld postulated quantisation of action for absorption as well as for emission (though when he developed the theory with Debye[101] in 1913 he returned to continuous absorption) and for corpuscular as well as for radiative emission, thus covering the whole range of radioactive phenomena. He emphasised the fact that his theory was based upon a fundamental law, rather than upon particular hypotheses such as Planck had introduced[102] for resonator behaviour in his new theory, or such as Haas had introduced in his atomic theory which Sommerfeld attacked both in detail and in principle.[103] "As for me", he asserted, "I prefer a general hypothesis on h to particular models of atoms"[104] and, he might have added, of radiation, for he dismissed the light-quantum hypothesis on the same grounds.[105] He admitted, as Lorentz had done, that the light-quantum hypothesis had a strong heuristic value as an explanatory mode,[106] and also that it was in general simpler to apply than his own theory, but he objected that it lacked the absolute nature of the action principle, that it was too much in contradiction with the classical theory, and that h, the action, was a relativistic invariant whereas $h\nu$, the energy, was not. Sommerfeld's attempts to apply the general action principle to particular cases came under severe criticism, especially from Einstein, and he had to admit that his theory was far from practical, but it was nevertheless the most attractive response to the quantum problem to date. He claimed, moreover, though incorrectly as it turned out, that experimental results favoured his theory over Einstein's for the photoelectric effect.[107]

Necessity of quanta

The balance of opinion at the Solvay Congress was heavily against the light-quantum concept and in favour of a theory that allowed, at the very least, for the continuous absorption of light, the quantum emission effects being a feature of atomic behaviour rather than of the structure of light. Considering the evidence available, which had forced even Einstein to accept an element of the continuum theory, this was still very reasonable, but that evidence was already beginning to turn in favour of the light-quantum concept. We may recall that already in 1910 Jeans had demonstrated, albeit unrigorously, the necessity of energy quantisation in respect both of resonators and of the aether, and this conclusion was soon verified by two other investigations. In 1911, just before the Solvay Congress, Ehrenfest published a rigorous and highly illuminating proof of the necessity of the quantum hypothesis,[108] and immediately following the Congress Poincaré published another proof, less illuminating but, backed by his enormous authority, much more influential.[109] Between them, these investigations ensured that the light-quantum concept could no longer be altogether dismissed; they thus represented a crucial step in the development of quantum conceptions, and one that must be examined more closely.

Jeans asked the question,

> "Can any system of physical laws expressible in terms of continuous motion (or of mathematical laws expressible in terms of differential equations) be constructed such that a system of matter and aether tends to a final state in which Planck's law is obeyed?" [110]

He felt that "the theory put forward by Planck would probably become acceptable to many" if the answer were in the positive, but he concluded that "the answer obtained is in the negative."[111] He examined the problem in terms of waves in the aether, rather than in terms of resonators, and found that, on the assumption that "the energies of waves of different frequencies must be represented by different sets of coordinates",[112] as experiment seemed to show, a continuous treatment led necessarily to the equipartition law. He claimed, moreover, that Planck's law led back necessarily to his probability formula, and that this in turn necessitated his quantum hypothesis, since no other distribution of vibrations could give the same probability distribution. The argument was not rigorous, as Jeans assumed the quantum hypothesis in his recovery of the probability formula, but his work did show that some element of discreteness was necessary, and he had presumably convinced himself that this had to take the form of Planck's quantum hypothesis.

A further conclusion of Jeans' is also interesting:

> "The analysis has, however, shown that the truth of Planck's law requires something more than appeared in Planck's original papers. It is now apparent that it is not enough to postulate systems of vibrators capable only of holding definite multiples of a fixed unit of energy: we see that the energy in the aether itself must also be atomic."[113]

This reads at first like Einstein's original light-quantum hypothesis, but this clearly cannot be, for that does not lead to Planck's law at all. What Jeans was in fact saying was that, as he went on to explain, the structure of the aether had to be such as to allow only discrete amounts of vibration, a consequence of considering the vibrations directly rather than through the resonators. In principle, these discrete amounts of vibration had no reason to be localised, and were not therefore comparable with Einstein's original conception of light-quanta. According to Jeans' analysis, however, absorption had to be discrete, and this was the most significant aspect of his conclusion: for it is difficult, if not impossible, to conceive of discrete, and hence localised and effectively instantaneous, absorption within the context of the classical nature of light. Jeans' analysis therefore supported Einstein's conclusion of 1909 that light had somehow to be both localised (for discrete absorption) and dissipated (for the localised quanta to interfere, as in the Planck statistics).

Jeans' analysis had a limited impact, for in the first place it was not rigorous and in the second its implications were rejected by Jeans himself. We may deduce from the motive that he gave for his investigation that he would rather have reached the opposite results to those he did. This is confirmed by his final conclusion, which was essentially the same as it had been five years previously, namely that

> "If it is agreed that these conditions do not hold in nature, then we are driven to supposing that the state of the aether represented by Planck's law is not a final steady state."[114]

Having subjected the quantum to its only analysis in ten years, and having found the evidence to be in its favour, Jeans still could not accept it.

Ehrenfest's analysis was published in Annalen der Physik in the month of the Solvay Congress. Since the editors of this journal were Wien and Planck, the analysis was presumably discussed at the Congress, but as both editors were opposed to the light-quantum concept this discussion may have been minimal. There is in fact no record of it.

In omitting to mention Ehrenfest's work, Wien and Planck have been joined by historians, and I have therefore outlined this work in Appendix D. Following Debye and Jeans, Ehrenfest considered the black-body problem directly in terms of radiation, or vibrations in the aether, rather than in terms of resonators.

He analysed the effect, on the radiant energy distribution, of adiabatic changes in the system and, restricting his analysis essentially to the empirical behaviour in the high and low frequency limits, deduced that the weight function of the distribution was necessarily of the form postulated by Einstein. The radiation, he concluded, could take on only a discrete range of energy values. Ehrenfest's demonstration showed explicitly that the quantum behaviour had necessarily to be of the same type as had been assumed in the derivations of Planck's law; moreover, unlike Jeans' demonstration of a similar result the previous year, it was both precise and rigorous.

Ehrenfest also clarified the relationship between the <u>physical</u> hypotheses necessitated by Planck's law and the independent light-quantum hypothesis of Einstein's 1905 paper. By 1909, Einstein himself had recognised a difference between the two possibilities, and had begun to insist upon the non-independence of the light-quanta, but Ehrenfest's was the first clear analysis of this difference. He defined two possible sets of assumptions, namely

(α) that a resonator could have energy values $0, h\nu, 2h\nu, \ldots$, or, as he showed, equivalently, that it could absorb and emit energy only in units of $h\nu$,

and (β) that the radiation, whether in matter-free space or attached to resonators, consisted of free, "mutually independent", particles of energy $h\nu$.

He observed that, whereas (α) led to Planck's law,

> "One can show: The assumption (β) does not lead to Planck's radiation formula, but to one of a simply infinite group of other radiation formulae, the selection of one particular formula from this group being dictated by an additional condition."[115]

This analysis highlights an essential feature of the emerging quantum problem, for although Ehrenfest expressed assumption (β) in terms of the <u>structure</u> of radiation, he had to express assumption (α) in <u>behavioural</u>[114a] terms. Einstein, who was already seeking a structural interpretation of this assumption, had talked of non-independent quanta, but this begged the question of how the quanta, by their very nature localised, could interact. The general consensus of opinion was in favour of retaining the wave concept of light propagation, this being the structural concept classically associated with light. Since this treated light as a disturbance in the field, rather than as an independent existent, it was also preferred by the behaviouralists, but it too begged a conceptual question, namely what structure, or behaviour, of matter could lead to the required assumption, (α). This assumption could not be given a satisfactory structural interpretation, and Ehrenfest was therefore forced to describe it in behavioural terms.

Poincaré's necessity proof[116] arose from a discussion at the Solvay Congress in which Nernst had suggested a possible continuous derivation of Planck's law. Poincaré had agreed that such possibilities should be exhausted before making any radical changes in physics but, having examined the problem on his return to Paris, he inserted as a footnote to the Congress report his conclusion that

> "The hypothesis of quanta appears to be the only one to lead to the experimental radiation law, if one accepts the formula usually adopted for the relationship between the energy of the resonators and that of the aether and if one supposes that energy exchanges could occur between the resonators by the mechanical impact of atoms or electrons."[117]

His approach to the problem in his paper of 1912 was to consider the energy distribution over the Planck resonators, and the alterations to the classical theory necessary to obtain the observed result. He concluded (see Appendix D) that no resonator could at any time adopt energy values other than odd multiples of $\varepsilon/2$ (in agreement, once the zero point energy of Planck's 1911 derivation of his law had been taken into account, with Ehrenfest and Jeans), and that any distribution leading to finite total radiation must involve some discontinuity.

Comparing the arguments of Ehrenfest and Poincaré, it is the former that seems the more extensive and the more original. Both were rigorous, given the classical assumption of the independence of vibrations of different frequencies,[118] but whereas Poincaré's conclusions were based on the precise form of Planck's law, Ehrenfest assumed only the general limiting behaviour. Poincaré's conclusion as to the implication of finite total energy was included in Ehrenfest's earlier treatment, and this had the virtue also of dealing directly with the radiation, whereas Poincaré still relied upon Planck's classical formula for the connection between radiation and resonators.

In terms of their reception, however, the importance of the papers was reversed. Ehrenfest's paper was overshadowed by the Solvay congress (at which he was not present), and reached a rather unsympathetic German speaking audience. Wien's dislike of the quantum concepts was too strong to be overcome, and, although Stark and Einstein presumably approved of Ehrenfest's analysis, they did not comment upon it . The quantum physicists who probably carried most weight in Germany were Planck and Sommerfeld, and they were too tied up in their own theories, expounded at the Congress, to bother about Ehrenfest: in fact both continued for some years to put forward continuous absorption theories, explicitly ruled out by both Ehrenfest and Poincaré. In France, where there was relatively little interest in the quantum theory, Poincaré's work seems to have been taken as definitive, while in England it was given a lot of emphasis by Jeans, who was finally convinced, in 1913, of the impossibility of his own circumvention of the quantum problem.

Wave-particle duality

The necessity proofs were crucially important, for they established that, for Planck's law to hold, both the emission and absorption of radiation had to be discrete processes, and although atomic mechanisms could be conceived of for discrete emission discrete absorption seemed impossible to account for within the classical framework. In Germany, Sommerfeld, Planck and others got round this problem by simply ignoring the necessity proofs, but some British physicists proved less blind. Although they had a strong regard for the classical aether they did not share the total aversion of their German colleagues to structural hypotheses, and there was moreover a strong British tradition of unorthodox speculation. Of the physicists on the continent who had taken an interest in the quantum problem, the most highly respected in Britain were Poincaré and Lorentz, so that once their interest in the problem had been aroused, by the Solvay Congress and by Poincaré's papers, some British physicists at least were in a position to view it with an open mind. The main problem was the success of the classical conceptions, especially in the context of interference phenomena, and many saw this as insuperable, but some were prepared to follow Einstein in recognising the need for a dual conception.

Thus it was in Britain that the 'wave-particle duality' of light first found general recognition.

One of the first moves in the direction of wave-particle duality was a suggestion made by Bateman[119] in 1913. We may recall Einstein's suggestions of point-singularities in a neo-Maxwellian field, and the fact that these were either dismissed or ignored, apparently without any research into the possibilities. Bateman noted that

> "It is implicilty assumed that electromagnetic fields with other types of singularities [i.e. other than the electron] are non-existent." ,[120]

and complained that

> "This hypothesis is of such a sweeping nature that it ought only to be adopted after a careful study of the different types of singularities which solutions of Maxwell's equations posess." [121]

He made such a study, and found[122]

> "one type of electromagnetic field which may perhaps be of some physical interest as its chief characteristic is a corpuscular type of radiation."

Had not its physical significance been rather hard to grasp, this might have been an important development in quantum history, but as it was it was still indicative of British attitudes of the period.[123]

Another British contribution of 1913 came from Campbell,[124] the author of Modern electrical theory, a very influential textbook the second edition of which included an early and exceptionally clear statement of the duality problem, as well as a fascinating suggestion as to a possible solution:

> "The present position appears to be that the wave theory, as supported by Sommerfeld, and the corpuscle theory, supported by Einstein, are each capable of explaining a great many facts connected with radiation; but that, in general, the facts which are explicable by one theory are not explicable by the other. It almost seems that the energy itself is transferred by the corpuscles, while the power of absorbing energy and making it perceptible to experience is transfered by spherical waves, but it is impossible to suggest anything explaining this distribution. The problems of radiation are not yet solved." [125]

Indeed not. If Campbell's grasp of the problem was remarkable for the period, his suggested remedy was even more so. Obviously, it went right against classical physics, but this did not worry Campbell, whose main notoriety in this period arose from his serious discussion of, and apparent sympathy with, Thomson's Faraday-tube theory, and who was always, as a philosopher, very critical of the dogmas of traditional science. He approached the duality problem without preconceptions, and drew the simplest conclusion possible in the light of the quantum effects, on one hand, and the interference effects, on the other. His suggestion had no immediate repercussions, but in the longer term it represented a crucial stage in the history of quantum concepts, for it linked the dualistic conception held by Newton, which formed the foundation for the eighteenth century theory of light, with that of de Broglie, from which developed the modern quantum theory.[126]

Bateman and Campbell were not the only British physicists to discuss the quantum problem in 1913, for there was also considerable discussion of it at the annual meeting of the British Association for the Advancement of Science. There had already been some discussion at the previous year's meeting, but this had been dominated by Bragg, who was not then convinced of Poincaré's proof of necessity, and by Rutherford, who went so far as to dismiss even Planck's law, on the grounds that a "double exponential" could be fitted to anything.[127] In 1913, Lorentz was invited to attend and the discussion, dominated by him and by Jeans, was far more sympathetic to quantum concepts. It was in fact at this meeting that Jeans announced his conversion to the light-quantum hypothesis, finally admitting of his suggestion that a final state might never be reached that

> "The more one works on this assumption, the more one is forced to realise that all the facts are against it." [128]

In both his 1910 paper and the discussions at the Solvay Congress he had indicated his belief that if quantisation were necessary it would have to apply to radiation itself, and he now declared that he felt "logically compelled to accept the quantum hypothesis in its entirety",[129] supporting this conclusion with a lengthy discussion of Poincaré's necessity proof.[130]

The necessity of the light-quantum concept was also conceded by Lorentz, but, repeating almost verbatim the opinions he had expressed at the Solvay Congress, he insisted that this conception was not sufficient, for

> "It must, I think, be taken for granted that the quanta can have no individual and permanent existence in the aether, that they cannot be regarded as accumulations of energy in certain minute spaces flying about with the speed of light. This would be in contradiction with many well known phenomena of interference and diffraction." [131]

Jeans also recognised this problem and had to admit, despite his own preference for the light-quantum theory, that

> "It is hardly too much to say that the two theories appear to be in active antagonism whenever they come in contact. Everywhere the undulatory theories demand that radiation should be capable of spreading and dividing indefinitely; while the quantum theory demands the reverse, at least when there is interaction between matter and ether." [132]

These discussions stimulated a lot of interest within the British Association, with the result that Jeans was asked to prepare a report on the situation. This was published in 1914,[133] and its main conclusion was on the need for some sort of dual conception:

> "It may be asserted with confidence that until some kind of reconciliation can be effected between the demands of the quantum theory and those of the undulatory theory of light, the physical interpretation of the quantum theory is likely to remain in a very unsatisfactory state. Probably the hope of most physicists is that some sort of a compromise may ultimately be effected, but at present any practical attempt at such a compromise appears to require the abandonment of something which is essential to one or other of the two theories." [134]

Jeans' report may be taken as the definitive British work of the period, and it firmly established the need for a wave-particle duality. In 1916, this conception also reached a popular textbook, namely the second edition of Richardson's Electron theory of matter,[135] in which he wrote that

> "It is difficult, in fact it is not too much to say that at present it appears to be impossible, to reconcile the divergent claims of the photoelectric and interference groups of phenomena. The energy of the radiation behaves as though it posessed at the same time the opposite effects of extension and localisation." [136]

Apart from the sympathetic attitude towards strucural hypotheses in general, there appear to have been two sets of factors behind the British recognition of wave-particle duality. The first set comprised Poincaré's necessity proof on the one hand and the problem of interference phenomena on the other. Jeans, in his report, took Poincaré's proof as the main argument in support of the light-quantum concept, and concerning the interference phenomena both he and Campbell drew on Taylor's 1909 experiments. Campbell argued that there could be "no doubt" of the essential features of the quantum theory, but that

> "If a single quantum occupies any appreciable fraction of this space [one cubic decimetre, according to the experiments] how can it be absorbed by a single electron; if it does not, how can it interfere with another quantum?" [137]

The second set of factors, equally crucial, comprised the results of two new experimental investigations, the details of which were published in 1912: that by Laue,[138] showing the crystal diffraction of X-rays, and that by Richardson and Compton,[139] which seemed to support the light-quantum interpretation of the photoelectric effect. The importance of Laue's result is indicated by its impact on Bragg. Having advocated his corpuscular theory of X-rays for many years, Bragg was too convinced of his position to reverse it on the basis of the new evidence, and he insisted that

> "These results do not really affect the use of the corpuscular theory of X-rays. The theory represents the facts of the transfer of energy from electron to X-ray and vice-versa, and all phenomena in which this transfer is the principal event." [140]

But the evidence did force him to admit, in 1912, that the problem was no longer to decide between two theories of X-rays, "but to find one theory which posesses the capacity of both",[141] and in 1913 he observed that

> "The problem remains to discover how two hypotheses so different in appearance can be so closely linked together." [142]

It must be remembered that Bragg was still thinking in terms of a material interpretation of X-rays rather than a true quantum concept, but this was not stressed in his discussions. Laue's result emphasised the similarity between X-rays and light, and, in view of the new interest in the quantum concepts, Bragg's observation must have been interpreted by many physicists as a statement of the wave-particle duality of light. Jeans, in his 1913 discussion, noted that the conflict between the quantum and wave theories was "perhaps, shown at its keenest in the case of the X-ray",[143] and this conflict may have been felt by many British physicists, who recognised the corpuscular properties of X-rays.

Richardson's results must have precipitated a similar conflict. Bragg's corpuscular theory had not been applied to light, Thomson's quantum hypothesis had been generally dismissed as ridiculous, and in Britain as elsewhere the desire for a wave theory of the photoelectric effect had been almost universal. A typical viewpoint was that of Millikan who wrote, concerning the photoelectric effect, that

> "The facts <u>which have been here presented</u> are obviously most completely interpreted in terms of [Einstein's] theory, however radical it may be. Why not adopt it? <u>Simply because no one has thus far seen any way of reconciling such a theory with the facts of diffraction and interference so completely in harmony in every particular with the old theory of ether waves.</u>" [144]

Richardson's results were a crucial factor in his own analysis of the quantum problem, and they were also used by Jeans as experimental support for Poincaré's theoretical arguments. They represented the most convincing experimental evidence yet in support of the light-quantum concept and, when placed alongside the results of Laue, Taylor and Poincaré, they provided very strong evidence indeed of the need for a dual wave-particle conception of light.

Quantum statistics

This evidence was supported, in a sense, by research into the nature of Planck's probability formula (appendix A, eqn.[9]). Planck's law was still the masterpiece of confusion it had always been, and in the most widely used derivation, Planck's of 1911, as well as in most other derivationsto date, the probability formula adopted in 1900 was still postulated without any physical justification or explanation. In the derivations of Einstein and Poincaré, and the analyses of Ehrenfest and Jeans, it was replaced by an assumption as to the possible energy values of the resonators or aether, but this avoided the whole issue of the nature of the quanta, by concentrating on the behaviour of the matter or aether to which they pertained. In the period 1912-15, however, sparked off by the necessity proofs, a series of papers were published, clarifying this issue.

In 1912, Natanson[145] tried to get to grips with Planck's combinatorial probability formula and with its implications for the structure of radiation. Planck's formula, for the number of ways of distributing P energy elements over N resonators, corresponded mathematically to the case where the energy elements were identical, or indistinguishable. Einstein in 1909 and Ehrenfest in 1911 had introduced the notion of non-independent quanta, but without specifying the dependence, and the notion of indistinguishability had still not been introduced at all in 1912. This was rectified by Natanson, who analysed Planck's formula from the mathematical point of view, distinguishing between three possibilities, namely

(i) that all the energy elements and all the resonators were distinguishable ("identifiable"),

(ii) that the resonators were distinguishable, but that the enrgy elements were not,

and (iii) that neither energy elements nor resonators were distinguishable. He correctly associated Planck's formula with possibility (ii), but he tied himself in knots in the process, and he did not discuss the structural implications at all. His paper could have been important, for by giving the mathematical background to Planck's formula it opened the way for physical discussion, but the confusion of the presentation was compounded with that intrinsic in the structural conceptualisation of indistinguishable particles, and the paper does not seem to have had much impact.

The next development was a paper by Krutkow,[146] in 1914, containing a derivation of Wien's law based on the explicit assumption of mutually independent light-quanta. The connection between the two had been indicated by Einstein in 1905, and Ehrenfest in 1911 had stated that independent quanta did not lead to Planck's law, but Ehrenfest had not given any analysis to back up his statement and a derivation such as Krutkow's was still needed to settle the issue, and to establish the quantum requirement firmly between the wave and particle limits (Rayleigh's law and Wien's law), as neither one nor the other.

Krutkow's treatment was complicated, being based on the consideration of a large system of balls in urns, the balls representing processes making available a quantum for possible absorption by the urns, or resonators. The finite energy of the resonators was expressed by letting the number of balls, "P", tend to infinity with the number of unabsorbed quanta tending in the same limit towards 'P'. The analogy was dubious, but if the limits were taken with due care (a difficult process) the method worked; the conclusion, moreover, was very clearly stated, which was the most important thing.

The first clear discussion of Planck's probability formula was contained in a short paper by Ehrenfest and Kam erlingh-Onnes[147] in 1915. They considered the problem of dividing the P elements (energy) among the N containers (resonators) by considering the possible arrangements of P elements and $N-1$ partitions in a one-dimensional box. Thus, for example, with nine elements and six partitions one possible arrangement would be

$$(\varepsilon\varepsilon / \varepsilon\varepsilon\varepsilon // \varepsilon / \varepsilon / \varepsilon\varepsilon /) \qquad [\ \varepsilon \text{ elements}, \ / \text{ partitions}\ ]$$

If both elements and partitions were treated as distinguishable, the total number of possible arrangements would be $\{P+N-1\}!$; but the partitions were naturally indistinguishable,[148] since they did not really exist, and if the elements were a so assumed to be indistinguishable the total number of arrangements would have to be divided by both the number of arrangements of the partitions among themselves and the number of arrangements of the elements among themselves. This gave Planck's formula, $W = \frac{(P+N-1)!}{P!(N-1)!}$.

The authors also referred to the assumption that Einstein had associated with Wien's law in 1905, namely that the relative probability of m particles being found in a volume v , as compared with v_0 , was $(v/v_0)^m$. Since this volume could be identified as the number of containers, this assumption was equivalent to $W \propto N^P$, and they claimed, but did not prove, that this led to Wien's law.

The importance of this paper was considerable, for although they did not consider the structural significance of the indistinguishability of the energy elements, and did not in fact go beyond the work of Natanson and Krutkow, Ehrenfest and Kamerlingh-Onnes expressed the mathematical situation in a very clear and appealing way, and to a very wide audience: their paper was published, in the leading journals of the three most important scientific communities, in English, German, and Dutch. To mathematicians, the paper must have looked trivial, but this only emphasises the point that Planck's formula was generally incomprehensible to physicists, not only when first put forward but also for many years thereafter.

I.4. A QUANTUM CONCEPTUAL IMPASSE

In the years following the first Solvay Congress, both Sommerfeld and Planck developed their theories. Sommerfeld, working with Debye, tried to apply his general principle to particular problems, including the black-body radiation law, but without much success, while in 1914 Planck introduced yet another derivation of this law,[149] in which both emission and absorption were assumed to be continuous. This was also unsuccessful, however. In 1913, the experiments of Marx and Lichteneker[150] indicated that there was no accumulation period in the photoelectric effect, and this result appears to have been a crucial factor in the decision of Sommerfeld and Debye to abandon the theory of action, which had turned out to necessitate continuous absorption. In 1916, Wilson[151] criticised Planck's theory on the same grounds. With the advent of the Bohr theory of the atom, Sommerfeld devoted his attentions to this, and in 1915 both Wilson[152] and Ishiwara[153] tried to bring together the Bohr theory and Planck's law. They both saw that the Bohr theory quantisation rule, which had been established by Sommerfeld, was identical with the quantisation rule for the Planck oscillators, but they interpreted this rule differently; for whereas Wilson adopted the standard interpretation in terms of a discrete set of possible states, such that "interchanges of energy ... are discontinuous",[154] Ishiwara adopted Planck's interpretation of regions of equal probability. This gave Wilson the opportunity to discuss Planck's hypothesis, and he did so the following year,[155] concluding that it necessitated both continuous and discontinuous energy changes, combining the worst of both worlds. Wilson's criticism was not in itself of crucial importance, as the point had already been made with greater force by Poincaré, but Planck's continuing failure to achieve a satisfactory derivation of his law finally turned his attention, like Sommerfeld's, to other matters. Still neither Planck nor Sommerfeld could bring themselves to accept the concept of light-quanta; but by 1916 their attempts to avoid this concept had failed.

By this time, the evidence in support of the light-quantum concept was fairly strong, but this concept was still rejected, not only by Planck and Sommerfeld but also by the majority of physicists. It was simply impossible, in the face of the evidence from interference phenomena, to abandon the wave picture of light, and although one could accept the idea of wave-particle duality this was impossible to interpret consistently in terms of structure; the same was true of the notion of indistinguishable particles. To most physicists this structural inconsistency of the dual theory was a worse fault than the inconsistency with experiment of the classical theory, for whereas unexplained experiments were a natural part of physics the criterion of consistency had always been paramount in any scientific theory.

Even those who did recognise duality did not surrender their hope of an eventual reconciliation of wave and particle aspects in some new, structurally consistent, interpretation. As much as anyone they retained the ideals of the nineteenth century, and what distinguished them from their colleagues was no deep philosophical chasm but a simple openness of mind. In part of course this distinction did stem from methodological differences, for the ideals of an electromagnetic or similarly behavioural world-view precluded the discreteness of the quantum concept, while the classical aether, and speculations upon its structure, had become so much a part of the 'suggestive' mechanical approach to physics as to be virtually unshakeable. Only those prepared to adopt an intermediate methodology were at all open to the conception of wave-particle duality, and even there there was naturally a strong element of conservatism. Even if the faults of the dual theory were accepted as no worse than those of the wave theory, they were still manifest, and most physicists saw no reason to change to a theory that was so blatantly unsatisfactory.

A partial comparison may be made with relativity theory. To the 'behavioural' physicists on the continent relativity and the light-quantum concept stood starkly opposed, the one being concerned largely with behaviour and the other with structure; they accepted relativity reasonably quickly and the quantum concept not at all. To the 'structural' physicists in Britain, on the other hand relativity and the light-quantum concept represented equal challenges to the classical aether concept. In the context of relativity theory, Goldberg[156] has found the pervasiveness of the aether concept to have been almost complete among British physicists at this time, and this played a significant part in the British reaction to the light-quantum concept. It is no accident that the first advocates of duality, Jeans and Campbell, were among the first in Britain to take relativity seriously.

Whether they accepted or rejected wave-particle duality, physicists were faced with a conceptual impasse, and the debate between the two viewpoints had also reached the same stage. The problem was still open to structural speculation, in respect either of light-quanta as structures in the classical aether or of quantally behaving atomic mechanisms, and I have considered the course of such speculations in appendix E; but they achieved no significant success and were to play no part in the eventual development of quantum mechanics.

Alternative attractions

As it happened, the conceptual impasse coincided with a variety of external factors, all of which led the attention of physicists away from the quantum problem, and there was therefore a pronounced lull in the discussions of this problem, from about 1915 right up to 1920.

Of the external factors, the most obvious was the war. Young scientists on both sides were apt to be conscripted, leaving a temporary shortage of fresh insight; there was a natural tendency away from the conceptual problems of physics towards practically applicable topics; and international communications between scientific communities suffered. The last point may have been particularly important, for the quantum problem as it then stood was more likely to have been discussed at personal meetings than through published papers.

The war was one factor. Another was the rival attraction of other branches of physics. As it happened, the period produced two new theories, both of which offered exceptional scope for investigation, and many physicists who might otherwise have been attracted towards the quantum problem turned instead to these. One was the theory of relativity, and this had a strong dual appeal. The problem of its application to the many relevant branches of physics provided an enormous amount of routine work, while, more importantly, the theory also had an unrivalled appeal for those interested in basic conceptual problems. Relativity theory was in its early stages, with fair prospects for development, whereas quantum theory seemed to be at a dead end, and it was not only fundamental but also unequalled in terms of immediate philosophical content. It attracted precisely the kind of physicist who would be interested in the quantum problem away from that problem.

The development of relativity theory was also relevant in another way, for the British, as we have seen, were peculiarly fond of the aether, and thus reluctant to accept the new theory. It began to be a major research topic in Germany in about 1910, and this may explain why the Solvay Congress does not seem to have attracted many German physicists, apart from those already involved, to the quantum problem. In Britain it was not taken seriously for another five years, and this left the conceptually inclined British physicists free to concentrate on the quantum problem. When relativity did arrive in Britain, it did indeed have the suggested effect, most notably on Jeans, who followed up his 1914 report by turning away completely from quantum theory, towards relativity theory.

The second new theory was Bohr's theory of the atom. This was itself a very important element of the history of quantum concepts, and I shall consider it below, but it left, as we shall see, the wave-particle issue completely open, and in fact attracted attention away from this issue. Once Bohr's idea had been accepted (on empirical rather than conceptual grounds) as a working model, it became the basis of a wholly technical theory with relatively little relation to any conceptual discussion of the quantum problem. Given the Bohr model-building rules, and the subsequent development of these, there was an immense amount of work to be done following the theory through for particular elements and in particular conditions. Moreover, the theory did not quite work: it was good enough to justify an immense amount of research, but just poor enough to offer in addition a strong element of challenge, and it thus offered scope for both routine development and speculation. It combined the appeal of research at the frontiers of physics with that of a game, and it attracted several physicists, most notably Sommerfeld and Landé, who might otherwise have concentrated on more general aspects of the quantum problem.

<u>The old quantum theory</u>

The old quantum theory, based on Bohr's atomic model, was virtually the sole subject of quantum investigation in the period 1915-20. Apart from a contribution by Einstein, it avoided altogether the crucial issue of the structure of light and was largely a technical theory, irrelevant to this or to any other basic conceptual issue. Despite this, however, it did represent a vital stage in the conceptual development of quantum theory, for reasons that do not seem to have been recognised by historians. In the first place it acted as a training ground for young physicists, who reached maturity quite familiar (as previous generations had not been) with the idea of quanta; it also acted to familiarise physicists in general with this idea. Secondly, as we shall see in the next chapter, it prompted a serious conceptual crisis when its apparent success turned suddenly into failure. Thirdly, in the very act of ignoring the quantum problem it prepared the way for the [illegible] changes that were to come in physical methodology.

In Bohr's atomic model, conceived in 1913,[157] both emission and absorption were supposed to be quantised and effectively discrete. Considering the difficulties of incorporating discrete absorption into the classical framework, the model might be supposed to have been based on the light-quantum concept, but in fact Bohr does not seem to have considered the wave-particle issue until after the theory had become established, and the model was taken up by others as an idealisation, leaving room in principle for continuous transitions. The field singularities of the electron theory had <u>not</u> generally been associated with dimensionless point-masses, and in the same way the quantum jumps in Bohr's theory were not generally associated with physically instantaneous transitions. The theory actually afforded some hope to the adherents of the classical conception of light, for it seemed to attribute the quantum behaviour explicitly to the behaviour of the atom, rather than to the structure of light, and this aspect is reflected in the fact that it was developed largely by Sommerfeld, who still could not accept the light-quantum concept. Bohr himself, when he did consider the quantum problem, also decided against this concept.

Bohr's theory was based upon five assumptions:-

(i) Emission and absorption of radiation by an atom take place only in transitions between 'stationary states' of the atom.

(ii) The equilibrium between these states is governed by classical mechanics, but the transitions are not.

(iii) The radiation emitted or absorbed in a transition is homogeneous, of frequency ν given by $E = h\nu$ where E is the energy difference between the two stationary states.

The quantisation rule, governing which of the infinitely many possible states could be adopted as stationary states, could for a simple system (electron orbiting around a positive nucleus), be given by

(iv) The angular momentum of the electron is an integral multiple of $h/2\pi$, with the normal or permanent (lowest energy) state of the atom being given by

(v) The angular momentum of each electron in a permanent state equals $h/2\pi$.

This model arose largely out of the problem of giving stabiltiy and natural dimensions to Rutherford's classical atom. This led naturally to the qualitative features of the model, the stationary states being stable Rutherford orbits on the basis of considerations purely internal to the problem. The quantum, h, which had been linked with spectral emission by Nicholson[158] in a paper that Bohr read while struggling with the problem of achieving the stable orbits, entered the model as a natural constant whose dimensions and order of magnitude satisfied Bohr's requirements. The details of the model became clear to Bohr when he saw how it could be tied in with Balmer's spectral formula, and did not, apparently, arise from any consideration of the quantum problem.[158a]

Development of the old quantum theory

For the most part, the development and application of the theory were as independent of the quantum problem as its inception had been.

The theory could be likened to a game with three rules, the first of which was the quantisation rule. In 1915, Sommerfeld[159] introduced the generalised quantum conditions, $\oint p_k \, dq_k = n_k h$, for each pair of momentum and position coordinates (p_k, q_k), and the next year he succeeded[160] (as did Debye) in explaining the normal Zeeman effect. Later, periodic quantum theory problems were linked with the classical Hamilton-Jacobi theory for systems with separable equations,[161] the action variable being defined as $J_k = \oint p_k \, dq_k = n_k h$.

This formulation was introduced by Schwarzchild and Epstein, who combined it with the second rule of the theory, Ehrenfest's adiabatic principle,[162] to explain the first order (peculiar to Hydrogen) Stark effect.[163] The adiabatic principle stated that

> "If a system can be affected in a reversible adiabatic way, allowed motions [stationary states] are transformed into allowed motions",[164]

and it allowed one to establish the stationary states of a complex system adiabatically related to a simple one and, more generally, to carry some of the conclusions of the classical theory over into the quantum theory.

In 1918, Bohr[165] introduced the third rule in the form of the correspondence principle [CP]. This was based on an assumed "necessary connection" between the quantum and classical theories "in the limit of slow vibrations" (i.e. as $\nu \to 0$), and was actually applied in the slightly wider limiting region of high quantum numbers, and, more generally still, wherever the classical theory gave what seemed to be the right answer. Bohr considered a system with one degree of freedom, and a transition between two states of this system with high quantum numbers and close frequencies. For two states (n', ω') and (n'', ω'') whose energies were $E' = hn'\omega'$ and $E'' = hn''\omega''$ (for the Planck oscillator, $\omega' = \omega''$, but this did not hold in general), he considered the limit "where n is very large [$n'-n'' << n'$], and where the ratio between the frequencies of the motion in successive stationary states differs very little from unity",[166] in fact requiring $\frac{\omega'}{\omega''} \rightarrow 1$ faster than $n \rightarrow \infty$. In this case the frequency of emitted or absorbed radiation was given by $\nu = (n'-n'')\,\omega$, where $\omega \sim \omega', \omega''$, and Bohr compared this with the classical Fourier expression for particle displacement, $\sum C_\tau \cos 2\pi(\tau\omega t + c_\tau)$. He linked the classical harmonic given by τ_o with the transition $\tau_o = (n'-n'')$, and reasonably expected the probability of a quantum emission of the frequency $\tau_o\omega$ to be the same as the intensity of a classical emission of the same frequency, namely C_{τ_o}. Finally, he suggested that this relationship should hold in some (vague) way for small quantum numbers too, despite the fact that the frequencies could no longer be expected to correlate there ($\omega' \neq \omega''$) as they had done, in practice, for the higher states.

The quantisation rule and the adiabatic and correspondence principles formed the basis of the old quantum theory, and all were essentailly behavioural in their emphasis, and unrelated to the problem of the structure of light. The theory as described so far was, however, restricted in that it could treat only the stationary states of the atom and not its transitions; when Einstein remedied this, in part, in an extremely important paper of 1916, he also provided further support for the light-quantum concept.[167]

Einstein saw that the rate of emission (or absorption) of radiation by an atom according to the classical theory would have to take a new form in the quantum theory based upon the Bohr atom. The discrete emissions (or absorptions) would have to take place either at regular intervals, which led to impossible situations, or else irregularly. In the latter case, he saw that to agree with the experimentally verified classical intensities there must be defined a probability of emission (or absorption) such that over a long period the total energy of a given frequency emitted (or absorbed) agreed in the two theories. Classically, an atom was known to emit energy freely, unprovoked by any outside field, and also, in the presence of such a field, both to absorb energy from it and to be stimulated by it into emitting energy of the same frequency as the field. Einstein therefore assumed, for transitions between two states m , n , (taking $E_m > E_n$) the existence of probabilities:

Probability of emission $dW = A_m^n\, dt$
" " absorption $dW = B_n^m \rho\, dt$
" " stimulated emission $dW = B_m^n \rho\, dt$,

ρ being the density of external radiation of frequency $\nu = \frac{(E_m - E_n)}{h}$.
As he explained,

> "I was led to these hypothese by my endeavour to postulate for the molecules in the simplest possible manner, a quantum-theoretical behaviour that would be the analogue of the behaviour of a Planck resonator in the classical theory." [168]

Einstein did not discuss the question of a mechanism, but he likened the situation to that of a radioactive decay, stating that

> "The statistical law which we assumed, corresponds to that of a radioactive reaction, and the above elementary process corresponds to a reaction in which only γ-rays are emitted." [169]

This suggests that, as we should expect, he intended the transitions to be causally determined, for although causality in radioactive reactions had been challenged it was almost universally accepted that some hidden mechanism did exist, responsible for the decays. In Bohr's theory, an unknown mechanism for the transitions had been assumed to exist, and this situation was unchanged by Einstein's development.

Einstein's new hypothesis, and the derivation of Planck's law to which it led (discussed further in appendix G) constituted the first and more immediately important part of his paper. A second part was largely ignored at the time, but was considered by Einstein to be "more important" than the first. He noted that "in general one restricts oneself to a discussion of the energy exchange, without taking the momentum exchange into account",[170] and, suggesting that this restriction was theoretically unjustified, he rectified the situation by "proving" — or very strongly suggesting — that

> "If a radiation bundle has the effect that a molecule struck by it absorbs or emits a quantity of energy $h\nu$ in the form of radiation (ingoing radiation), then the momentum $h\nu/c$ is always transfered to the molecule. For an absorption of energy, this takes place in the direction of propagation of the radiation bundle, for an emission in the opposite direction. ...
> If the molecule undergoes a loss in energy of magnitude $h\nu$ without external excitation, by emitting this energy in the form of radiation (outgoing radiation), then this process too is directional. Outgoing radiation in the form of spherical waves does not exist. During the elementary process of radiative loss, the molecule suffers a recoil of magnitude $h\nu/c$ in a direction which is determined only by "chance" according to the present state of the theory."[171]

Thus light was found to be clearly particulate, carrying a definite localised momentum. Although he had not given up his dual viewpoint, Einstein here placed a very strong emphasis indeed on the particle aspect of light, presumably feeling that this aspect was still in need of such emphasis.

That this was indeed the case, despite recognition of the wave-particle duality, is indicated by the fact that Einstein's was the only published defence or use of the light-quantum concept in the period 1916-20. His conclusions were supported, in retrospect very strongly, by Millikan's experimental results of 1916,[172] which showed such close agreement with Einstein's prediction on the photoelectric effect as to destroy any hope that the light-quantum theory could be proved wrong in that context. Millikan himself, however, could not accept the light-quantum concept, despite his own results, arguing in his 1917 book on the electron that the pure particle theory of light "is found so untenable that Einstein himself, I believe, no longer holds to it",[173] and refusing to accept any dual conception: this must have negated a lot of the support his results might otherwise have afforded to the light-quantum concept, and there appears in fact to have been no attempt, prior to the 1920's, to use the results as an argument for this concept.

Indeed, there was as we have noted little discussion of any kind on the quantum problem in the years following Einstein's paper; but what little there was was entirely opposed to the light-quantum concept. In 1916, Barkla[174] argued from X-ray evidence that all absorptions and emissions were continuous, so that quanta must be characteristic of the atom and not of radiation, and although the evidence was not very compelling Barkla's conclusions were cited in 1918 by Schott,[175] who stuck rigidly to the classical conceptions. Continuous radiation theories were also put forward that year by Bichowsky[176] and Flaumm,[177] while in 1919 Houston[178] claimed that X-ray ionisation, always looked upon as a quantum phenomenon, could in fact be explained classically. Millikan, whose book on the electron went through eight impressions before 1923 and carried considerable weight, discussed extensively the two alternatives of the light-quantum hypothesis on the one hand and "a peculiar property of the inside of an atom"[179] on the other and came out strongly in favour of the latter, even though he found it "equally subversive of the established order of things in physics."[180] The Einstein theory (which had the misfortune to be linked here with Thomson's hypothesis) was found to be "erroneous", "wholly untenable", and "in fact... pretty generally abandoned."[181]

The methodology of the old quantum theory

By 1920 there had been no real progress on the wave-particle issue for several years. The debate between the few proponents of duality and the many of the classical wave theory had long been at an impasse, and neither group had been able to advance towards a conceptualisation that was consistent both with experimental evidence and internally, in a structural sense.

Things had changed over the years, however, for the old quantum theory was now well established; this had made the general concept of quanta familiar to physicists as a whole, while a new generation of young physicists had started research from a quantum viewpoint (albeit a rather vague one) rather than from that of the classical theory. Of particular importance was the fact that the physicists working in the old quantum theory were, consciously or otherwise, preparing for a move away from classical methodology. This, like all the other aspects of our characterisation of classical physics, was as yet unchallenged, but certain features of the old quantum theory reveal a definite, if unconscious, shift of perspective.

This change does not seem to have been analysed by historians, but it can be seen in at least two aspects of the theory: Bohr's model itself, and the way this model was used.

Bohr's model had, from a methodological standpoint, two features: first, it was a model of the inside of the atom, and secondly it was explicitly at variance with classical mechanics and the electron theory. The first of these features was characteristic of a structural methodology, while the second could only have arisen, normally, within a behavioural methodology, and together they represented a sort of freethinking positivism, altogether new to modern physics. J.J.Thomson speculated freely on atomic models, but these were subject to classical mechanics and they were also intended as suggestive, not to be taken too seriously, but Bohr's model was the complete opposite: it was intended as a serious picture, as reality, but it was not bounded by the classical theory. In an excellent analysis, Heilbron and Kuhn[182] have shown how Bohr, who started his research just when the classical theory was running into serious trouble early in the century, was already convinced by 1911 that the classical mechan cs was wrong. This put him in a unique position, for whether they had given priority to mechanics or to the behaviour of the electromagnetic field, physicists had not previously doubted the validity of classical mechanics. The combination of Bohr's view in this respect with his general use of a structural approach, in the tradition of which classical mechanics was most deeply entrenched, made his position doubly unique ; it meant that he judged a theory by its consistency on its own terms (rather than on those of the classical theory), and above all, since without a correct theory he saw nothing else to work on, by its results.

In the case of his atomic model, the results were dramatic. Had they not been so it would probably have been ignored, some physicists declining to take an atomic model seriously and the remainder refusing to countenance the departure from classical theory, but as it was the model could not be ignored by any physicists with the slightest openness of mind. It was taken up by 'behavioural' physicists who were prepared to overlook the lack of a sound mechanical basis and especially by Sommerfeld, who liked a basic picture to work with[163] but who preferred, as we have seen, mathematical consistency and simplicity to detailed mechanisms.

The second innovation stemmed from the first and concerned the use of electron orbits and quantum numbers. Bohr had propounded the model as 'real', and its success ensured that it was taken as being effectively so, at least, but since structure took second place to results in the priorities of those working on the model the details of the structure were varied at leisure. Just as astronomers of old had adjusted their epicycles to agree with any new data, so Sommerfeld and his colleagues adjusted the choice and relative configuration of electron orbits, in trying to apply the model to higher elements.[164] They did not concern themselves much with why this or that orbit should be preferred, and they thus continued the juxtaposition of a 'real' model with a totally positivistic attitude to its details started by Bohr. With the introduction of quantum numbers this trend was exaggerated, for although the numbers were originally descriptive of physical properties they soon came to precede these properties, the choice of a new quantum number (a new degree of freedom) preceding the debate as to what this new number represented physically. The positivism in the choice of orbits was thus continued, and strengthened, in the treatment of quantum numbers.

II

THE QUANTUM CONCEPTUAL CRISIS (1921-23)

By 1920, there had been no great interest in the conceptual problem of quanta for over five years. In the early 1920's, however, the situation changed dramatically, for there suddenly arose a strong revival of interest in the problem. This led, through a series of conceptual crises, to the development in the middle of the decade of the new quantum mechanics. Although they have not been analysed comprehensively by historians (many aspects have not even been mentioned), the roots of the new theory in the crises that preceded it are deep and complex. The main elements of the new 'quantum perspective', replacing the classical ideals of physics, had their origins in the crisis period, although they were not formalised or generally recognised until some years later. I shall therefore analyse this period in some detail, drawing where possible upon unpublished source material and refraining from any reliance (which was occasionally necessary in chapter I) upon secondary sources.

The conceptual developments with which I shall be concerned fall into two categories, reflecting the division already noted between the general conceptual analysis of the quantum problem and the technical development of the old quantum theory. Perhaps the most important developments in the period 1921-3 were those arising from the failure of the old quantum theory: first discussed in 1921, this failure led to the introduction of basic conceptual discussion into the theory, and to a severe conceptual crisis . This development was paralleled by, but only vaguely connected with, that of the more general conceptual discussion. The revival of general interest, which can also be dated to about 1921, led on the one hand to de Broglie's concept of matter-waves, probably the most important single concept for the origins of the new theory, and on the other hand to such radical concepts as energy non-conservation and the abandonment of strict causality, crucial factors for the origins, interpretations and ideals of that theory. I shall first analyse the sources of the revival of general interest , then the crises to which this revival led, and then the crisis of the old quantum theory; finally in this chapter I shall discuss these developments in the context of the changing characterisation of physics with which I am primarily concerned.

II.1. THE REVIVAL OF GENERAL INTEREST IN THE QUANTUM PROBLEM, AND THE SHARPENING OF THE WAVE-PARTICLE DILEMMA.

There were undoubtedly many sources of the general revival of interest in the quantum problem in the early 1920's, but three in particular may be isolated: the problems of relativity theory , the evidence of new X-ray experiments, and the development of Bohr's ideas.

In the early days of relativity theory, its conceptual attractions had seduced many physicists who might otherwise have devoted themselves to the quantum problem. Relativity theory, however, was concerned with the fundamental properties of matter and light; once the mathematics of the theory had been sorted out, the physicists found themselves faced with problems concerning the structure of matter and light, and this naturally brought some of them back to the quantum conceptions. The first interaction between relativity and quantum theory seems to have followed from the publication, in 1918, of Weyl's general theory of gravitation and electromagnetic phenomena.[2] In 1919, Pauli published a criticism of this theory,[3] and the following year he discussed his work with

Einstein, Einstein writing to Born that

> "Pauli's objection is directed not against Weyl's but also against anyone else's continuum theory. Even against one which treated the electron as a singularity." [4]

This objection was not originally related to quantum concepts by Pauli, but it clearly was by Einstein,[5] and Pauli himself later supported the light-quantum concept.

In 1921, Weyl again published a controversial paper,[6] this time launching one of the biggest debates in the history of relativity theory. Weyl, who was supported by Eddington,[7] claimed that matter was the prime existent behind the space-time field, while Einstein defended the concept of a pure field theory. This debate sparked off a spate of theories, and in 1922 Bucherer[8] tried to use the light-quantum concept as the basis for a physical theory of gravitation, the mathematical framework of which was supplied by general relativity. Meanwhile, the problems of special relativity had also led to consideration of the light-quantum concept. In 1921 Emden[9] tried to apply this concept to the problem of the Doppler shift, and in 1922 Schrödinger[10] pursued the same course more rigorously and more successfully.

Just as relativity theory prompted a renewed interest in the light-quantum concept in about 1921, so did experimental considerations. At the third Solvay Congress, held in that year,[11] Maurice de Broglie read a paper entitled "The relation $E=h\nu$ in photoelectric phenomena". This was a survey of a wide range of experimental effects of the photoelectric type (and the inverse thereof), all of which pointed to a light-quantum interpretation, and the same year Louis de Broglie published an analysis of light based upon this interpretation,[12] writing that his brother's experimental results had convinced him that both emission and absorption were discrete processes, and that the light-quantum concept should accordingly be investigated. Also at the 1921 Solvay Congress, Millikan announced his acceptance, at last, of the light-quantum concept,[13] undertaking a heated defence of this concept against Barkla. Millikan's conversion, which was naturally extremely important, was based upon some new photoelectric experiments, in which he found that

> "Contrary to preceding views including my own, the energy "$h\nu$" is transfered ... to the free, i.e., the conduction electrons of the metal, and not merely to those bound in atoms." [14]

This seemed to leave the quantum property as "an intrinsic property of light itself", and Millikan concluded that

> "The burden of accounting for the emission of electrons with the energy $h\nu$ can no longer be thrown back upon some unknown mechanism in the structure of the atom." [15]

The situation was quickly confused when an argument was found that seemed to evade this conclusion,[16] but the event still served to encourage renewed interest in the light-quantum concept.

Both the influences discussed so far tended to support the light-quantum concept, but the third was in strong opposition to it. Since entering the quantum theory in 1913 with his atomic model, Bohr had thought a lot about the quantum problem, and had come out wholeheartedly against the light-quantum concept. In his 1918 survey of the foundations of quantum theory,[17] he had adopted the Einstein probability coefficients for transitions, but had blatantly ignored the rest of Einstein's 1916 paper concerning the localised momentum of radiation. In 1920, he wrote that

"I shall not here discuss the familiar difficulties to which the hypothesis of light-quanta leads in connection with the phenomenon of interference, for the explanation of which the classical theory has shown itself to be so remarkably suited.",[18]

but in 1921, at the Solvay Congress,[19] he did discuss them, concluding that

"Such a concept [as the light-quantum] ... presents apparently insurmountable difficulties from the point of view of optical interference."[20]

Finally, in 1923, in the course of the most clear and extensive account of quantum theory to date, he lashed out at the light-quantum concept, drawing on the familiar problem of interference, and also on another problem that had been bothering him, namely how to define the 'frequency' of a particle:

"As is well known, this hypothesis introduces insuperable difficulties, when applied to the explanation of the phenomena of interference, which constitute our chief means of investigating the nature of radiation. We can even maintain that the picture, which lies at the foundation of the hypothesis of light-quanta, excludes in principle the possibility of a rational definition of the conception of a frequency ν , which plays a principle part in this theory."[21]

In private, Bohr had been worried about frequency for some years, for, as he had explained to Darwin in 1920, one could only define a frequency through a wavelength, and a wavelength through interference, and interference through the wave concept of radiation.[22] Darwin, who had suggested, in 1919, work that

"would force us to look for our modification in Planck rather than Maxwell; a consequence I should regard as very satisfactory",[23]

agreed, and also had another objection, relating to the frequency of a transition. If, in an absorption, a quantum of light was absorbed with a definite frequency, then the final state of the atom was presumably determined only after the absorption took place; but on the other hand the absorption could only take place, apparently, if the frequency of the quantum had one of a given set of values. As he expressed the situation in 1923:

"It is not possible to assume that the atom goes right into its upper quantum state: but instead we are forced to believe that the atom, so to speak, knows what the upper state is like without going there."[24]

Silberstein had also referred to this, in 1920, calling it

"an extraordinary performance, one, that is, that enables the atomic system to hit precisely upon the frequency required."[25]

The problem could be avoided by viewing the transition as a whole, and was if anything more severe in the case of the wave conception of light than in that of the light-quantum conception; but Darwin saw it the other way round. His correspondence with Bohr reveals a strong desire to be approved of by the latter, and it was under Bohr's influence, apparently, that he argued in 1923 for a reconciliation of wave and particle views, but one based upon the wave view as primary.[26]

It is clear that by 1923 the wave-particle controversy was heating up considerably. Compton's famous results,[27] revealed at the end of 1922, completed this process, providing the strongest evidence yet for the light-quantum concept. Since the effect is so well known, and since its history has been thoroughly analysed elsewhere,[28] we shall not describe it here, but shall rather concentrate on reactions to it.

Compton's own deductions from the evidence, made apparently without prejuduce, are clear:

"The obvious conclusion would be that X-rays, and so also light, consist of discrete units, proceeding in definite directions, each unit posessing the energy $h\nu$ and the corresponding momentum h/λ ."[29]

"Both from the standpoint of the experimental evidence and from the internal consistency of the theory we ... seem forced to the conclusion that each quantum of scattered X-rays is emitted in a definite direction." [30]

Many physicists clearly disagreed with Compton's conclusions, and many published attempts at alternative explanations of his results avoiding the light-quantum concept;[31] Compton himself, between writing and submitting the paper on quantum scattering, actually published a paper[32] on the total internal reflection of X-rays, a phenomenon which, as he admitted, was "not easy to reconcile"[33] with the conclusions he had drawn from the scattering results. But both the experimental results and the theoretical conclusions nevertheless had a considerable impact upon the wave-particle problem. In the first place, helped partly by Duane's challenge as to the correctness of the results, which was kept up for about a year,[34] Compton's work became very well-known in fairly wide circles, attracting a lot of attention to the problem. Secondly, the arguments presented in support of the light-quantum concept were very strong; in general terms they were the first arguments to have sufficient force to really break the domination of those drawn from interference effects and arguing against this concept, and more particularly they do seem to have encouraged the conversion of one notable physicist to the light-quantum viewpoint. This was Sommerfeld, whose opposition to the light-quantum had once been very strong indeed. Sommerfeld, who had written to Bohr in 1918 that

"The wave process occurs only in the aether, which obeys Maxwell's equations and acts quantum-theoretically as a linear oscillator with arbitrary eigenfrequency ν. The atom merely furnishes a definite amount of energy and angular momentum as material for the process.", [35]

a clear statement of the alternative to the light-quantum concept, now wrote to him of the Compton effect that "after it the wave theory of Röntgen rays will become invalid",[36] and to Compton that "there can be no doubt that your observation and theory are accurate".[37]

Sommerfeld's conversion had in fact been a possibility for some time. Heisenberg, studying the anomalous Zeeman effect, had concluded at the end of 1921 that "in order not to conlict with experience" it was necessary to "place ourselves deliberately in opposition to classical radiation",[38] and although Sommerfeld had argued then that energy non-conservation was an alternative solution,[39] he admitted to Einstein very soon afterwards that "inwardly I also no longer believe in the spherical waves."[40] The conversion had other causes too, for Debye, independent of Compton and from more theoretical considerations but at about the same time, also put forward a theory of scattering based upon the light-quantum concept.[41] Sommerfeld and Debye were past colleagues, and some influence is extremely likely in this particular case, while, more generally, the coincidence of Debye's and Compton's results must have strongly emphasised their importance. Sommerfeld may also have been influenced by Pauli, who had been his student, and who applied Compton's results in 1923 to a successful probabilistic treatment of the temperature equilibrium between radiation and free electrons.[42] This was the first quantum treatment of the problem to give both the Maxwell distribution for the electrons and Planck's formula for the radiation, and it represented a very notable achievement of the light-quantum viewpoint.

Since Compton's result led to no other notable conversions, we may conclude that its impact was in some ways limited, and insufficient to overcome the existing arguments against the light-quantum concept, but it was at least sufficient to stand up against these arguments, and to bring the wave-particle issue into a much closer balance.

Thus, surveying the situation in 1923, we find that the two sides in the wave-particle debate were more equally matched, and more critically aware of their differences, than they ever had been before. Apart from those who clung absolutely to the classical theory, the quantum physicists could be distinguished in terms of this debate by two contrary viewpoints:

(1) that light should be conceived as localised particles interacting mutually and with matter, in some as yet undetermined way, to create wave-like effects, (the light-quantum viewpoint)

and (2) that light should be conceived in terms of a continuous dissipated wave-motion interacting with matter, again in an undetermined way, to to create quantum effects.

Of these, the second was still the more popular. On one hand, it carried the authority of Bohr, as we have seen, and of Planck, who had been unsuccessful in his attempt,[43] in 1921, to criticise Poincaré's arguments for the necessity of the light-quantum concept, but who still refused to accept this concept. On the other hand, it totally dominated the non-specialist accounts of the quantum problem. Millikan's doubts did not reach his book until the 1924 edition (by when they had been confirmed as a result of Compton's results) , while in 1922 Kramers, heavily influenced in all probability by Bohr, had written in a semi-popular work that

"The theory of light-quanta may thus be compared with medicine which will cause the disease to vanish and kill the patient." [44]

In 1923 Adams, in a commissioned review article, wrote of the light-quantum hypothesis that

"The impossibilities, in the present state of our knowledge, of reconciling such a view with the great mass of evidence arising from physical optics makes any such hypothesis very improbable.",[45]

however good it might be at explaining the photoelectric effect. We should also note that Jordan, one of the young physicists who were to dominate the development of quantum mechanics, was in 1923 pursuing some serious anti-light-quantum research. Jordan always maintained a positivistic attitude, but there is surely some significance in the fact that he chose for his Ph.D dissertation[46] to try and counter the arguments Einstein had used in support of the light-quantum concept in 1916. This choice of subject may also indicate something of his professor's viewpoint, in this case Born's.

The balance of opinion favoured viewpoint (2), but the balance of evidence favoured neither viewpoint, and since the proponents of (1) were more ready to consider a dual conception (the problem being one of acceptance of the light-quantum concept, not of rejection of the wave concept) they could claim the advantage here. Foremost among these proponents was Einstein, who in 1921 claimed to have found a decisive experiment in support of the light-quantum concept.[47] He had not done so, as Ehrenfest showed him,[48] but his support for the concept continued, and by 1923 this viewpoint was shared by Sommerfeld, Debye, and, important for the future, Schrödinger and Pauli.

To conclude, we must note that, as the dilemma sharpened, most physicists came to realise that, whatever their views on the subject, they were basically ignorant. Their opinions were based on personal and methodological grounds, rather than on hard evidence. Heisenberg recalled[49] that no-one really understood the wave-particle problem, and that, with the prominent exception of Bohr, few of those active in the old quantum theory would take sides with any conviction. They would use one model here, another there, depending which gave the right answer.

This approach was noted by other physicists too, in respect both of X-ray theory and the old quantum theory. Bragg wrote in 1921 that

> "In many ways the transference of energy [X-rays in Coolidge bulbs] suggests the return to Newton's corpuscular theory. But the wave theory is too firmly established to be displaced from the ground that it occupies. We are obliged to use each theory as occasion demands and wait for further knowledge as to how it may be possible that both should be true at the same time.", [50]

and, the same year, Emden came to a similar conclusion:

> "As is well-known, the laws of optics can no longer be traced back to a foundation. The once omnipotent wave theory fails over wide areas which can be handled simply and completely by the acceptance of the light-quanta. However, in the region of the wave theory, the light-quantum equally leads only to hypothetical dilemmas, so as sooner or later to vanish again. The discord, which shows itself through the antithesis of the two outlooks, can mostly be avoided, if one always stands on the ground of that theory which can handle the phenomenon in question the simplest." [51]

Bohr too, despite his strong stand on the issue, declared in his paper to the 1921 Solvay Congress that

> "We must admit that, at the present time, we are entirely without any real understanding of the interaction between light and matter." [52]

Einstein had written to Born in 1919 that

> "The quantum theory gives me a feeling very much like yours. One really ought to be ashamed of its success, because it has been obtained in accordance with the Jesuit maxim:'Let not thy left hand know what thy right hand doeth'.", [53]

and in early 1924 he found the two theories of light to be

> "both indispensable and - as one has to admit today in spite of twenty years of immense efforts by theoretical physicists - without any logical connection." [54]

Lorentz wrote of the quantum theory in 1923 that

> "All of this is of great beauty and of extreme importance, but we do not understand it.", [55]

and in the 1922 edition of his famous textbook, the influence of which cannot be overemphasised, Sommerfeld wrote that

> "Modern physics is thus for the present confronted with irreconcileable contradictions, and must frankly confess its 'non liquet'.", [56]

while in 1921 Born had made the definitive comment in a letter to Einstein:

> "The quanta really are a hopeless mess." [57]

They were.

II.2. THE CONCEPTUAL CRISIS: (1) DUAL PICTURES AND DE BROGLIE'S THEORY [58]

Admissions of ignorance constituted one, rather negative, aspect of the reaction to the sharpening of the wave-particle dilemma, but there was also a more positive aspect. Realising that neither the wave nor the particle aspect could account in itself for the phenomena, several physicists sought in the early 1920's to overcome this, either by combining the two concepts in a 'dual' theory or by making radical and fundamental conceptual changes. The former path had already been followed by Einstein in 1909 and by Campbell in 1913, and both their suggestions were to be taken up again. The first to renew the attack on the problem was Einstein, apparently stimulated by his thoughts on general relativity.

Writing to Born early in 1920, Einstein referred to Pauli's objection against continuum theories and, relating this to the quantum problem, expressed his own opinion that

> "I myself do not believe that the solution to the quanta has to be found by giving up the continuum. ... I believe now, as before, that one has to look for redundancy in determination by using differential equations so that the solutions themselves no longer have the character of the continuum." 59

Einstein's opinions throughout the history of quantum theory were extremely important, but often extremely confusing to the historian. We may recall that he had no sooner convinced himself that the light-quantum, as a physical phenomenon, had to exist, than he had realised that the underlying theory must be a continuous field theory, with action propagated contiguously. This realisation, based on relativistic considerations, lay with the problem of interference phenomena behind his arguments for duality in 1909, and it was also at the root of the above statement. That he should have continued to argue in support of the structural light-quantum concept (as in his 1921 correspondence with Ehrenfest),while at the same time insisting upon a continuous and apparently non-structural field theory (matter being treated as secondary to the field), has naturally led to some confusion, but the viewpoint can be understood. Einstein's insistence upon a continuous field theory was based on fundamental considerations, and the statement quoted above represented his ideal; but his methodology, which had not changed since 1905, allowed the use of structural concepts as a foundation for the behavioural theory and its description. Indeed, he thought that description was only possible with the aid of structural concepts, and since empirical considerations favoured the light-quantum concept he felt that the behaviour, although defined continuously, had to be described in terms of this concept. That matter was treated as a secondary phenomena was also natural, for, it must be realised, Einstein's concept of a field was in itself structural, much as Faraday's had been.[60] In summary, Einstein was prepared to use structural concepts both to interpret behaviour physically and to guide him to this behaviour.

The statement quoted above reflected Einstein's ideal position, but it was not one he could immediately attain, and he therefore investigated, as a guide towards it, some purely structural dual pictures. One of these was expounded in 1922 by Lorentz, acting in his customary role as publiciser of interesting ideas:

> "The hypothesis of light-quanta, however, is in contradiction with the phenomena of interference. Can these two be reconciled? I should like to put forward some considerations about this question, but I must first say that Einstein is to be given the credit for whatever in them is sound. As I know his ideas concerning the points to be discussed only by verbal communication, however, and even by hearsay, I have to take the responsibility for all that remains unsatisfactory.
>
> "Let us suppose that in the emission and propagation of light, there is something which conforms wholly to Maxwell's equations, but that it has practically no energy at all, the electric and magnetic forces being infinitely small. Then in this, let us say, Fresnel radiation we shall have the ordinary laws of reflection, interference, and refraction, but we shall see nothing of it. On a screen you will have something like an undeveloped photographic image.
>
> "We can now imagine that in the production of light this Fresnel radiation is accompanied by the emission of certain quanta of energy that are of a different nature. Although their precise nature is unknown, we may suppose that energy is concentrated in small spaces and remains so. The quanta move in such a way in our "pattern" that they can never come to a place where in this pattern there is darkness. In thus travelling from the source outward each quantum has a choice between many paths. The probability of following

different paths is proportional to the intensity of the radiation along these paths in Fresnel's radiation." [61]

The problem was how to define the motion of the individual quanta, and Lorentz discussed the possibility that the quanta might travel along Poynting's vector. Some, he suggested, might go forwards and others backwards, the net energy transfer being dtermined by the mean velocity. Unfortunately, it was not at all clear whether this would work, and since it was physically unconvincing it was not pursued, but it is indicative of the type of approach to the duality problem being discussed at the time.

Another example of the dual approach was described by Schottky[62] in 1921. Whereas in the Lorentz account the causal relationship between the quanta of energy and the guiding radiation field was not discussed, Schottky considered the possibility that the quantum transitions might be directly determined by absorption and emission intensities of the classical type of field. This idea also seems to have stemmed from Einstein; he had rejected it, however, on the grounds[63] that it would involve inexact (statistical) energy conservation, which could eventually produce arbitrarily large velocities out of nothing, and Schottky followed his example.

De Broglie

Einstein's attempts to formulate a dual theory were unsuccessful, but they provide the background for a theory that had enormous repercussions. Largely stimulated by Einstein's earlier work, Louis de Broglie started in 1921 to look for a dual theory himself, and by 1923 he had found one that was to form the basis of much of the new quantum mechanics.

In 1921, in his first paper on quantum theory,[64] de Broglie explicitly adopted the light-quantum concept, explaining that experiments on X-rays had convinced him that both absorption and emission were discrete processes. As we have seen, these experiments did not convince the majority of physicists, but de Broglie was in a special position. In 1921, he was still only a student, but he must already have been familiar with the light-quantum concept for about ten years, his brother Maurice having been secretary to the first Solvay Congress.[65] Moreover, many of the X-ray experiments referred to had been conducted by Maurice in his private laboratory. Thus de Broglie had been brought up, physically, with the experimental evidence in support of the light-quantum concept, and his intellectual influences tended the same way. As a Frenchman, he may well have been influenced by Poincaré's arguments, but more important was the influence of Einstein, for whom he had a tremendous admiration. His methodology seems to have been close to that of Einstein, and he placed a lot of weight upon the latter's work.[66]

Thus de Broglie was in every way open to the arguments supporting the light-quantum concept, and it was natural for him to adopt it. Once he had done so, two personal characteristics came into play: in the first place, his approach was freely speculative, a characteristic that had been typical of the family for centuries,[67] and even naive, while in the second place it was also highly rational and ordered. The second characteristic has not been recognised by historians, but it is important, for de Broglie's famous thesis was not an isolated speculation, but the end result of a logical and structured analysis, commenced in 1921.

De Broglie's[68] starting point was the problem, which had already troubled Bohr and Darwin, of how to define the frequency of a particle. The relationship $E=h\nu$, which featured in the title of his brother's paper to the 1921 Solvay Congress, was the fundamental relationship of the light-quantum conception, and when de Broglie decided to analyse this conception he saw immediately, in the relationship, the frequency problem. Seeking a solution, he presumably examined the known quantum phenomena. Most of these, including the photoelectric and ionisation effects, were of little use since the light, once absorbed, effectively disappeared; but in one phenomenon the light seemed to survive, and this formed the basis of de Broglie's analysis. Discussed in Einstein's 1905 paper and in Maurice de Broglie's 1921 paper, it was the effect described by Stokes' law, in which, to quote Einstein, "monochromatic light ... is changed by photoluminescence to light of a different frequency".[69] The resultant frequency was always less than the original, which was difficult to understand classically, but natural in terms of light-quanta, for no more energy could be emitted than had been absorbed.

Considering this effect, de Broglie commenced his investigation of particle frequency[70] with the information that light, affected by matter, tended to pass from a higher to a lower frequency. This had been known for years, but it gained a new significance in the light of de Broglie's investigation. Searching for a clue to the significance of the frequency of a light-quantum, he made the analogy, obvious in retrospect but apparently original, with the second law of thermodynamics. Frequency, he suggested, was to light as temperature was to matter. Unfortunately, the concept of the temperature of a single material particle raised just as many problems of definition as did that of the frequency of a light-quantum, and although this confirmed the strength of the analogy (which, 56 years later, is still at the centre of de Broglie's ideas) it rendered it quite useless.

His study of the purely particulate phenomena of light having led nowhere, de Broglie turned to those phenomena in which the dual nature was brought out. The most important of these was clearly Planck's law, and since no derivation of this had yet revealed its precise structural significance it offered scope for investigation. The derivations to date had tended to start with the wave viewpoint, but since de Broglie's overall programme was still an investigation of the light-quantum concept he naturally started with this concept, introducing wave properties only when he had to, and looking on these as a guide to the nature of the light-quantum and to the interpretation of its frequency.

The first stage of de Broglie's investigation of Planck's law,[71] published in 1922, was to study the consequences of taking the light-quantum concept without any wave properties, and this resulted in a derivation of Wien's law. This derivation was in itself quite important, for that by Krutkow had not been very satisfactory, and it was also interesting, for it introduced implicitly the first of de Broglie's important innovations, namely that the light-quanta were to be treated as small, fast, <u>material</u> particles. It is not clear what led him to adopt this conception, which was not made fully explicit until 1923, but he argued then that,[72] since for a very small rest mass m_0 the observed frequency ν and (large) velocity v were realted as $h\nu = \frac{m_0 c^2}{\sqrt{1-v^2/c^2}}$ or $\frac{v}{c} \sim 1 - \frac{1}{2}\frac{m_0^2 c^4}{h^2 \nu^2}$, the requirements that m_0 be fixed for all particles and experimentally undetectable and that v (variable) be experimentally indistinguishable from c allowed a wide range of frequencies, compatible with those observed for light. This argument was ingenious, and it allowed him to sharpen the wave-particle issue by considering light-quanta as identical particles, differing only in their velocities and in complete analogy with other particles such as electrons. In the usual conception, the frequency appeared as an internal property of the light-quanta, so that these were neither identical nor, being massless, particles in the usual sense of the word. It could be that a desire to sharpen the issue in this way was behind de Broglie's 1922 paper also; he referred to the light-quanta as having speeds close to (rather than equal to) c and, since the background to his work was the study of X-rays, Bragg's concept of material X-ray corpuscles must have been familiar to him. It is possible, however, that the innovation was ingenuous as well as ingenious, for the above arguments were not related in the 1922 paper, where he argued simply that no derivation of Planck's law had taken into account relativity theory, and that this seemed to him a serious omission considering that the subject matter moved close to the speed of light. It is not of course a serious omission for the usual conceptions, in which there is no velocity variation to be observed, but naively the argument seems reasonable, and it may have contributed to, rather than resulting from, the innovation.

De Broglie's 1922 derivation of Wien's law was, in accordance with his assumptions, a straightforward exercise in relativistic particle mechanics. He calculated (see appendix G) the distribution of relativistic particles in phase space, and then took the material limit, which gave Maxwell's velocity distribution law, and the light-quantum limit, which gave Wien's law,

$$\rho \propto \nu^2 e^{-h\nu/kT}.$$

Having established the consequences of a purely particulate light concept, de Broglie next asked what modification was required to get Planck's law. He knew that the particles were somehow non-independent, but he sought a more specific characterisation, and concluded from a comparison of Wien's law with Planck's that one could obtain the latter from the former by considering 'molecules' of light, composed of several quanta: in place of $e^{-h\nu/kT}$, one needed $\{e^{-h\nu/kT} + 2e^{-2h\nu/kT} + 3e^{-3h\nu/kT} + \dots\}$. Making this substitution, and developing a method sketched by Planck the previous year for obtaining the overall constant from statistical considerations without recourse to the classical theory, he easily derived Planck's law.

De Broglie recognised that his 'molecular' hypothesis represented little conceptual advance and, pursuing his systematic investigation of the light-quantum concept, he turned in 1922[73] to the phenomenon that followed Planck's law both logically and historically. This was the phenomenon described by the Einstein fluctuation formula, which comprised two terms, one obtainable from the light-quantum hypothesis in the limit of large numbers, and the other from the classical wave theory. From de Broglie's perspective, the latter term had somehow to be obtained by a modification of the former hypothesis. He argued that

> "One must, without doubt, compromise between the classical theory and the new one, by introducing to the latter the notion of periodicity.",[74]

but he could still do no better than to hypothesise 'molecules' of quanta, exactly as in the derivation of Planck's law.

Thus, by the end of 1922, de Broglie's investigation had still not produced any tangible results. It had, however, led him to a clear visualisation of what was required, namely the introduction to the light-quantum theory of the "notion of periodicity", and from this visualisation developed his famous thesis. The main results were published[75] in Comptes Rendues in the autumn of 1923, with a summary in the Philosophical Magazine early in 1924, and these made an immediate impact while the full Thèse,[76] appearing later in 1924, revealed the development of the ideas.

The foundation of these ideas was contained in the formulae, given in the Thèse, $h\nu = E = mc^2$

Having failed to find the key to the significance of frequency in the observed quantum phenomena, de Broglie presumably returned to his starting point, $E=h\nu$. This was generally regarded as a definition of energy, but from de Broglie's point of view it was frequency that was defined, and this led him to the formulae above. Next, following up his conception of light-quanta as relativistic particles, he considered these formulae from the point of view of an observer, relative to whom a light-quantum moved with velocity $v = \beta c$; assuming the formulae to hold in the rest frame of the light-quantum, he transformed them tothat of the observer: $h\nu_0 = m_0c^2 \longrightarrow h\nu_0\sqrt{1-\beta^2} = m_0c^2/\sqrt{1-\beta^2}$.

This was clearly a contradiction. With the usual conception of light-quanta, in which they had zero rest mass and absolute velocity c, it would not have arisen, but it was a natural result of de Broglie's conception, requiring no insight beyond that of his 1922 papers: indeed he wrote of it as "a difficulty which had long intrigued me".[77] The solution to the difficulty had not, apparently, struck him before 1923, but then, searching for a "notion of periodicity", he realised that because of the contradiction this could not stem from the frequency as it stood (i.e. from $\nu_1 = \nu_0\sqrt{1-\beta^2}$). There must therefore be another 'frequency', he deduced, defined in terms of the mass of the light-quantum, or, in his conception, of any particle:

$$\nu = \frac{\nu_0}{\sqrt{1-\beta^2}} = \frac{m_0}{\sqrt{1-\beta^2}} \cdot \frac{c^2}{h}.$$

Having deduced the existence of this second frequency, de Broglie proceeded to investigate it, and found that it could be consistently represented by a wave motion, in phase with the internal phenomenon defined by the original frequency and in the same direction as the particle, but with speed $V=c/\beta$. It was not, he found, an ordinary physical wave, but rather a phase wave, and this prompted him to reexpress the result in terms of a wave group. For a group of phase waves with similar frequencies ($\sim \nu = \frac{1}{h} m_0c^2/\sqrt{1-\beta^2}$) and similar phase velocities ($\sim V = c/\beta$), he found that the group velocity, given by U: $\frac{1}{U} = d(\nu/V)/d\nu$, was in fact equal to the particle velocity $c\beta$. He took this result as confirmation of his hypothesis, and, from the fact that the group velocity was generally interpreted as the velocity of energy propagation, he seems to have derived his interpretation of interference phenomena. Clearly interference had to occur between the phase waves, but these, he deduced, carried no energy, a result he justified by the fact that they moved with velocity greater than c. He was thus led to a suggestion similar, as he realised, to that of Campbell in 1913, namely that

> "The probability of reactions between atoms of matter and atoms of light is at each point bound to the resultant (or rather its mean value) of one of the vectors characterising the phase wave: where the resultant is nil, the light is undetectable; there is interference. One conceives then that an atom of light crossing a region where the phase waves interfere could be absorbed by the matter at certain points and not at others." [78]

The above quotation is from the Thèse: in the earlier papers, he gave only an indication of how interference might be accounted for, writing that

> "When a phase wave crosses an excited atom, this atom has a certain probability of emitting a light-quantum determined at each instant by the intensity of the wave.", [79]

and in other ways too these papers were less revealing than the Thèse.

De Broglie did not, in the papers, give the origin of his phase wave conception, but instead simply claimed that every particle, be it of light or gross matter, was the seat of an internal phenomenon such that

> "For a fixed observer, [the phenomenon] has at each point of space the same phase as a wave spreading in the same direction" [80]

with velocity c/β , where $c\beta$ was the velocity of the particle. He showed that

> "The rays of the phase wave are identical with the paths which are dynamically possible", [81]

and that

> "If, at the beginning, the internal phenomenon of the moving body is in phase with the wave, the harmony of phase will always persist", [82]

a result he "expressed in another way" as the identity of group and particle velocities. From these results, he deduced that his hypothesis was possible, and he went on to apply it in two important cases, giving a new derivation of Planck's law (for which see appendix G) and an interpretation of the Bohr quantisation conditions for the atom.

The original purpose of the Bohr atomic model had been to impose stability upon the electron orbits of the Rutherford atom, using the quantisation conditions; but this stability had never been explained physically, or to the satisfaction of many physicists who considered the orbits in classical terms. De Broglie, analysing the motion in the atom of both the electrons and their associated phase waves, found that

> "The motion can only be stable if the phase wave is tuned with the length of the path." [83]

The phase wave of an electron orbiting around a nucleus would, naturally, run into itself, and stability could only be achieved if it was in phase with itself when it did so. This condition gave a series of possible orbits whose path lengths, l , were multiples of the phase wavelegth, λ , in complete agreement with the Bohr model. The stability condition was $l = n\lambda$, or

$n = \frac{l}{\lambda} = \int \frac{\nu}{V}\,dl$, and this was equivalent to the quantum conditions,

$$nh = \int \frac{h\nu}{V}\,dl = \int \frac{h m_0 c^2 \, dl}{h\sqrt{1-\beta^2}\,(c/\beta)} = \int m_0 \beta c \, dl/\sqrt{1-\beta^2} = \int p\,dq ,$$

in the usual notation.

This was the most important result obtained by de Broglie, and it would no doubt have convinced people that he was right, had not his hypotheses been so unacceptable: light-quanta, disturbances propagated faster than the speed of light, and matter waves.

De Broglie's hypotheses raised the whole problem of the relationships between waves and particles, on the one hand, and between light and matter on the other. Concerning the former, his conceptions clearly involved a much closer link than had generally been accepted, and he realised this, writing in the Thèse that

> "The history of optical theories shows that for a long time scientific thought has wavered between dynamical and wave conceptions of light; however, these two representations are without doubt less in opposition than has been supposed and the development of the theory of quanta seems to confirm this conclusion." [84]

One important aspect of this connection arose in his analysis of the Bohr orbits, for the stability condition, $\int \frac{1}{\lambda}\, dl = \int \frac{\nu}{V}\, dl = \text{constant}$, was none other than Fermat's principle, while the equivalent quantisation condition, $\int p\, dq = \text{constant}$, was Maupertuis' principle of least action. Schrödinger pointed out later that the connection between the two was well-known to Hamilton as his optical-mechanical analogy, but in private Hamilton had in fact gone beyond mere analogy,[85] and de Broglie now did so in public: according to his conceptions, Fermat's principle, the fundamental law of wave optics, and Maupertuis' principle, the fundamental law of particle dynamics, were absolutely equivalent.

This result also bore on the relationship between light and matter, which were essentially identical according to de Broglie's conception, and which had traditionally been treated as wave and particle forms respectively. Applying a historical perspective to the relationship, de Broglie came to an interesting overall perspective:

> "Our dynamics (in its Einsteinian form) has remained behind optics: it is still at the stage of geometrical optics. If it appears to us today likely enough that all waves carry a concentration of energy, on the contrary the dynamics of a material point conceals without doubt a propagation of waves, and the true meaning of the principle of least action is to express an agreement in phase."[86]

De Broglie had commenced his analysis by assuming a particle structure for both light and matter, but the phase wave concept he derived proved so successful that he ended up advocating a wave structure for both; if we illustrate his perspective on the light-matter-wave-particle relationships with a figure, we can see how:

	PARTICLE		WAVE
LIGHT	Geometric optics	$\xrightarrow{\text{superceded by}}$	Wave optics
MATTER	Particle mechanics		

He himself had already gone beyond this perspective, by combining the wave and particle aspects of light, but the prospect of completing it in the obvious way, by postulating a wave theory of matter to supercede the particle mechanics, seems to have proven irresistible. In his Thèse, he already sought to minimise the particle aspects, and later in 1924 he went so far as to write that

> "The whole theory will only become really clear when one succeeds in defining the structure of the light wave and the nature of the singularity constituted by the quantum of which the movement must be predicted by obtaining the situation uniquely from the wave point of view."[87]

Thus it was that de Broglie's theory of light-quanta ended up as one of matter waves. His attempt to persuade Dauvilliers to conduct some electron diffraction experiments was unsuccessful,[88] so he had no direct evidence to support his conclusions, and he had no clear explanation either of why light showed interference so much more readily than matter, though he suggested that it might be connected with the proximity of the particle and wave speeds in this case ($\beta c \sim c/\beta \sim c$). But for all its problems, and all its highly unpopular features, the theory was bold and imaginative, and it had sufficient appeal, as we shall see, to act as the progenitor of both the wave and matrix versions of quantum mechanics.

II.3. THE CONCEPTUAL CRISIS: (2) RADICAL CONCEPTS AND THE IDEAS OF BOHR.[89]

At the beginning of this chapter, we outlined three major sources of the revival of general interest in the quantum problem. Of these, (general) relativity considerations seem to have prompted Einstein's work, and a combination of experimental and (special) relativity considerations de Broglie's. The third source was the development of Bohr's thought, and this acted as a fulcrum for a set of suggested changes to physics, far more radical than anything in Einstein's or de Broglie's theories. De Broglie's conceptions were novel, but they were essentially structural, and as such were irrelevant so far as many physicists were concerned. The radical suggestions put forward by Bohr and others, on the other hand, affected two fundamental laws of physics: that of energy conservation and, more basic still, that of strict causality.

In 1919, Darwin prepared a draft manuscript,[90] originally intended for eventual publication, on the contradictions within quantum theory, contradictions that he thought were emphasised by each triumph the theory had. He also sent a note[91] based on this manuscript to Bohr for his comments, and an examination of both draft and note gives the distinct impression that the whole (there were many corrections to the draft) was very much aimed at getting Bohr's approval, and represented as much what Darwin thought Bohr might like as what he liked himself. Darwin's idea was supposedly to look at the proofs that brought out most clearly the contradictions in the theory, and knock away the assumptions one by one. He forecast the possible conclusions in one sentence that will act as an ideal basis for our discussion:

> "It may be that it will prove necessary to make fundamental changes in our ideas of space and time, or to abandon the conservation of matter and electricity, or even in the last resort to endow electrons with free will."[92]

Energy conservation

Three possibilities, but when it came down to it Darwin was only concerned with a variant on the middle one, namely the abandonment of exact energy conservation, and rather than deducing it from his analysis he seems quite clearly to have set out with it as a conscious target. Examining the proofs by Jeans and Poincaré of the necessity of the light-quantum concept, he 'found' that both rested on the unjustified assumption of energy conservation: indicating a strong preference for the classical theory, he noted the problems of interference and frequency definition, and also threw in the artificiality of Planck's 1911 theory as sure grounds for suspecting the simpler solution of energy non-conservation. He thus concluded that exact conservation should be replaced by statistical conservation (or even by systematic energy changes, for certain classes of phenomena), which, helped by the principle of least action, would still hopefully allow a definition of the most probable state of a system.

The letter to Bohr was considerably less extreme than the manuscript (in its uncorrected form, at least), but the importance of energy non-conservation, for which the photoelectric effect was adduced as the main evidence, the light-quantum concept being rejected on the usual grounds, was still made quite clear. Darwin considered the "case against energy conservation quite overwhelming",[93] and in 1922 he expressed this view in print,[94] when he associated it with a defence of the wave theory of light as a consistent and accurate whole.

In apparent reply to Darwin, Bohr agreed that "on the quantum theory, conservation of energy seems to be quite out of the question",[95] and expressed his own feeling that something funny must go on in the atom, triggered somehow by the incident light. In a paper presented at the 1921 Solvay Congress, he referred to the light-quantum concept as seeming

"to offer the only possibility of accounting for the photoelectric effect, if we stick to the unrestricted application of the ideas of energy and momentum conservation ", [96]

and this appears to have been his first public reference to the possibility of abandoning energy conservation as an alternative to accepting the light-quantum concept. The same year, he also prepared a manuscript that did not reach publication, in which he repeated the above statement verbatim, but followed it up with the comment that

"At this state of things it would appear, that the interesting arguments brought forward more recently by Einstein [i.e. those of 1916] ... rather than supporting the theory of light-quanta will seem to bring the legitimacy of a direct application of the theorem of conservation of energy and momentum to the radiation processes into doubt." [97]

Here it can be seen that Bohr was not so much pitting non-conservation and light-quanta against each other as clearly supporting the former, and in a 1923 survey paper he wrote that

"A general description of the phenomena, in which the laws of the conservation of energy and momentum retain in detail their validity in their classical formulation, cannot be carried through." [98]

This unequivocal statement was in tune with the description of Bohr's ideas that Ehrenfest had sent to Einstein the previous year, writing that

"He is much more willing to give up the energy and momentum theorems (in their classical form) for elementary atomic processes, and to maintain them only statistically, than to 'lay the blame on the aether'.", [99]

and a manuscript from 1924 makes it quite clear how strongly Bohr felt about this issue:

"However, the theory of light-quanta may be characterised as an endeavour to uphold the unlimited validity of the classical principles of the conservation of energy and momentum. On the other hand, in a description as that considered above, it is a principle feature that these principles lose their strict validity for atomic processes and appear only as statistical results of probability laws." [100]

We may recall that in 1911 Einstein had seen the situation in exactly the same terms, only the other way around. He had given strong arguments then in support of his contention that non-conservation and the light-quantum concept were the only possibilities, but if there was little acceptance of the latter before the 1920's there was even less mention of the former. Einstein had assumed that above all things energy conservation was sacred, and he was right: if continuous radiation was equally sacred, then the attitude was to avoid rather than provoke a confrontation between the two. Even Einstein's 1916 paper, which leads very easily, as we have seen, to ideas of energy non-conservation, does not seem to have provoked any response in this respect, except from Einstein himself, who was fully aware of the problems, and from Schottky, later, who seems to have had peculiar reasons of his own.

It was only in 1922, after Bohr had relaunched the problem, that physicists seem to have come round to considering energy non-conservation as the necessary lesser of two evils. Heisenberg recalled[101] that with the exception of Sommerfeld the physicists in both Munich and Göttingen were prepared, after Einstein's paper, to consider the possibility of statistical energy conservation, but there is no evidence that this preparedness went any further than a shrug of the shoulders. In 1921, however, Sommerfeld considered energy conservation,[102] and in 1922 he concluded, in the latest edition of his famous book, that

"The mildest modification that must be applied to the wave theory is, therefore, that of disavowing the energy theorem for the single radiation phenomenon and allowing it to be valid only on the average for many processes." [103]

The same year, Einstein and Ehrenfest were forced to consider the possibility of energy non-conservation in the context of the Stern-Gerlach results,[104] and in 1923 Born and Heisenberg[105] were also forced to non-conservation as a provisional conclusion in their study of adiabatic field transformations in connection with the Helium atom. Thus by 1923, statistical energy conservation had become a well established, and even a provisionally accepted, idea.

Space-time description

Another of Darwin's suggestions was that of "fundamental changes in our ideas of space and time", and this too was a prominent feature in Bohr's thought. The origins of this idea were in the wave-particle duality which posed the problem of how radiation could be at the same time diffuse and localised, and it seems to have been mentioned first by Richardson, who wrote in 1916 that

> "It may be that it is impossible consistently to describe the spatial distribution of radiation in terms of 3-dimensional geometry." 106

The spatial behaviour of radiation was clearly very odd, and if one insisted, as Bohr did, on preserving the wave nature of the radiation then the quantum transitions, looked at one way as involving non-conservation, could be seen in another way as involving a strange temporal behaviour, quite analogous to the spatial. The failure of the quantum theory to provide an adequate space-time description of radiation processes was noted by Kramers, who wrote in 1922 that

> "The hope of attaining such a description must perhaps be allied to the representation of 'physical individuals' or material particles of an even lower order of magnitude than the smallest particles now known ... and to ideas of more fundamental nature than those now known; we are here outside our present sphere of experience.", 107

and Bohr himself claimed in his 1923 paper that there was a generally held view that

> "A description of atomic processes in terms of space and time cannot be carried through in a manner free from contradiction by the use of conceptions borrowed from classical electrodynamics", 108

though, as he noted in the introduction to the paper,

> "From the present point of view of physics, however, every description of natural processes must be based on ideas which have been introduced and defined by the classical theory." [my emphasis] 109

Again, though, it is the 1924 manuscript that gives his views most clearly:

> "It is more probable that the chasm appearing between these so different conceptions of the nature of light is an evidence of the unavoidable difficulties of giving a detailed description of atomic processes without departing essentially from the causal description in space and time that is characteristic of the classical mechanical description of nature." 110

Causality

The word "causal" in the above quotation brings us to Darwin's third possibility, that of endowing electrons with free will. This is a large and complicated subject, but it is one that must be treated thoroughly, for it is absolutely basic to the change-over from the classical to the quantum ideals of physics. In the wake of Forman's work on the subject,[111] the whole question of causality in quantum theory, and indeed in physics as a whole, is in urgent need of review, and in appendix H I shall summarise my conclusions on the question and my reasons for disagreeing in many cases with Forman; but for the present I shall confine myself to the causality problem as it affected physicists actively engaged upon quantum theoretical research, before the advent of quantum mechanics.

Recalling the three alternatives offered by Darwin, namely

(1) Non-conservation,
(2) Absence of a space-time description,
and (3) Free will,

we must first establish that although these are clearly interrelated they are not identical. There is a tendency among writers on this subject to define carefully what is meant by 'acausality', and then to assume that everything not in accordance with the definition was really meant to be. This must be avoided. In the present context, 'causality' may be equated with the classical notion of 'determinism', and we shall use the words interchangeably, but a problem arises with the notion of 'acausality'. There is a distinction between the absence of a satisfactory causal treatment and the positive repudiation of the possibility of such a treatment. To avoid confusion we shall abolish the word 'acausal' altogether, referring to the latter treatment as 'anticausal', and describing the former in longhand (so to speak). A similar problem arises with the word 'chance', which can refer either to anticausality or to ignorance under present circumstances, and we shall therefore avoid this word also.

Darwin's third suggestion was clearly anticausal, free will being essentially a 'cause' of anticausality, indistinguishable from it so far as science is concerned; his first two, however, were not. Energy non-conservation is often taken to imply anticausality, but this is not strictly true. Many physicists had no qualms about a statistical entropy law as part of a deterministic physics, and although a statistical energy law would have been harder to swallow there is no a priori reason why it should not have been accepted in the same way; alternatively, energy might vary sytematically as Darwin suggested. Non-conservation and anticausality are not then identical, but, having said this, we must immediately qualify it. For a statistical energy principle would not have been possible within the causal framework without some refinement in the energy concept or in the structural conception of matter: it would be necessary for example to somehow keep track of the deviation of the energy from its statistical norm. The simplicity of the energy concept and of the conservation principle gave classical physics a large measure of its security, and many would have treated the abandonment of conservation as being as bad as a rejection of causality, and as quite probably implying such. In short, an advocate of non-conservation was not necessarily an advocate of anticausality, in his own mind, but he may well have been so in the minds of determinists: in the same way a person is often attacked for abandoning his religion when he himself claims only to differ on an inessential point of dogma.

In the present context we know that Einstein, for example, defended conservation throughout, and it appears that he did see its rejection as a virtual abandonment of causality.[112] But what of the advocates of non-conservation? It is possible that ~~Bohr~~ Born and Heisenberg, in the conclusion to their paper, Sommerfeld, in his book, and physicists in general as recalled by Heisenberg all rejected causality; but since none of them appear to have said so explicitly, despite, as we shall see, a favourable environment in which to do so, it is highly unlikely. In particular, it is very hard indeed to imagine Sommerfeld rejecting causality, and we know from a reference to Born's "causal way of looking at things",[113] and from his own recollections,[114] that he did not do so either. Even Darwin, while advocating energy non-conservation, talked of "systematic" (i.e. deterministic) energy changes, and considered free will only "in the last resort".

Clearly the causality issue can not be decided on the basis of energy conservation, and the same is true in respect of Darwin's second possibility, the absence of a space-time description. This does not amount to anticausality (though again it may have been interpreted as such by some people), so much as to the absence of a framework in which either causality or anticausality can be defined. If the causality requirement is expressed as

'Given a situation A in space-time, we can determine a later situation B',

then the absence of a space-time description involves a negation of the premise rather than of the conclusion. In 1923, Senftleben expressed the problem clearly, writing that

> "Planck's constant h limits in principle the possibility of describing a process in space and time with arbitrary accuracy.",[115]

and he also noted the implication: the conclusion of causality cannot be drawn, but neither can that of anticausality. As we shall see later, it seems to be this position that in fact characterises quantum mechanics, and not anticausality as such.

Suggestions of anticausality in physics had already arisen around the turn of the century out of attempts to understand radioactive phenomena. They were linked then with the nineteenth century philosophies of Contingency, as expounded by Renouvier and Boutroux, and Tychism, due to Peirce, both philosophies involving a rejection of determinism. Tychism was based on the absence in a deterministic physics of any possibility of growth, and drew on the failure of classical physics to provide exact laws, and on its resort to statistical ones, as in the kinetic theory. Contingency was in effect an extreme form of positivism: since one could never observe every infinitessimal stage in a causal chain, one had no justification for assuming that complete causality existed. Clearly, the phenomena of radioactivity, for which no causal mechanisms were known, gave support to both philosophies. However, as I noted in the introduction, the influence of philosophy upon physics at that time was negligible. The only thorough discussion of the problem by a physicist seems to have been that by Poincaré in 1904, when he came out in strong support of causality, and, despite the popular discussion of an abandonment of causality, and despite the strong philosophical trend in that direction, I can find only one major physicist, Exner, espousing the anticausal viewpoint.

In its first phase, the quantum theory was often linked closely with that of radioactivity, either explicitly or through the analogy between the radioactive and photoelectric phenomena, and it offered much the same scope for the introduction of anticausal ideas. But by 1919, the quantum physicists still seem to have been unaffected by the philosophers, though Planck's emphasis on the retention of causality in his 1911 theory suggests that he was aware of their existence. In the early 1920's, however, the revival of interest in the quantum problem brought with it discussions of the causality issue, and several physicists actually espoused the concept of anticausality.

The problem we meet in attempting to analyse the causality issue in quantum theory in the early 1920's is that, although the acceptance of anticausality in this period was very restricted, the pressures towards this viewpoint were numerous, varied, and apparently strong. We shall therefore start our analysis by outlining these pressures.

The purely internal pressures towards anticausality in quantum theory were themselves very strong. The discreteness essential to the theory did not necessitate an abandonment of causality, but it clearly highlighted the possibility, especially when emphasised by Poincaré and Ehrenfest, and especially when linked with Poincaré's suggestions of the abandonment of differential equations and of discrete time. Poincaré himself did not treat these as anticausal,[118] but many physicists, following Renouvier's emphasis on the link between causality and continuity, might have done so: Jammer's misreading of the situation indicates how tempting the conclusion is.[119]

A second source of internal pressure was the Bohr atomic model, especially when combined with the Einstein probability assumptions. A causal mechanism was implicit, but its absence, explicitly, was notable. Although there were probabilities in the kinetic theory, one knew whence they came (namely the microscopic motions of the moelcules), and in the context of radioactivity the probabilities were at least simple enough for a mechanism to appear feasible. In quantum theory, however, there was no prospect of a satisfactory mechanism, and this must have been particularly apparent in Einstein's 1916 paper, where he had to resort to 'chance': he intended it to mean 'causes as yet unknown', but it must have been very provocative.

As well as the above pressures, there were also some internal to physics, but not to the quantum theory, for relativity theory had also run into problems with causality. In 1919-20 there was a debate between Holst and Petzoldt as to whether or not it was possible to define a causality applicable to all frames of reference, within the special theory of relativity.[120]

From physics to philosophy : the debate between Hilbert and Brouwer, in which Hilbert defended classical logic against the suggestion that there were equally valid non-classical systems, also related to the causality problem. The main influence of this debate must have come from the suggestion that classical systems were not sacrosanct, but the philosophical school represented by Brouwer, namely that of Dutch intuitionism, was strongly opposed to determinism. This was true also of other philosophies of the period, for Peirce's ideas had been developed into the pragmatism of William James, while Danish philosophy was dominated by Kierkegaard's existentialism, German by Husserl's phenomenology (a development of existentialism), and French by Bergson's philosophy of time. All these philosophies were opposed to causality.

Finally, we should note the popular Lebensphilosophie of the German Weimar republic. This was not sympathetic to either causality or physics, drawing as it did from existentialist philosophies and classical German romanticism, and emphasised by the insecurity of Europe, and particularly Germany, in the post-war years it dominated German popular thinking.[121]

The problem of analysing the relative strengths of these pressures is a virtually impossible one. In the present context we should note that the combined pressure must have been immense, but that the resistance to it must have been equally so. Born and Einstein were both aware of both the internal and external pressures, but they continued to uphold causality, Born until 1926 and Einstein all his life.[122] Lorentz too defended causality in this period,[123] and Tetrode put forward a theory in 1922 that seems to have been a reaction to what he saw as anticausality inherent in quantum theory. To remove this, he introduced predetermination, writing of the quantum theory without this that

> "The recent development of natural science has led to ... causality partly conditioned by chance." and

> "According to the earlier view the emission of light-quanta, e.g. by a Hydrogen atom, is determined by chance."

Considering the force of the pressures towards anticausality, that doctrine had very few adherents indeed, but there were some. Thus in 1921 Schottky[125] proposed a theory combining anticausality with advanced action at a distance, chains of events being seen as indivisible threads such that each end was conditioned by the other, but such that the threads themselves were not causally arranged. This was an anticausal development of Einstein's dual theory in which the 'ghost' field was allowed to determine the quantum motions <u>only</u> probabilistically, and it seems to have been prompted by the external pressures linked with popular philosophy. In 1922, influenced by Exner's archaic ideas, Schrödinger launched an appeal for the "liberation from the rooted prejudice of absolute causality".[126] Finally, in 1923, Senftleben[127] noted that whereas in the past one used to regard natural phenomena as arising statistically from microscopic causal changes, one now had to describe these changes themselves statistically. He did not see any grounds for actively espousing anticausality, but he could find no grounds either for continuing to believe in causality.

By 1923, anticausal ideas had penetrated quantum theory, but only to a limited extent. Neither Schottky nor Senftleben had any real influence, and Schrödinger later reverted to a belief in causality, so that although the ideas were around they still represented a minority viewpoint. In 1924, however, they were introduced to the mainstream of quantum theory by Bohr,[128] and we shall therefore close this section by looking at Bohr's views on the causality issue in the period leading up to this introduction.

Bohr had a deep interest in philosophy, and was very familiar with the work of Høffding and, through him, James and Kierkegaard. Moreover, he was clearly aware of the internal pressures against causality as well. However, until the end of 1923 or the beginning of 1924, he did not adopt anticausality as such. He frequently referred to the role of chance and probability in quantum theory, but he invariably added the proviso "in the present state of the theory". In 1922, he wrote that

> "In the present state of the theory ... [transitions are] considered to be a question of probability", [129]

while in his 1923 survey he made a double proviso:

> "In the present state of the theory, it is not possible to bring the occurence of radiative processes, nor the choice between various possible transitions, into direct relation with any action which finds a place in our description of phenomena, as developed up to the present time.", [130]

and, to make quite sure that chance should not rule, he refered to the "unknown mechanism which is answerable for the emission of radiation". Even in the Bohr-Kramers-Slater paper, which did seem, in 1924, to abandon causality, the caution continued:

> "At the present state of science it seem necessary, as regards the occurence of transition processes, to content ourselves with considerations of probability." [131]

This may be seen as a justification leading up to the explicit abandonment of causality that occurred, as we shall see, later in the paper, but the fact that Bohr found it necessary suggests that his caution may well hide a personal rejection of causality much earlier than the printed one. It has been suggested[132] that his use of the word 'spontaneous', in 1918,[133] was meant to imply anticausality, but he explained in 1923 that

> "This occurs spontaneously: that is, without any <u>assignable</u> <u>external</u> stimulation" [my emphasis], [134]

precisely, I should suggest, as in the classical theory, and there are no other indications of explicit anticausality before the end of 1923. In a manuscript of 1923/4, however, which may have been written before or after the Bohr-Kramers-Slater paper, he wrote, without the usual proviso, that

> "Every change of the atom is to be regarded as contingent on probability laws", [135]

and extended his reference to the absence of a space-time description to the absence of a causal space-time description. By this time Bohr had thus accepted anticausality as well as non-conservation and the absence of a space-time description. Why he had done so, it is impossible to say exactly; the internal influences seem to provide a sufficient explanation but, as with Bohr's other major innovations, the external factors are too strong to be ignored.

II.4. THE CONCEPTUAL CRISIS:(3) THE OLD QUANTUM THEORY[136]

The most important manifestation of the conceptual crisis in quantum theory in the early 1920's was in the failure of the old quantum theory. Most of the work within this theory was devoted to generalising the Bohr atomic model to more complex atoms, and to atoms in the presence of electric and magnetic fields. It was dominated by Sommerfeld and his "Institute for number mysticism"[137] in Munich, and by Landé, and it was not in retrospect very successful; but the initial success with the hydrogen atom produced an aura that lasted for about ten years and dominated the lack of success in other areas.

In the early 1920's, however, it became apparent that the combination of classical physics, quantisation conditions, and the adiabatic and correspondence principles, was not going to be sufficient to 'save the appearances'. The main problems arose in the related fields of the complex structure of higher elements and the anomalous Zeeman effect exhibited by these elements.[138] One did not in fact have to go very high up the periodic table to run into trouble, and much of the investigation centered around the helium atom: even here, attempts to account for its structure within the framework of the old quantum theory continually failed. Langmuir,[139] Epstein[140] and Van Vleck,[141] in America, and Kramers[142] in Denmark, all encountered difficulties with helium in 1921-2, and word of these soon reached Germany.[143] In Germany, meanwhile, Born embarked in 1921 upon a programme of pushing the theory as far as it would go, in an attempt to find out its limitations and shed light upon the modifications that were needed.

Born was the ideal man for this investigation; recalled by Heisenberg[144] as a 'mathematical methods man', he was more interested in the existence or otherwise of solutions than in the solutions themselves, and he was as happy with negative as with positive results. This was as well for as Pauli, who was his first assistant in the programme, remarked,

> "The effort expended does not correspond to the results achieved, especially as these results are chiefly negative." [145]

Born's chief concerns to date had been relativity theory and the structure of crystals,[146] but in 1921 he was given a chair in theoretical physics at Göttingen, where Hilbert presided over the mathematics department that had been raised up by Felix Klein, and where Franck had arrived the previous year to head the experimental physics department. The unique set up at Göttingen, where these three departments were uncommonly close, was to play a major role in the development of quantum mechanics. On arrival, Born was anxious to establish a close connection with Franck's department, and it seems to have been this that led to his involvement in quantum theory. Franck's experiments with Hertz[147] between 1913 and 1920 had provided the most direct evidence of support of the Bohr model, and Franck was an ardent admirer of Bohr. He was also in close communication with Bohr, and knew the limitations as well as the achievements of the quantum theory; when Born came along, he persuaded him to subject it to the theoretical investigation it needed.[148]

Of the techniques suitable for such a task, that with which Born was most familiar was perturbation theory, and this formed the basis for his work with Pauli in 1921.[149] This work led Born to the conclusion, noted before, that the quanta were a "hopeless mess",[150] and in 1922 he continued his investigation with Heisenberg, like Pauli one of Sommerfeld's pupils.

Born and Heisenberg wrote two papers on the quantum theory, both published in 1923. In the first,[151] they ran into troubles with crossed fields, analogous to those shown up by the experimental work of Stern and Gerlach in 1922.[152] The Stern-Gerlach results showing space quantisation in a magnetic field provided splendid experimental confirmation of the quantum theory, but both theory and experiment presented a conceptual problem, for a change in field specification led immediately to a change in the quantum state of the atom. This discrete change in atomic state would seem to require a discrete amount of energy, but the continuous field change could not produce such an amount immediately. Einstein and Ehrenfest commented in 1922 on this aspect of the results,[153] and noted the explanations that might possibly account for it, including energy non-conservation: this is the conclusion to which Born and Heisenberg were also forced.

In the second paper,[154] in which the assistance of Pauli was acknowledged, they subjected the helium atom to a thorough investigation, and their conclusions were quite clear:

> "We have now set ourselves the problem of examining all possible orbital types in excited helium atoms, of selecting the quantum theoretically permissible solutions, and calculating the energy values, so as to establish whether or not orbits are present which give the empirical terms correctly. The result of our investigation is negative: one reaches through the consequent application of the known quantum rules no explanation of the helium spectrum." [155]

These conclusions did not go completely unchallenged,[156] but in general the paper was taken as decisive proof that the old quantum theory failed for the helium atom.[157]

Meanwhile, Pauli had been doing some work of his own on the helium ion H_1^+, and his results,[158] published in 1922, gave another example of how the quantum theory went wrong. The theory gave, according to Pauli, a stable state of very high energy, and another state that had lower energy corresponding to the empirical value, but that was not theoretically stable.

The only success during this period with the problems of complex spectra and the anomalous Zeeman effect was achieved with the magnetic core, or rump, model of the atom, which was developed by Heisenberg, Landé and Sommerfeld, and pursued principally by Landé and Heisenberg.[159] It was a glaring feature of this success, however, that it necessitated hypotheses quite contrary to the old quantum theory. To obtain the correct atomic energy in a magnetic field, the core of the atom (atom minus valence electron) had to be counted twice in its contribution to angular momentum and magnetic moment (g-factor) , and to obtain the observed Zeeman splitting, half-integral quantum numbers had to be introduced for some components, while the choice of selection rules (giving the possible transitions) appeared to be quite inconsistent, even arbitrary.[160]

The half-integral quantum numbers were seen as the biggest problem. They had been introduced by Heisenberg early in 1922, and Bohr had quickly seen that

> "The entire method of quantisation (half-integral quantum numbers and the rest) appears not to be reconcileable with the fundamental principles of the quantum theory." [161]

Naturally, as author of the principles under threat, Bohr did his best to evade the half-integral numbers, but to no avail. Concurrent with the work by Born and Heisenberg, which can be seen in one respect as an attempt to get the correct terms without half-integrals (i.e. by sticking rigorously to the old quantum theory), Bohr and Pauli pursued this same path explicitly. The results were never published, but Bohr showed the work to Landé, and wrote that

> "It was, as you saw, a desperate attempt to stick with integral quantum numbers, because we hoped to see, even in the paradoxes themselves, a hint of the paths upon which one might seek the solution of the anomalous Zeeman effect." [162]

Heisenberg had been hesitant about accepting the inevitability of his own hypothesis, but in November 1922 he had written, also to Landé, that

> "I myself, as much so now as Prof. Sommerfeld, am almost convinced that the half quantum numbers, against Bohr's opinion, are right", [163]

and here the American work played a part, for he wrote that

> "Sommerfeld writes to me from America that an American mathematician Van Vleck has found the values of 22V for the ionisation potential according to Bohr model to be experimentally false." [164]

Half-integral quantum numbers, however, gave a value to within the experimental error, and Heisenberg concluded that "The Bohr model must also be false." [165]

The resistance by Bohr and others to half-integral quantum numbers was strong, but in the spring of 1924 first Hund[166] and then Born and Heisenberg[167] proved to their satisfaction that they were necessary, while in February 1924, Pauli had written to Bohr that

> "The atomic physicists in Germany today fall into two groups. The one calculate a given problem firstly with half-integral values of the quantum numbers, and if it doesn't agree with experiment they then do it with integral quantum numbers. The others calculate first with whole numbers, and if it doesn't agree then they calculate with halves. But both groups of atomic physicists have the property in common, that their theories offer no a priori reasoning, which quantum numbers and which atoms should be calculated with half-integral values of the quantum numbers, and which should be calculated with integral values. Instead they decide this merely a posteori by comparison with experiment. I myself have no taste for this sort of theoretical physics and retire from it to my heat conduction of solid bodies." [168]

Another major problem was with the theory of the fine structure of X-ray doublets, which had been based on relativistic corrections to the motion of the electron (the "relativistic effect").[169] It became apparent in 1923 that this was not sufficient to explain the observations, and when Landé, in search of a solution, emphasised the formal analogy between this problem and that of complex spectra[170] the quantum mess got even messier. It was realised that the relativistic and core theories really applied to the same cases, but although each was inadequate on its own, they produced together a joint correction that was far too large. The battle between the two theories did not really begin until 1924, but by late 1923 the problem was already apparent, adding to that of the half-integral quantum numbers.

Finally there was the problem of the statistical weights. Using the range of quantum numbers then available (or rather the mechanical properties of the atom with which they were linked), the theory was persistently giving the wrong stationary states. The trouble was that the theory as it stood gave the (empirically) wrong number of degrees of freedom for the parts of the atom. In terms of the core model, it appeared to give either one too many or one too few degrees of freedom to the core, and one to many to the valence electron. This led Bohr[171] as early as 1920 to introduce a "Zwang" (constraint) regulating the possible choice of orbits, despite the fact that this Zwang was simply not explicable in terms of the mechanical model of the atom. Landé[172] referred in 1922 to a "non-mechanical adjustment of the rump", and Bohr summed up the situation in a manuscript prepared with Pauli in 1923: [173]

> "In the electron assemblage in an atom, we have to do with a coupling mechanism which does not permit a direct application of the quantum theory of mechanical periodic systems; in particular, there can obviously be no question of accounting for the complex structure in terms of the exclusion, based on the consideration of an adiabatic transformation, of certain motions, compatible with this theory, as stationary states of the atom. Rather, we are led to the view that the interplay between series electron and atomic core, at least as far as the relative orientation of the orbit of the series electron and those of the core electrons is concerned, conceals a "Zwang" that cannot be described by our mechanical concepts and has the effect that the stationary states of the atom, in essential respects, cannot be compared with those of a mechanical periodic system."

Bohr, we may recall, had never worried about deviations from classical mechanics, and he naturally hoped that the introduction of an unmechanical constraint might also help the theory out of some of its other difficulties by providing the modification to mechanics that was needed; he wrote that

> "According to our view it is just this constraint that finds it expression in the regularity of the anomalous Zeeman effect, and, in particular, is responsible for the failure of the Larmor theorem.", [174]

but he could go no further than this rather vague statement.

Bohr was sufficiently perturbed by all this to put off repeatedly the publication of this manuscript,[175] which was a second part to the survey paper that had appeared in 1923.[176] After the "desperate attempt to stick with integral quantum numbers", Pauli wrote to Bohr in July 1923 that

> "Perhaps in the course of the summer you may after all get a saving idea about complex structure and the anomalous Zeeman effect", [177]

but it was not to be. In December, Bohr received a long letter from Heisenberg[178] containing a new approach to the anomalous Zeeman effect, and decided to put off the paper until Heisenberg could come to Copenhagen in 1924. By then events had overtaken him.

Heisenberg's new approach to the anomalous Zeeman effect problem[179] was to seek a general formula for the g-factor, and this led him to a new quantum principle,

[H Hamiltonian, J action variable]

$$H_{quantum} = F(J+\tfrac{1}{2}) - F(J-\tfrac{1}{2}) = \int_{-1/2}^{1/2} H_{classical}\, dJ$$

of which he had high hopes. In February, he wrote to Bohr that he saw " a hope of getting the half quantum numbers out of the $\int H\, dJ$ formalism".[180] Landé was also optimistic, but Pauli was critical, and wrote to Landé that [181]

> "I don't share your opinion at all about Heisenberg's new theory. I even think its ugly. For despite radical assumptions it yields no explanation of the half quantum numbers and the failure of the Larmor theorem (especially the magnetic anomaly). I don't think much of the whole thing."

The last sentence sums things up. It was written in December 1923, and in the summer of that year Paschen[182] and Landé[183] had already accepted the failure of the old quantum theory in its present form, while Pauli himself had declared that the quantum theory supplied "no sufficient grounding" for the treatment of complex spectra, and that something "in principle new" was needed for the anomalous Zeeman effect. The old quantum theory had failed:

> "This failure can scarcely be doubted any longer, and it seems to me to be one of the most important results of the last few years that the difficulties with many body problems lie in the physical atom, not in the mathematical treatment (when e.g., the helium term comes out wrong in Born and Heisenberg, this certainly does not lie in the fact that the approximation is insufficient)." [184]

As we have seen, Bohr was also troubled, and Heisenberg, although temporarily optimistic about his new theory, had long recognised the disastrous state of the old. Early in 1922, he had been aware that he was only playing games with it, and had written to Pauli that he enclosed

> "My Zeeman roast with quantum sauce, without hereby pledging myself to the goal of convincing you of my point of view." [185]

This reflects Pauli's opinions as well as his own of course, but he was clearly aware of the inadequacy of his work and he wrote to Bohr in February 1923 [186] with the information that he was beginning to follow Bohr and Pauli in accepting that the mechanics had failed. He followed this with a letter to Pauli in which he offered two alternatives: "either new quantum conditions, or proposals for the modification of mechanics." [187]

Thus Landé, Paschen, Bohr, Pauli and Heisenberg all agreed [188] that the old quantum theory had failed, and so did Born, who wrote in 1923 that

> "The whole system of concepts of physics must be reconstructed from the ground up." [189]

Over the next few years, the last four named physicists did exactly that.

Despite the obvious failure of the Bohr model in the situations described above, the general reaction was to seek to modify it, rather than to abandon it altogether; this was partly because it had, in some cases, proved very successful, and partly because there were no viable alternatives. Thus Bohr did not progress beyond his suggestion of a Zwang and although Born looked for a more radical change, he does not seem at this stage to have had any ideas as to how this might be achieved. But Pauli and Heisenberg both had more positive ideas on the subject, and these were to be very important for the future development of quantum mechanics.

From their early days together in Munich, Pauli and Heisenberg had doubted the existence of the electron orbits in the atom.[190] Since there was no viable alternative they continued to use them where necessary, adopting Sommerfeld's attitude that "the end justifies the means",[191] but by the end of 1921 Heisenberg was well aware of the problems involved. It was already felt in Munich that one had to give up much of the old theory,[192] and Heisenberg, guided perhaps by Sommerfeld's liking for general theories, found himself comparing the Bohr atomic model on the one hand with the CP on the other, concluding that the latter was if anything the more important, since it was at least founded upon experiment.[193]

In 1922, following the results of his investigation with Born, Pauli apparently wrote to Heisenberg, asking him

> "Do you honestly believe that such things as electron orbits really exist inside the atom?", [194]

and Heisenberg recalled that "the belief in clearly describable models was seriously shaken for the first time"[195] as a result of this investigation. He wrote back to Pauli asking

> "But what is the alternative?". [196]

but in two papers written with Sommerfeld later in the year he adopted the CP, to the exclusion of the atomic model, writing that

> "We need not form exact conceptions as to the model origin of the atomic orbits", [197]

and that

> "The correspondence principle ... renounces any model insight." [198]

In February 1923, after working with Born, Heisenberg wrote to Pauli that

> "This result appears ... very bad for our present conceptions.", [199]

and in March that

> "Basically we are now both of the conviction [i.e. both he and Born] that all He. models are erroneous", [200]

while a few months later Pauli, "very distressed that I have not succeeded in finding a satisfactory model interpretation" [201] for the anomalous Zeeman effect, adopted " a purely phenomenological" description and "abandoned all use of models". [202] He wrote to Landé that

> "I am convinced that ... there is no hypothetical periodic model, and that something in principle new must be done." [203]

and in the paper on the subject he argued that

> "For the time being very great difficulties oppose the model interpretation of the empirical regularities" [204]

and that

> "It is not unreasonable to attempt to determine the simple formal properties of the values of the combination terms for the anomalous Zeeman effect by refraining from model considerations." [205]

In October, Heisenberg wrote to Pauli that

> "The model conceptions have principally only a symbolic sense", [206]

and by February 1924 Pauli was able to crystalise their opinions in a letter to Bohr:

> "The most important question appears to me to be this one, to what extent may one in general speak of fixed orbits of electrons in stationary states. I think that this can in no way be assumed as self evident, especially in view of your observations about the balance of statistical weights in coupling. Heisenberg has in my view precisely hit the mark when he doubts the possibility of speaking of fixed orbits. Doubts of this kind Kramers has never considered as reasonable. I must nevertheless insist on this because the point appears to me too important." [207]

Thus Pauli and Heisenberg gradually convinced themselves, and possibly also Bohr and Born, that the orbital model had to be abandoned; but it had already, by 1923, been abandoned for one particular sphere, as they were well aware. This very important step had been made in the theory of dispersion, which was the first branch of physics to come up against the problem of transition intensities, as opposed to mere frequencies, in the old quantum theory.

Spectral theory, with which the old quantum theory had been most concerned, was directed in its early stages towards a theoretical derivation of the observed emission and absorption frequencies, and was thus closely tied in with the Bohr atomic model which could, and did, act as a basis for such derivations. However, the atomic model itself gave no account of the intensities, and these could be obtained only through the CP, i.e. by drawing on the classical theory. The problem of intensities thus represented a later stage than that of frequencies, and a sytematic attack could not be launched upon it until after the advent of Einstein's probability coefficients and the CP: the first such attack was actually carried out by Kramers, in Copenhagen, for his Ph.D. thesis in 1919.

Earlier attempts at a quantum theory of dispersion had been made, [208] but without the CP they were doomed to failure. The procedure adopted had been to take the Bohr orbits as 'real' and apply the classical theory of electrodynamics to them. When Bohr introduced the CP in 1918, he was careful to note that

"We must consequently assume that the ordinary laws of electrodynamics cannot be applied to these [stationary] states without radical alterations!", [209]

and it was made clear that the correspondence should be between the classical frequency and the quantum transition frequency, not the quantum mechanical frequency. The first quantum theory of dispersion to abide by the CP seems to have been that due to Ladenburg in 1921.[210] Ladenburg's aim was to use dispersion theory to derive the quantum absorption and emission intensities, rather than the other way around, and he did this by applying the classical dispersion theory to a system of oscillators with the Bohr transition frequencies; from this he derived the transition probabilities, in agreement with a variety of experimental measurements. In using the classical theory of dispersion, Ladenburg virtually returned to the model of the atom as a set of resonating electrons, but he omitted the hypothesis of the resonating electrons themselves. His atom was composed neither of quantally orbiting electrons (though this picture had to be assumed if the frequencies were to be derived theoretically) nor of classically oscillating ones, but merely of absorption and emission frequencies (which he in fact took not from theory but from experiment).

This new model of the atom later became known as the 'virtual oscillator' model, and it was to play a vital role in the conceptual development of quantum theory. It was only implicit in Ladenburg's 1921 paper, but by 1923 it had been made explicit both by Ladenburg himself and by Bohr. Ladenburg and Reiche wrote in a paper of that year that

"We believe on the grounds of observed phenomena that we must consider the end result of a process of a wave of frequency ν incident upon the atom is not fundamentally different from the effect which such a wave exerts on classical oscillators: ... Even the force of scattered waves seems repeatedly to agree with that from an oscillator."[211]

This naturally appealed to Bohr, as being fully in the spirit of the CP. He admitted that things were as yet unsatisfactory, but in an unpublished manuscript of 1921 he saw hope in Ladenburg's suggestion:

"Although it is at present an unsolved problem, how a detailed theory of dispersion can be developed on the basis of the quantum theory, a promising beginning on the indicated basis might nevertheless seem to be contained in the interesting considerations about this phenomenon, recently published by Ladenburg.";[212]

and writing to Darwin on the subject in 1922, he said that he thought dispersion was more continuous than in Darwin's own theory, and that it should be attributed to some "mechanism" that was called into play when the atom was illuminated by light,

"with the effect that the reaction of the atom corresponds to that of a harmonic oscillator in the classical theory with the frequency coinciding with that of a spectral line."[213]

He summed up the situation in the published part of his 1923 opus:

"On the one hand, as is well known, the phenomena of dispersion in gases show that the process of dispersion can be described on the basis of a comparison with a system of harmonic oscillators, according to the classical electron theory. ... On the other hand, the frequencies of the absorption lines, according to the postulates of the quantum theory, are not connected in any simple way with the motions of the electrons in the normal state of the atom. ....

"According to the form of the quantum theory presented in this work, the phenomena of dispersion must then be so conceived that the reaction of the atom on being subjected to radiation is closely connected with the unknown mechanism which is answerable for the emission of the radiation on the transition between stationary states. In order to take account of these observations, it must be assumed that this mechanism, which is designated in the preceding paragraph the coupling mechanism, becomes active when the atom is illuminated in such a way that the total reaction of a number of

> atoms is the same as that of a number of harmonic oscillators in the classical theory, the frequencies of which are equal to those of the radiation emitted by the atom in the possible processes of transition, and the relative number of which is determined by the probability of occurrence of such processes of transition under the influence of illumination." [214]

In other words, one should use the classical theory for intensities, whether of dispersion or otherwise, and its alright because one doesn't know what causes transitions anyway.

It is interesting that Bohr referred in 1923 to a comparison with "a number of atoms", whereas in both the earlier extracts he had talked of comparing the atom (singular) with a set of harmonic oscillators. The distinction is important, for the Bohr atom was simple in comparison with the classical atom, and it is difficult to see how it could contain within it all the possible transition frequencies, as the classical one did; in fact it was an explicit feature of the model when it was introduced that it should not do so. If the Bohr atom was to be retained, therefore, the comparison should have been with a number of atoms, and it would seem that in print Bohr did retain it, but that in private he did not: he was not given to loose wording, after all. As we have noted, the idea of replacing the orbital model was around at the time, and Pauli in fact wrote to Sommerfeld in June 1923 that

> "I often think, that maybe not only in dispersion, where it is under the influence of a simply harmonic periodic external force, but also in the mutual effects of the electrons in the atom, the individual electron orbits control themselves more as a system of oscillators in which the frequencies are associated not with the motion but with the transition (something similar has already been said by Epstein)." [215]

I cannot, unfortunately, locate Epstein's remark, which may have been verbal, but Pauli's is extremely interesting. Not only each individual atom, but also each individual electron orbit seems to be associated with a set of oscillators, and, moreover, this picture is extended beyond the single case of dispersion.

Pauli's remark was made privately, but the quantum physicists formed a very close-knit community, and his views would quickly have become generally known. He had spent the previous year with Bohr in Copnhagen, and saw Heisenberg briefly the following January. He was also in Copenhagen, for a "welcome diversion"[216] with Kramers from October to December of 1923, and the subject of discussion then was almost certainly the precise content of the letter to Sommerfeld. Kramers wrote to Bohr during this period that he had been preparing a paper for the <u>Philosophical Magazine</u> and entertaining Pauli, and we may be confident that the two overlapped. In the paper, on X-rays,[217] Kramers more or less repeated Bohr's views on dispersion, but he was noticeably more emphatic about it being a number of atoms that must be used in the correspondence, writing that

> "The quantum theory in its present state tells nothing about the mechanism of absorption and does not therefore permit the direct calculation of the probability that an absorption process may occur." [218]

> "The only procedure which offers itself at present seems to consist in estimating the statistical result of a great number of such emission processes - in a way suggested by Bohr's correspondence principle - from the radiation which on the classical electron theory would be emitted by the free electrons in consequence of the change in motion produced by the forces owing to the electric particles in the atom." [219]

> "One should expect that every possible transition corresponds to a certain frequency present in the motion of the electron." [220]

This seems to be a combination of Pauli's ideas, including the extension of the oscillator treatment beyond dispersion, and even the oscillator type behaviour of the individual electrons, with Kramers' reluctance, as noted by Pauli,[221] to give up the orbital model; it confirms that Pauli's ideas were discussed.

Kramers' paper was published in November, and it represents the stage that the virtual oscillator approach had reached by the end of 1923. Like the CP, of which it was an offspring, the quantum theory of dispersion was very successful, but it represented the total failure of the old quantum theory. For it was in essence no more than a continuation of the classical theory, but without the structural picture that this had involved.

II.5. SUMMARY AND DISCUSSION

The period 1921-3 saw a great revival of interest in the quantum problem and with this, partly causing it and partly caused by it, came a serious conceptual crisis. Helped by new experimental results, the wave-particle dilemma sharpened considerably, while the failure of the old quantum theory also posed great conceptual problems. Within both contexts it came to be recognised by 1923 that a new theory -- a new mechanics or a new conceptual framework -- was needed, and by then there were also a variety of more particular suggestions as to what should be changed.

Working from the related problems of interpreting the frequency of and imposing interference properties on light-quanta (whose existence he took to be empirically established), de Broglie developed a theory in which light-quanta and material particles were treated as qualitatively identical and in which both were initially associated with, and later supposed to be constructed from, groups of waves. De Broglie's approach was essentially structural and based on the particle concept; but in its final form the theory treated the particles as secondary manifestations of phase waves. This was in agreement with Einstein's current conception of light-quanta, but de Broglie's conclusion was not derived as Einstein's had been from any fundamental analysis of the requirements of a unified theory. Rather, it arose from purely structural considerations, and from the success of the matter-wave concept. It also reflected a bias toward the wave concept that seems to be intrinsic in the dual conception of light, for although the proponents of duality, and especially Einstein and de Broglie, tended to start from a particle concept of light, the problem of explaining interference effects in terms (according to Taylor's experiments) of <u>single</u> quanta naturally led to the particles being given a wave basis. It was possible to conceive of singularities or resonances in a wave form which might appear as particles, but it was not possible to derive wave behaviour from a single particle.

This intrinsic wave bias was to be an important element in the development of quantum mechanics and it may be linked, as we shall see, with developments within the old quantum theory. The other feature of de Broglie's theory to be noted is that the waves were phase waves. They were not yet propagated in multi-dimensional phase-space, as Schrodinger's waves were later to be, and there was as yet no explicit departure form the classical ideal of a consistent structural description;

In analysing the light-quantum concept, Bohr had seen much the same problems as had de Broglie, but rather than seeking to overcome them by modifying that concept he had taken them as proof that it was totally unacceptable. Einstein had suggested as early as 1911 that the only alternative to the light-quantum concept was the abandonment of energy conservation, and although the conclusion ahad not been accepted at the time Bohr had come by 1923 not only to believe in energy non-conservation but also to express this belief in public. Moreover, investigations within the old quantum theory had led to the same conclusion and its possibility, if not its necessity, seems to have been fairly widely accepted by this date. The idea of energy non-conservation was in clear opposition to the classical theories of physics, but it could only subjectively be seen as opposed to the fundamental ideals of these theories. Einstein seems to have equated it with an abandonment of causality, and others no doubt did likewise, but those who propounded it seem for the most part to have retained the classical ideals and to have seen it as no threat to them.

Bohr was the exception to this rule, and he challenged the classical ideals by propounding the abandonment of a space-time description and by accepting, privately, an abandonment of causality. Both these innovations, which were closely linked and stemmed from the problem of explaining quantum phenomena with the wave concept of light, went completely against the classical ideal of a consistent causal description in space and time, but the former was far more acceptable (largely because it was not clear what it meant) and was pronounced publicly; the latter went against the fundamental essence of physics as an exact predictive science, and was at this stage expressed only in private. The origins of the two ideas may also have differed slightly, for while those of the former idea seem to have been wholly internal to the quantum problem, those of the latter may have included Bohr's philosophical background: but his is not clear. There were very great pressures towards anticausality both internal and external to the quantum problem, and there were other advocates of this viewpoint: Schottky, who was influenced by the popular philosophy of the Weimar culture, and Schrödinger, who was influenced by the nineteenth century philosophical backround of Exner. But both were isolated externally (and personally) motivated cases, of no importance for the future development of quantum theory or even for the development of the causality issue within that theory. Senftleben conducted and analysis from which he deduced that causality could not be justified in the theory (though he did not advocate anticausality), and Tetrode reacted to what he saw as an absence of causality in it, but again these views were of no real importance.

Bohr's ideas on causality, conservation and a space-time description were virtually peculiar to himself. They were made possible by the same characteristic, namely a belief in the failure of classical mechanics, as had played a large part in the origin of his atomic model, and this characteristic reappeared also in the context of the failure of the old quantum theory. Led by Bohr's advocacy of a non-mechanical "Zwang", most of the physicists engaged on the problems of the theory came to recognise the need for some new mechanics. The other element behind his theory of 1913, the use of a structural model, seems however to have shrunk in importance as his ideas matured. As his atomic model failed, he came to emphasise the importance of the correspondence principle, and also its status as part of the foundations of the old quantum theory, rather than as a mere link between the quantum and classical theories. In a sense this was necessary, for the theory without the CP was incomplete, being unable to predict intensities; moreover, correspondence with the accurate parts of an old theory had always been used as a guide towards a new one. But Bohr was talking about the logical structure of an existing theory, not about the construction of a new one, and his attitude, in so far as it implies a renunciation of the possibility of completing the theory any other way, implies also a renunciation of the possibility of deducing the macroscopic (classical) behaviour of a system from the microscopic (quantum) behaviour -- a possibility that could not be doubted within the classical ideology of physics. It is tempting to suppose such a renunciation, which would have had enormous epistemological implications, but there is no evidence in Bohr's writing, or in that of any of his colleagues, to support this supposition. Although his later references to the subject were confused, it seems clear that at this time Bohr was merely describing the 'present state of the theory'. As things stood, it had, to be logically complete, to incorporate the CP as part of its foundations, and Bohr seems to have seen this not as an epistemological innovation but as an indication of where to seek the new theory that he felt was needed — in a theory, that is, that combined aspects of the quantum and classical theories.[222]

Bohr treated the atomic model and the correspondence principle as part of the same theory, and did not pit the one against the other. Since they were applicable to different problems, such a comparison could not in fact be strictly made, but Pauli and Heisenberg seem to have seen the two aspects as contradictory and, in a sense, as rival theories. Both these physicists had started research under Sommerfeld in the new positivist tradition that we noted in the last chapter, and both grew up sceptical of the Bohr model but with the idea of a 'quantum theory' well ingrained. By the end of 1923, both had rejected the use of the orbital model (where possible) in favour of a purely behavioral, phenomenalist methodology based on the CP In dispersion theory, where the orbital model found no use at all, this behavioural approach was actively employed by Bohr, Kramers and Ladenburg, but in 1923 Pauli went much further and suggested that not only the atom but also the individual electrons inside it should be seen in terms of sets of frequencies, a model of the atom's structure being abandoned altogether. This can be seen as reflecting the dominance of the wave side of duality for light, the electrons in the atom were restricted in number just as were the light-quanta in interference experiments

The new picture of the atom was not directly in conflict with the classical ideals, for it amounted to no more than the recognition that the atom was far more complex than Bohr had allowed, but it was nevertheless one of the most important changes of the crisis period for the future of these ideals. It took the old quantum theory out of its rather peculiar state of 'structured positivism' to one of a more straightforward phenomenalism (in methodology of course, not as yet in epistemology). The atom was replaced by its absorption and emission frequencies, with no structural origin being given for these frequencies but with a behavioural quantum condition being imposed upon them. Because of the need for the quantum behaviour, the new methodology could only have arisen after a period with the old, in which the rules of quantum behaviour were built up from the Bohr model, but once this behaviour had been established it was possible to dispense with the model.

To summarise, the important elements that had been introduced to quantum theory by 1923 were:de Broglie's conception of light and matter, with its use of multidimensional wave forms; the phenomenalist conception of the atom as a set of frequencies; the feeling that a new mechanics was needed; the preparedness to consider the abandonment of energy conservation; and Bohr's abandonment of a space-time description. From these factors stemmed quantum mechanics and a formalised and generally accepted new characterisation of physics.

III

THE ORIGINS OF QUANTUM MECHANICS

III.1. THE BOHR-KRAMERS-SLATER THEORY

The famous paper by Bohr, Kramers and Slater (BKS) on the quantum theory of radiation was completed in January 1924 and published the following May.[1] Its aim was

> "to arrive at a consistent description of optical phenomena by connecting the discontinuous effects occurring in atoms with the continuous radiation field in a somewhat different manner from what is usually done." [2]

In many ways, the paper corresponded closely with Bohr's 1923 survey,[3] to which it may, according to BKS, be considered a supplement. Light-quanta were discussed, for example, and dismissed since they

> "can obviously not be considered as a satisfactory resolution to the problem of light propagation. This is clear even from the fact that the radiation 'frequency' ν appearing in the theory is defined by experiments on interference phenomena which apparently demand fo their interpretation a wave constitution of light." [4]

The introduction of transition probabilities was accompanied by the usual justification "at the present state of science",[5] and doubts were noted

> "whether the detailed interpretation of the interaction between matter and radiation can be given at all in terms of a causal description in space and time of the kind hitherto used for the interpretation of natural phenomena."[6]

The paper did however go beyond earlier accounts in a new hypothesis:

> "We will assume that a given atom in a certain stationary state will communicate continually with other atoms through a time-spatial mechanism which is virtually equivalent with the field of radiation which on the classical theory would originate from the virtual harmonic oscillators corresponding with the various possible transitions to other stationary states. Further, we will assume that the occurrence of transition processes for the given atom itself, as well as for other atoms with which it is in mutual communication, is connected with the mechanism by probability laws which are analagous to those which in Einstein's theory hold for the induced transitions between stationary states when illuminated by radiation." [Spontaneous radiation was induced by the atom's own virtual radiation field.] "The occurrence of certain transitions in a given atom will depend on the initial stationary state of the atom itself and on the states of the atoms with which it is in communication through the virtual radiation field, but not on the occurrence of transition processes in the latter atoms. .... We abandon .... any attempt at a causal connexion between the transitions in distant atoms, and especially a direct application of the principles of conservation of energy and momentum, so characteristic for the classical theories." [7]

The first thing we note about this hypothesis is that it contains no new individual ideas. As we have seen, a virtual oscillator type of treatment was already in general use for dispersion theory and , under the guise of the CP, it was finding increased favour in atomic theory in general. The application of this treatment to "a given atom" had not been a feature of published work, but it had been considered privately and must have been familiar to physicists working in the field. Energy and momentum non-conservation had been fairly widely mooted and had been strongly advocated by Bohr, who had also gone a long way towards the abandonment of causality.[8]

The theory contained no new ingredients then, but the emphasis upon the different ideas, their combination in a single theory, and their publication, were all new and of extreme importance. Never before, in public at least, had

Bohr abandoned causality without the proviso of it being "at the present state of science"; the application of the virtual oscillators to a single atom had not been suggested in public either, and nor had the concept of the oscillators forming a symbolic atomic model (as suggested privately by Pauli) rather than just arising from an application, to save the appearances, of the CP. Indeed, although they remained "virtual", and although the "formal nature"[9] of the treatment was stressed, the oscillators acquired in the BKS theory a far greater degree of reality than hitherto.

The implications of the new theory were enormous, and will of course be discussed, but first we must ask what brought it about. It represents, clearly, a crystallisation of ideas that were already 'in the air', but what was the catalyst behind this crystallisation? And why did Pauli, who seems earlier to have proposed a virtual oscillator atomic model, reject the theory while Kramers, who had refused to abandon the orbital model and who had stressed that the virtual oscillator treatment should only be applied to a large number of atoms, was among its authors? The answer to the second question will emerge in full only gradually, as we build up through an analysis of the development of the BKS theory a picture of the different attitudes of the physicists concerned with it. As to the first question, it is well known that the catalyst was an idea of Slater's, but the origins and reception of this idea have not before been thoroughly analysed: they are in fact quite fascinating.

Slater's idea

Slater arrived in Copenhagen after a short spell in Cambridge (England), soon after completing his doctorate at Harvard. He recalled[10] that he had the idea in his mind before leaving Harvard that one could not abandon, as Bohr had done, the relationship between the length of a finite wave train and the width of a spectral line. In the classical theory, the sharpness of a spectral line was related to the period over which a wave was emitted, the wave undergoing a gradual change of frequency as the emitting electron spiralled inwards. In the Bohr theory the emission process could be treated as virtually instantaneous and Bohr, arguing that a visible spectral line arose from a large number of transitions in different atoms, had related the line width to an uncertainty of motion in the stationary states. Owing to the restricted applicability of the classical theory to the stationary states there was an uncertainty as to their precise energy, and assuming independent variations for each state Bohr had calculated a spread in frequencies corresponding to the classical one. Slater claimed that he found the idea of instantaneous transitions "quite silly", and that he felt a long period of emission was needed. He would appear, therefore, to have inclined strongly towards the classical wave picture of light, but in December 1923 he wrote to Kramers from Cambridge, telling him of his

> "rather surprising conclusion that the only possible way of getting a consistent explanation was in the direction of light-quanta",[11]

and expounding an idea as to how this might be done:

> "Of course, the quanta can't travel in a straight line with the speed of light; but it seems possible to suppose that there is an electromagnetic field, produced not by the actual motion of the electrons, but with motions with the frequency of possible emission lines (or, in an impressed field, of possible absorption lines), and amplitudes determined by the correspondence principle, the function of this field being to determine the motion of the quanta. If this motion is determined by the condition that Poynting's

theorem shall hold over an average taken over a long period of time, definite patterns are described, and the probability of moving along the paths is such, for example, as to account for interference, many quanta being led to the bright spots in the field." 5Ψo

From where did this idea come? Slater recalled attending a course given by Cunningham at Cambridge, which got him thinking about Poynting's vector, and firmly denied any other influence. But although this could conceivably have triggered off the idea it seems far more likely to have provided the final piece of an idea already emerging. In his letter Slater told Kramers that he had been looking at dispersion theory, so he must already have been familiar, through the work of Ladenburg, Bohr[13] and maybe Kramers himself, with the use of virtual oscillators in their old form. His idea also bears a strong resemblance to Einstein's dual theories, which he might well have heard of in America, Lorentz having discussed them in California. But neither Bohr's theory nor Einstein's seems likely to have sparked off Slater's idea, and neither would have led to the conception of light that he now put forward. So where did his idea come from, and what made him change from a classical conception of light to the one he did?

Slater denied all influences. Asked specifically about Fowler, he replied that (although "under [his] guidance")[14]† he got from him only politeness,[15] and asked if he knew of de Broglie's work at the time he replied in the negative (though the interviewers did not, unfortunately, press the point,[16] mentioning it only in passing). But he did have some reason, as we shall see, to be possessive of his idea, and the circumstantial evidence seems to me to point very strongly to his having been aware of, and sparked off by, de Broglie's ideas. His sudden conversion to the light-quantum concept could, admittedly, have resulted from the Compton effect (the timing would have been reasonable),[17] but the text of the letter to Kramers suggests a theoretical origin, and this suggests de Broglie, especially as de Broglie's paper for the Philosophical Magazine[18] was completed in October 1923 and communicated, by Fowler, for publication in February. We do not know whether Fowler was in a position, by the end of November, to refer Slater to de Broglie's work, but this seems likely; and if he was in a position to do this, he would surely have done it, for he must have known that Slater was interested in physics at the same fundamental level as de Broglie, and was sympathetic to such bold hypotheses as de Broglie put forward.

This speculation is strongly supported, moreover, by the other evidence, for Slater wrote to Kramers of quanta that "can't travel in a straight line with the speed of light", and although clearly applicable to de Broglie's light-quanta, which were material particles guided by waves, this was not applicable, so far as I can see, to any other light-quantum conception of the period. All other authors attributed the absolute velocity 'c' to the quanta, and we can therefore conclude with some confidence that Slater's idea, and through it the BKS theory, arose from the introduction of de Broglie's conceptions into the context (of Slater's letter to Kramers and of his thoughts as he prepared to go to Copenhagen) of Bohr's dispersion theory. De Broglie had associated with each atom a group of matter waves, and Bohr and Kramers had associated with a number of atoms a set of virtual classical oscillators, but since the two innovations had been features of entirely different world-views they had not previously been connected. Slater now made the connection, identifying the virtual oscillators with the source of the de Broglie waves, and thus investing them with a degree of reality they had so far lacked. As to the motivation behind his acceptance of de Broglie's ideas, which would seem to be seriously at odds with his original preference for the classical conception of light, this seems to have been connected with the problem of line widths that had troubled him in Harvard. For a separation, as in de Broglie's theory, of the energy-less wave emission from the particulate energy emission allowed the former to take place over a period of time and the latter instantaneously. Transferring this separation to the context of the Bohr theory, this allowed a satisfactory (so far as Slater was concerned) interpretation of spectral widths, without sacrificing the explanation of quantum transition effects.

Bohr's modifications

Bohr and Kramers adopted Slater's idea immediately on his arrival in Copenhagen, but they also modified it considerably. The idea, as published under Slater's name, was that

> "An atom may, in fact, be supposed to communicate with other atoms all the time it is in a stationary state, by means of a virtual field of radiation, originating from the oscillators having the frequencies of possible quantum transitions, and the function of which is to provide for stationary states conservation of energy and momentum by determining the probabilities of quantum transitions. The part of the field originating from the given atom itself is supposed to induce a probability that that atom lose energy spontaneously, while radiation from external sources is regarded as inducing additional probabilities that it gain or lose energy much as Einstein has suggested." [19]

But this, Slater recalled, was a third draft of his idea, much altered by Bohr to agree better with his own ideas, in particular on energy conservation and the causal relations between transitions in distant atoms.[20] Slater explained at the time that

> "The idea of the activity of the stationary states presented here suggested itself to me in the course of an attempt to combine the elements of the theories of electrodynamics and of light-quanta by setting up a field to guide discrete quanta. ... But when the idea with that interpretation was described to Dr. Kramers, he pointed out that it scarcely suggested the definite coupling between emission and absorption processes which light quanta provide." [21]

In a letter to Nature the following year, after the experiments of Compton, Simon, Bothe and Geiger[22] seemed to have decided in favour of light-quanta, he explained that when he had presented his original ideas to Bohr and Kramers,

> "they pointed out that the advantages of this essential feature would be kept, although rejecting the corpuscular theory, by using a field to induce a probability of transition rather than by guiding corpuscular quanta. On reflection it appeared that no phenomena at that time known demanded the existence of corpuscles." [23]

But this picture of him being calmly won over to the Copenhagen viewpoint is quite misleading.

He recalled (in an interview that was only reluctantly given) that he had a "horrible time" in Copenhagen, as Bohr and Kramers commandeered one half of his idea and simply rejected the other half.[24] Bohr apparently said that it was a fine idea, but that he and Kramers would modify it a bit — and that was the last Slater saw of it. He wrote to van Vleck in July 1924,[25] telling him that

Bohr and Kramers wrote not only the whole BKS paper themselves, but also that published under his name. He recalled that he himself had always preferred the light-quantum concept, together with both energy conservation and causality, and was not in fact converted to the Copenhagen viewpoint at all. Thus half his idea was effectively eclipsed by Bohr's use of it while the other half, which turned out to be more accurate than what replaced it, was rejected in his name but without his agreement. It is no wonder that he was possessive about his idea, and denied any influences.

Bohr's reaction is easily understood. The concept of a virtual oscillator model of the individual atom must already have formed loosely in his mind; Slater gave clear expression to that concept, and he accepted it wholeheartedly. The suggestion of light-quanta, on the other hand, struck him as heresy. He had already concluded that without light-quanta there could be no energy conservation, and he must have seen immediately that in the new theory there could be no causality either. He had already abandoned any prospect of causality "in the present state of science" when the virtual oscillator concept had been no more than an application of the CP designed to save the appearances, and once this concept was raised to the level of a 'real' model the possibility of causality (through some unknown mechanism) was completely ruled out.

With Kramers, the situation is less clear, for he had previously rejected the virtual oscillator treatment for individual atoms, and had been less enthusiastic than Bohr about abandoning causality. His views will become more apparent as we consider the development of the BKS theory, but the clue to his reaction here seems to be contained in Slater's recollections. Slater got on very badly with Bohr; he respected Kramers as an individual and seems to have got on quite well with him, but he found that when with Bohr he always became an acolyte, a "yes-man". As we shall see, Kramers had his own interpretation of the virtual oscillator concept, but he was Bohr's assistant, and he was working on dispersion theory, the context of the new idea: it was therefore natural for him to follow Bohr's line in this case.

Justification of the theory

Having transformed Slater's idea into his own version of the virtual oscillator theory, Bohr (for he was the real author of the BKS paper) had to justify it, and this in the face of some considerable difficulties. The theory did have one great advantage in that it allowed a treatment of the wave-like behaviour of radiation without resorting to pure and blatant positivism, but it achieved this only at an enormous cost. In the first place, there was the ontological status of the virtual oscillators. Heisenberg, with the genius of hindsight, thought that the assignment of a whole set of transition probabilities (virtual oscillator intensities) to an individual atom gave the probability concept a kind of reality on a par with Aristotle's Potentiality.[26] There is no indication of BKS having thought of it in this way, but the notion of Potentiality characterises well the status of the virtual oscillators and radiation, and it was not to everyone's taste: Pauli, for example, denounced the "virtualisation of physics".[27]

A second problem was that there were now two models of the atom, apparently incompatible yet both indispensable — a rather ironic result of Bohr's attempts to avoid the same situation for radiation. For consideration of intensities, the atom was treated as composed of virtual oscillators, but the concept of electron orbits was still necessary in order to calculate the frequencies. Even if these problems were ignored (and they could not be solved), the rejection of the light-quantum concept had to be reconciled with the quantum effect and, above all else, Bohr's insistence on non-conservation and the absence of a space-time description, and especially his repudiation of causality, all had to be justified.

Attempting such a justification, Bohr emphasised as he had in the past that the necessity of a continuous treatment of optical phenomena such as interference did "not admit an interpretation based on a simple causal connexion with transition processes in the propagating medium",[28] and he also drew an argument in favour of the new theory from some experiments by Stark. Stark had shown that atoms moving fast through a changing electric field behaved, apart from the Doppler effect, at any point as if they were at rest there, even when the change in the field took place well within the expected duration of a stationary state.[29] According to Bohr, this caused problems in the old theory where it required that the final states of as yet uneffected transitions should adapt to the field before the transitions were completed, and before they therefore existed. In the new theory, however, the oscillators could change gradually with the field, and the effect would follow naturally.[30]

These arguments did support the theory, but not very conclusively, and when it came to an explanation of the Compton effect Bohr ran into serious trouble. He did try, but his attempt, involving a motion of the virtual oscillators different from that of the electrons to which they were attached, was not remotely convincing.[31]

Reception of the theory

Fortunately for the development of the theory Bohr's prejudice's were well known, and it was quite possible to accept the theory in a spirit of pure positivism, as a new formula for saving the appearances, while rejecting or reserving judgement upon the anti-particle aspect with its associated non-conservation and anticausality. This was in fact the most common attitude among those concerned with the technical problems to which the theory related, and Born for example, pleased "above all in the fact that classical optics comes largely into its own again",[32] adopted the oscillator technique but explicitly reserved his opinion on the rest of the theory, taking the technique

> "independent of the critically important and still disputed conceptual framework of that theory, such as the statistical interpretation of energy and momentum transfer." [33]

Kramers applied the new technique to dispersion theory, but as we shall see he played down the physical innovations of te new theory (with which he does not seem to have been happy) and treated it as merely an extension of the CP.

Ladenburg wrote to Kramers offering congratulations and saying that both he and Reiche were glad that his idea coincided so well with their own considerations, but this was a response to Kramers' dispersion theory rather than to BKS, and it seems unlikely that they would have accepted the more controversial aspects of the BKS theory.[34] They did not in fact mention the issues of causality and conservation, and nor did the other physicists to favour or develop the theory,[35] with the single exception of Schrödinger. He was still under the influence of Exner, and wrote to Bohr on reading the BKS paper that since he had worked with Exner he had always been favourable to an element of absolute chance;[36] he also liked the return to the techniques of the classical theory, and published a paper full of praise for the new theory.[37]

Schrödinger did not, however, develop that theory; the only physicist among those who did to have shown any sign of accepting the wider implications was Heisenberg, who wrote to Bohr that although one of Einstein's objections to the theory troubled him he hoped to get out of it;[38] but even he seems to have been taking a very positivistic attitude. His first reaction to the theory was that

> "Bohr's work on radiation is indeed very interesting, but I do not really see it as an essential progress", [39]

and he only seems to have analysed it at any length after Born had tied it in with some of his own ideas, by when the implications for causality and conservation had been clearly relegated to secondary importance.

Among those concerned with the technical implications of the new theory, therefore, the universal attitude seems to have been to accept it as a technical development, and to reserve judgement on any deeper implications it might have. In fact, these implications were not debated very widely at all — the theory made nothing like the impact of, say, the Compton effect — and apart from Schrödinger those who did debate them were unanimous in rejecting them. In these terms Slater was, ironically, one of the opponents of the theory that bore his name, and he was joined in his opposition by the Americans Compton and van Vleck, both of whom insisted that the Compton effect provided "definite evidence of the existence of light-quanta",[40] while Compton also defended exact conservation.[41] Another critic was Stoner, who thought that the assumptions of the theory were not justified by its achievements, writing that

> "As to conservation, it may at least be said that it seems unnatural to assume that it does not hold in individual processes when there is no definite evidence of its breakdown, unless the supposition leads to a much more complete and satisfying explanation of observed phenomena than has hitherto been put forward." [42]

This opinion was also shared by Sommerfeld, who was skeptical of the BKS "compromise",[43] while Ehrenfest wrote to Einstein, who was the theory's strongest critic, that "this time, as an exception, I firmly believe you are right." [44]

Einstein, despite Ladenburg's optimistic report of his opinion that it "was decidedly not unfavourable",[45] rejected the BKS theory outright. He wrote to Born that

> "I find the idea quite intolerable that an electron exposed to radiation should choose <u>of its own free will</u>, not only its moment to jump off, but also its direction", [46]

and writing to Ehrenfest he insisted that light-quanta "would not allow themselves to be dispensed with",[47] and gave a whole list of reasons why he could not accept the BKS theory, including that

> "Nature seems to adhere strictly to conservation laws — why should action at a distance be different", that
>
> "A final abandonment of strict causality is very hard for me to tolerate",

and that, as a result of the independence of the transitions in distant atoms,

> "A box with reflecting walls containing radiation, in empty space that is free from radiation, would have to carry out an ever increasing Brownian motion." [48]

Pauli was able to discuss Einstein's objections with him, and included a similar list in a letter to Bohr.[49] Of the last of the above factors (which Einstein had already considered in the context of his own search for a dual theory, we might recall) he reported that Einstein "finds this disgusting (so he says)",[50] and he also included another of Einstein's arguments, namely that the spectral widths now appeared to depend on both the decay times and the uncertainty of the stationary state definition, with no reason given why these should coincide: the theory needed a "pre-established harmony", which he did not like.

Pauli himself was privileged enough to receive a preprint of the paper from Bohr, but he was also irreverent enought to reply that

> "I have tried, on the basis of the definitions of these two words [kommunisieren, virtuell] to guess what your work is really about. But its not easy. In any case its very interesting to me, and if I can be of any help regarding the grammar, I will gladly oblige." [51]

Although he had expressed ideas close to those of the new theory the previous year, Pauli had never shared Bohr's views on light-quanta; he now thought that the oscillator concept was getting out of hand, and leading Bohr in the wrong direction. Writing to Bohr in October, he admitted that he could not reject the theory on purely scientific grounds, that Einstein's objections did not worry him in particular, and that his own objections were not strictly logical. He had to admit, however, that he was totally opposed to the BKS theory, and so, moreover, were "many other physicists, perhaps even the majority."[52] Heisenberg wrote to Bohr in January 1925 that Pauli

> "does not however believe in the virtual oscillators and denounces the 'virtualisation' of physics" [53]

but he had to add that "it is not clear to me what he means by that", and there is no clear account of Pauli's objections until he attacked the theory in a letter to Kramers later that year:

> "They thus go in a completely wrong direction: it is not the energy concept which is to be modified, but the concepts of motion and force. Indeed one can define no certain path for the light quanta where interference phenomena are present, but nor can one define any such paths for the electrons in the atoms; and it is just as little justifiable to doubt therefore the existence of the light quanta on the grounds of the interference phenomena as it would be to doubt the existence of the electron." [54]

The first part of this criticism is clearly related to Heisenberg's new kinematics, newly found at the time of writing,[55] but the second is applicable in a wider context and at an earlier date, for Pauli's objection to the concept of electron orbits was already well established.

In summary, the reaction to the wider implications of the BKS theory was overwhelmingly hostile.[56] But among those able to ignore these aspects, the technique of the new theory was accepted. A new concept of a virtual oscillator model of the individual atom was thus established and the ideas of an intermediate state of reality and of the abandonment of causality were, if not accepted, at least put 'in the air'. In this way determinism and the expectation of a consistent structural description were seriously and openly challenged.

III.2. THE DEVELOPMENT OF THE VIRTUAL OSCILLATOR THEORY

Kramers' dispersion theory and Born's quantum mechanics

The first application of the virtual oscillator concept was due to Kramers, who completed a short paper on dispersion theory in March 1924, before the BKS paper had been published.[57] Starting from the classical formula for the electric moment of dispersed radiation,

$$P = E\sum_i \left(f_i \frac{e^2}{m} \cdot \frac{1}{4\pi^2(\nu_i^2 - \nu^2)} \right)$$

[f_i constants
E amplitude, elec. mom. incident radn.
ν frequency, " " " "
e, m charge, mass, of oscillating electrons
ν_i frequencies " " "]

he claimed that an application of the CP gave the expression

$$P = E\sum_i \left(A_i^a \tau_i^a \frac{e^2}{m} \cdot \frac{1}{4\pi^2\{(\nu_i^a)^2 - \nu^2\}} \right) - E\sum_j \left(A_j^e \tau_j^e \frac{e^2}{m} \cdot \frac{1}{4\pi^2\{(\nu_j^e)^2 - \nu^2\}} \right)$$

[$\nu_i^a, \nu_i^e, A_i^a, A_j^e$ absn. & emn. frequencies and probabilities
τ_i^a, τ_j^e constants; $\tau_i^a = 3mc^3/8\pi^2e^2(\nu_i^a)^2$ etc.]

and that

"The reaction of the atom against the incident radiation can thus be compared with the action of a set of virtual harmonic oscillators inside the atom, conjugated with the different possible transitions to other stationary states." [58]

Kramers gave no derivation of his formula in this first paper, but an objection by Breit to the negative contribution of the emission oscillators[59] gave him the chance to outline a derivation in a second paper, dated July.[60] There he started from the classical formula for the electric moment of forced vibrations for a motion whose undisturbed moment was given in terms of the action variables J_i by

$$M = \sum_\tau C_\tau(J) \cdot \cos(2\pi\omega t + \gamma).$$

[$\tau \equiv (\tau_1, \tau_2, \ldots \tau_n)$; $\omega \equiv \sum_i \omega_i \tau_i$
$J \equiv (J_1, \ldots J_n)$]

This was

$$P = \frac{E}{2}\sum_\tau \frac{\partial}{\partial J}\left(\frac{C_\tau^2(J)\,\omega}{\omega^2 - \nu^2}\right)\cos(2\pi\nu t),$$

[$\partial/\partial J \equiv \sum_i \tau_i \partial/\partial J_i$; E, ν, as above]

and he assumed that in the limit of the correspondence principle the formula could be carried over to the quantum theory. For the general quantum expression, he noted that quantum theory replaced the form $\omega = \partial H/\partial J$ by $\nu = \Delta H/h$, and he suggested that the argument presented itself that

"The symbol $\partial/\partial J$ [standard notation; Kramers used $\partial/\partial I$] has to be replaced by a similar difference symbol divided by h." [61]

In this particular case, $h\frac{\partial}{\partial J}\left(\frac{C^2\omega}{\omega^2 - \nu^2}\right)$ was replaced by

$$\frac{3c^3 A^a h}{(2\pi)^4(\nu^a)^2\{(\nu^a)^2 - \nu^2\}} - \frac{3c^3 A^e h}{(2\pi)^4(\nu^e)^2\{(\nu^e)^2 - \nu^2\}}$$

where $A\omega h = (2\pi\omega)^4 C^2/3c^3$ (energy per unit time in the CP limit), and his dispersion formula followed.

Between Kramers' two papers, the virtual oscillator concept was further developed and applied by Born, who completed his quantum peturbation theory in June.[62] His basic idea was to extend Kramers' treatment of dispersion, a matter-radiation interaction, to matter-matter couplings, thus achieving what he called a "quantum mechanics". He argued that

"Since one knows that ... atoms react to light waves completely 'non-mechanically', it is not to be expected either that the interaction between the electrons of one and the same atom should comply with the laws of classical mechanics; this disposes of any attempt to calculate the stationary orbits by using a classical perturbation theory complemented by quantum rules. For as long as one does not know the laws for the interaction of light with atoms, i.e. the connection of dispersion with atomic structure and quantum jumps, one is left all the more in the dark about the laws of interaction between several electrons of the same atom." [63]

He therefore considered whether it might be possible to extend Kramers' treatment, closer study of which

"leads one to investigate whether the method of quantisation used by him is not based on some general property of perturbed mechanical systems." [64]

In pursuit of this investigation, Born first reviewed classical peturbation theory, bringing the formulae for systems with and without external forces into the same form, and then went over classical dispersion theory. Then, in the crucial third chapter of the paper, he introduced the virtual oscillator concept, with "emission resonators", $\nu(n,n')$, and "absorption resonators", $\nu(n',n)$, for any given state, each corresponding to a higher harmonic of that state in classical theory ($(\nu\tau)$; $\tau = |n-n'|$).[65]

In the old quantum theory, quantisation had been performed in terms of action integrals, $J_k = n_k h$, and guided by this Born attempted to connect the quantum and classical frequencies of an unperturbed system H_0:

"The following quantitative connection exists between the classical frequency $(\nu\tau)$ and the quantum-theoretical absorption frequency $\nu(n',n)$. Let us imagine that the transition $n_k \to n_k' = n_k + \tau_k$ is performed in a 'linear' way; i.e. let us set for the action integrals $J_k = h(n_k + \mu\tau_k)$, $0 \le \mu \le 1$. Then we obtain on the one hand

$$(\nu\tau) = \sum_k \nu_k \tau_k = \sum_k \frac{\partial H_0}{\partial J_k}\tau_k = \frac{1}{h}\sum \frac{\partial H_0}{\partial J_k}\cdot\frac{\partial J_k}{\partial \mu} = \frac{1}{h}\frac{dH_0}{d\mu}$$

and on the other $\nu(n',n) = \frac{1}{h}\left[H_0(n+\tau) - H_0(n)\right]$;

therefore $\nu(n+\tau,n) = \int_0^1 (\nu\tau)\, d\mu$ ..... one can say that the ways in which $\nu(n+\tau,n)$ and $(\nu\tau)$ are obtained from H_0 stand in the same relationship as differential coefficients stand to difference-quotients." [66]

He next considered the interaction process described by the perturbation function λH_1 . For the classical perturbation energy he had obtained

$$W_2 = -\sum_{(\nu\tau)>0}{}' \sum_k \tau_k \frac{\partial}{\partial J_k}\left(\frac{|C_\tau|^2}{(\nu\tau)}\right)$$

, but, he noticed, this was of the same form as the classical frequency in that it was characterised by the operator

$$\sum_k \tau_k \partial/\partial J_k = \frac{1}{h}\, d/d\mu$$

. He concluded that

"We are therefore as good as forced to adopt the rule that we have to replace a classically calculated quantity, wherever it is of the form $\sum_k \tau_k \frac{\partial \phi}{\partial J_k} = \frac{1}{h}\, d\phi/d\mu$ by the linear average or difference quotient

$$\int_0^1 \sum_k \tau_k \frac{\partial \phi}{\partial J_k}\, d\mu = \frac{1}{h}\left[\phi(n+\tau) - \phi(n)\right]."$$ [67]

He noted that the classical C_τ would also have to be replaced, or rather, since "evidently it is only the quadratic expressions $|C_\tau|^2 = C_\tau C_{-\tau}$ which have a quantum-theoretical meaning", these $|C_\tau|^2$ would have to be replaced, say by $\Gamma(n',n) = \Gamma(n,n')$ which, he thought, could presumably be derived from the quantum theory much as the C_τ were from the classical theory.

Applying his new quantisation procedure to the perturbation energy, Born obtained

$$W_2^{\text{quantum}} = -\frac{1}{h}\sum_{\tau_k>0}\left\{\frac{\Gamma(n+\tau,n)}{\nu(n+\tau,n)} - \frac{\Gamma(n,n-\tau)}{\nu(n,n-\tau)}\right\}$$

, from which Kramers' dispersion formula followed immediately. Finally, he established a mathematical analogy, for a very simple case, with Heisenberg's 'new' formulae for the anomalous Zeeman effect.[68]

These early applications of the virtual oscillator concept raise several questions, as yet unanswered by historians. How did Kramers arrive at his dispersion formula? Did his derivation, using the new quantisation procedure, precede Born's work, follow it, or develop independent of it? How did the concept of difference equations arise, and how did Born put together the several ingredients of his new theory? Finally, how did these advances relate to more general conceptual developments, and how did they develop the new virtual oscillator concept?

In attempting to answer these questions, we now come up against a problem that is almost unique to the history of this century, that of group research. We have already noted the close communication during the crisis period between the physicists concerned with the failure of the old quantum theory, and we were then able to use it, to our advantage, to assert that ideas emerging during that period were familiar to all members of the community. There is a corresponding disadvantage, however, for the communication hinders any attribution of the inception or original expression of ideas to particular individuals, and it is this aspect of the situation that comes to the fore from 1924 onwards. In 1924, Bohr and Kramers in Copenhagen, Born in Göttingen, and Heisenberg, who oscillated between the two centres, all worked with the virtual oscillator concept.[69] The two university departments were exceptionally close; there was a heavy correspondence between the physicists and, unfortunately for the historian, considerable direct contact. Moreover, the group of physicists concerned effectively included Pauli, who was in close correspondence with Heisenberg and Bohr, who visited Copenhagen, and who was apt to meet enyone travelling between the two universities when they broke their journey in Hamburg. The five physicists whose contributions were of most significance at this stage of the development of quantum concepts were thus in virtually complete contact, with the result that Born's paper, for example, developed a new idea of Kramers' and acknowledged the help of both Heisenberg and Bohr.[70]

In addition to the closeness within the group, there was also considerable verbal interaction between members of the group and interested outsiders. In Göttingen, Born was very close to Franck's experimental department and he also held a regular seminar attended by both mathematicians and physicists, many of outstanding ability; at this time Fermi, Jordan, Hund and Cariogotto were all regularly present, and they also all attended Hilbert's seminars on axiomatics. While Göttingen was a Mecca for mathematicians, Copenhagen attracted physicists, and when Bohr wanted to sound out his emergent ideas he was apt to draw on anybody he could get hold of as an audience. With quantum theory in a stage of transition, with new ideas forming but with no-one quite sure what they meant or where they might lead, the discussion was intense and speculative. Ideas floated about very much 'in the air', sometimes emerging clearly from one individual, but more often than not common property and not directly attributable. When van der Waerden studied a similar problem to our present one, that of the origins of the spin concept in 1925,[71] both Goudsmit and Uhlenbeck reacted very strongly against what they saw as "microhistory", and with some justification.[72] Uhlenbeck complained that van der Waerden left out the "irrational" part of the history, and that he took no account of the fact that the idea of spin was very much "in the air". Concerning our present problem, Heisenberg in his recollections made much the same point, putting great emphasis on the atmosphere and on the joint ownership of ideas, and claiming that it simply did not matter who wrote what.[73]

As a historian I shall of course try to answer the questions that raise themselves, but to impose order on a situation where there was none would only distort the reality; any analysis I give is not to be taken as a 'causal' explanation of the developments concerned, but only as a partial illumination of these developments.

Kramers' outlined derivation of his dispersion formula was completed before and published after the publication of Born's paper, so the issue of priority is open.[74] The fact that the formula itself preceded Born's work suggests that Kramers might have priority, but it seems likely that his derivation was in fact dependent upon Born's work. The striking absence of any derivation in his first paper suggests that none existed, the formula being an educated guess justified in terms of the CP, and this is very likely: Heisenberg recalled that it was a result of inspired guesswork, arising from a combination of Bohr's ideas and the work of Einstein and Ladenburg,[75] while Slater, in his recollections, was convinced that it predated the new virtual oscillator concept.[76] This view is supported by Kramers' first paper, for the virtual oscillator concept was introduced there only by way of a visualisation, the argument leading to it being expressed purely in terms of the CP.

Kramers had been concerned late in 1923 with the CP, and is likely to have looked at dispersion from this point of view and to have seen that, in order to satisfy the CP and take account of the full range of Eintein's transition probabilities, another term had to be added to Ladenburg's formula. As he explained in the first paper, Ladenburg's formula was suitable for atoms in their normal (ground) state, when no stimulated emission was possible, but in general this emission required the extra term. We may presume that when Kramers got round to publishing his new formula, after the BKS development, he simply added the new visualisation to his account. That the derivation of the second paper was not strictly original is suggested by the fact that he still did no more than sketch its outlines, and by the fact that in the form presented it was far from adequate. His substitution was far from obvious and could have been derived only as by Born, through consideration of the quantum equivalents of the $|C_\tau|^2$, or else from the physical considerations behind his first paper. If Born's procedure were not used, then only for the case of a large number of atoms could the emissions and absorptions be paired off, a ν^a corresponding through choice of τ to a ν^e ($\nu(n+\tau,n) = \nu(n,n-\tau)$). Before the BKS development, Kramers had emphasised that the CP had to be applied to a large number of atoms, but in the wake of this development he was supposedly considering "an atom in a stationary state", in which case the absorptions and emissions could not be paired. In this case, a derivation such as Born's was needed, but Kramers gave no hint of such. We may therefore conclude that although the outline of the derivation probably dated from 1923, and from the context of a large number of atoms, its justification in the BKS context could only follow Born's work.

Born's theory rested on three basic supports: the idea that the use of virtual oscillators could be extended to matter-matter interactions, the new difference quotient quantisation rule, and a perturbation result in a form that was suggestive in the light of the new rule. It is not immediately clear which of these ideas were original, which followed which, or whether the crucial use of difference quotients followed or preceded the connection between classical and quantum frequencies with which it was linked in the paper; I shall therefore look at all these points, starting with the general question of difference equations.

The first suggestion that difference equations might be used in quantum theory was due to Poincaré back in 1911,[77] but there is no indication whatsoever of any link between this suggestion and their actual use in 1924. In this context their introduction is usually attributed to Born, but the situation is not that clear cut, for although Kramers' use probably depended on Born's (and even this is not certain) they were also used by Heisenberg, whose paper containing a "new quantum principle" was received on the same day as was Born's paper. We may recall that Heisenberg's new principle,

$$H_{quantum} = F(J+\tfrac{1}{2}) - F(J-\tfrac{1}{2}) = \int H_{classical}\, dJ \,,$$

was in fact conceived in October 1923, out of consideration of the g-factor problem in the core theory of the atom. He did not submit a paper on it for many months, as he tried in vain to deduce from it the use of half-integral quantum numbers, but he was working with Born in Göttingen when their papers were submitted and even helped with Born's paper. We must therefore attribute the first use of difference equations, albeit in a much cruder form than by Born, to Heisenberg: but where did he get the idea? Bohr's formula for the quantum frequency, $h\nu = E_1 - E_2$, was of course a primitive difference equation, and it may have been seen as such in about 1923, after a seminar attended by Heisenberg. This was one of the mathematics seminars at Göttingen; it was given by Courant and Siegel, and was entitled "Uber Differenzgleichungen".[78] The g-factor problem with which Heisenberg was primarily concerned led not only to half-integral quantum numbers but also to the replacement of j^2 by $j(j+1)$, and this may also have been very suggestive in the light of difference equation theory, leading Heisenberg to his new formalism.

However it arose, the difference equation approach would have been familiar in Göttingen the following spring, and some connection with Kramers' dispersion formula would also have been clearly apparent; the problem remained only of how and where to apply this approach. Heisenberg recalled later that this was so, and that the problem had been discussed in seminars, with vague suggestions being made that at such and such a point, where one had the operator $\partial/\partial J$, one really should have a difference equation.[79] Discussion was general and active in a situation that was described later by Born as a collective "grope" towards a new theory,[80] and somehow Born's paper emerged.

Exactly how this happened we cannot be sure, but we can reconstruct the main elements with a fair degree of confidence. The use of perturbation theory was natural to Born, who had used it for virtually all of his work on quantum theory. The 1924 version differed from previous ones in that it was based on a Fourier series expansion,[81] but the analogy between such an expansion, in terms of the classical harmonics, and the virtual oscillators was a close one and must have been fairly apparent, so that while the Fourier treatment had been quite inappropriate in the context of the orbital model its use in the new context would have been perfectly natural. Thus the classical perturbation formulae would have fallen naturally into the suggestive form that they did, and this form, together with that of Kramers' dispersion formula and the idea of difference equations would have led Born to the rough form at least of the quantisation rule required.

What about Born's discussion of the relation between classical and quantum frequencies, the suggestive form of which that he obtained seems to have been quite new? Jammer suggests that this was in fact at the root of the whole theory (the use of difference equations following from the correspondence between $\frac{1}{h}\partial H_0/d\mu$ and $\frac{1}{h}[H_0(n+\tau) - H_0(n)$)[82], but although this is very reasonable on the surface I can see no reason why Born should have been thinking in terms of frequencies. He is likely, from what we know, to have been thinking primarily about virtual oscillators and Kramers' dispersion formula, with difference equations also on his mind. Since his delight in the virtual oscillators was apparently due to their promise of a return to the classical theory, he is likely to have started out with the classical dispersion theory, using pertubation theory as was his wont and the Fourier series as I have suggested. The required quantisation would then have been apparent in outline from a comparison between the quantum and classical results, and in trying to justify and generalise the use of difference equations in this quantisation he may well have been led to consider the frequency relation; at any rate, this process seems far more likely that the reverse.

Finally, concerning the extension to matter-matter interactions, it is not clear whether this came after Born had evolved his quantisation rule, or whether it acted as a positive stimulus as his paper suggests, but either way it is easy to see its origins. Heisenberg's work on the anomalous Zeeman effect had made the need for such an extension painfully obvious, and the attempt to link up the new theory with this work was natural considering Heisenberg's presence and involvement.[83]

With Born's paper, and the discussions that accompanied and followed its generation, the concept of virtual oscillators was pushed aside slightly to make room for the new difference equation quantisation rule. This rule was seen as the foundation for the long sought-after new mechanics, and Heisenberg wrote to Landé that the nicest thing about the virtual oscillator theory was that one had now, with Born's calculations, some idea of what this new mechanics would look like,[84] while to Pauli he wrote that the difference equations were the key to the whole thing.[85] The virtual oscillators remained as unsatisfactory as when first conceived (when Heisenberg had been unenthusiastic about them), but now that a successful mathematical theory was stemming from them they became at once more readily acceptable, in a formal sense, and less significant, in a structural sense. Already Kramers, in his second paper, had talked of them as being "meant only as a terminology",[86] and it was now possible to treat them as such, and to return to the behavioural and positivist approach of the CP. This was in fact necessary for their development, for the problems that they entailed if taken in any stronger sense could only become more apparent as their implications were explored. In this context one problem that we have not yet considered was the distinction between two completely different sets of oscillators, one for absorption and one for emission. This distinction was necessary for the consideration of a single atom in a given state, when the absorption and emission frequencies did not coincide, but it was conceptually unsatisfactory, the more so since the emission oscillators corresponded to e^2/m for the equivalent classical oscillator being negative; this characteristic, later to be incorporated in the theory of holes, defied physical conceptualisation. Born brought the absorption and emission oscillators together, writing (as a result of his difference equation approach) $\sum$(absn. terms - emn. terms) where Kramers had written $\sum$(absn. terms) $-$ $\sum$(emn. terms), but if this mathematical form were to be interpreted physically the condition had to be imposed that the oscillators should exist: an atom in a given state could have only finitely many emission oscillators, but infinitely many absorption oscillators, and this was not reflected in the mathematical theory. Born insisted, in recognition of this, that "the two kinds of resonator have a different behaviour",[87] a feature to which Pauli strongly objected; on the one hand resonators that only emitted or only absorbed presented difficulties, while on the other hand a mathematical theory that did not reflect this necessary state of affairs was obviously unsatisfactory.

The dispersion theory of Kramers and Heisenberg

Following Born's work, the development of the new theory continued in both Göttingen and Copenhagen. Heisenberg, having been involved with Born's work, went to Copenhagen for the winter of 1924-5 and worked there with Kramers on an extension of the dispersion theory,[88] and with Bohr on the application of the virtual oscillator approach to the problem of fluorescent polarisation. The work with Bohr was very important for Heisenberg's own development but it broached completely new problems, and I shall therefore consider it it in the next section; that with Kramers was more straightforward.

Kramers had assumed that an atom under the influence of coherent incident radiation, frequency ν, was a source (as in the classical theory) of coherent spherical waves of the same frequency, $\Re(Be^{2\pi i\nu t})$, ...[*]
and also of waves corresponding to possible transition processes,
$\Re(\sum_q a_q e^{2\pi i \nu_q t})$, where ν_q were the transition frequencies and a_q were related to Einstein's transition probabilities; the interference between the waves gave the dispersion formula. Although it was well known that, classically, the atom was also a source of incoherent waves, these were usually ignored in approximation and had been so by Kramers, but Smekal had introduced emissions of this type, supposed to result from possible changes in the state of the atom during the scattering process.[89] The effect was that [*] was replaced by the more general formula, $\Re\{Be^{2\pi i\nu t}+\sum_k B_k e^{2\pi i(\nu+\nu_k)t}+\sum_l B_l e^{2\pi i(\nu-\nu_l)t}\}$.
[ν_k; ν_l transition frequencies]

Smekal thus generalised Kramers' formula, but he did it on the basis of the light-quantum hypothesis, not on that of the virtual oscillators, and Bohr naturally felt the need to reproduce his results on a wave-theoretical basis. Heisenberg and Kramers did this and they also managed to show that yet another term should be included, of the form $\sum_j \beta_j e^{2\pi i \nu_j t}$.

According to their paper, the idea of extending the theory beyond Smekal's results was due to Kramers, and Heisenberg recalled that the redaction of the paper was also. But Kramers still apparently thought of dispersion in purely physical terms, guessing the quantum equivalent of the classical form, and Heisenberg had to emphasise the importance of a rigorous argument.[90] The extension of the theory required was straightforward, but the paper had one very interesting feature: the phrase 'virtual oscillator' was not mentioned in it at all. The authors based their argument on the CP, referring to the BKS theory only as an example of how this could be applied, and then discussed the problem in terms of classical, apparently 'real' waves. They argued that 'the atom emits waves' rather than that 'the atom acts as if it emits waves', and they emphasised that the

> "requirements for a wave-theoretical description of optical phenomena remain satisfied, even when the momentum changes of the atom for the transition are not neglected." [91]

In part this must have been a conscious response to Smekal's use of the light-quantum concept, and in part a consequence of Kramers' physical way of looking at things; his apparent dissatisfaction with the virtual oscillator concept might also have been relevant though, for the terminology indicates a return to the CP as theme, utilising the achievements of Born's theory but without using the now redundant and unsatisfactory virtual oscillator concept. The authors do not seem to have been advocating a return to the classical picture (as the terminology might suggest) , for although they declined to discuss the "curious fact that the centre of these spherical waves moves relative to the excited atom",[92] they were well aware of the problems entailed in this and other features.

Born and Jordan

In Gottingen in the spring of 1925, Born and his assistant Jordan also developed the new theory, this time into the realm of non-periodic phenomena.[93] They set themselves the problem of "examining systematically the correspondence relations for non-periodic models",[94] and were obviously well pleased with the results, which included a derivation of Planck's law:

> "Many of them will have been found already by other authors in other ways, but still we note, in the prospect of wider applications, that our derivations are not only simple but general." [95]

The essence of the treatment, which was also examined independently by Dirac,[96] was simply to replace the Fourier series with Fourier integrals in the application of the CP. There were convergence problems, but the development was essentially technical, and of little conceptual importance, especially since it was already out of date on publication, as a result of Heisenberg's work. It is to the immediate origins of this work that we must now turn.

III.3. THE BACKGROUND TO HEISENBERG'S NEW KINEMATICS

It is important to realise that although the BKS theory put forward a virtual oscillator model of the atom this did not replace the orbital model, which was still necessary for the derivation of the stationary states and transition frequencies. The contradiction between the orbital model and the CP, apparent to Heisenberg and Pauli if not to Bohr (who avoided it by emphasising the "formal nature" of the whole theory), was emphasised but not resolved.

Pauli's exclusion principle

Of those who had been concerned with both aspects of the theory, the most critically aware of the problems it faced was probably Pauli. He had never really believed in the electron orbits, and he had suggested that individual electrons acted each as a complete set of oscillators, anticipating the BKS theory and Born's development of it; but he realised that the orbital model was still absolutely necessary and, while others developed the virtual oscillator theory (which he himself rejected), he continued to explore this model. As we shall see, both the problems it entailed and, especially, his reaction to them still had a crucial role to play in the development of quantum theory.

We may recall that, in 1923, Landé had noted a formal analogy between optical theory, then based upon the core model of the atom, and the theory of the fine structure of X-ray spectra, explained in terms of the relativity effect.[97] In the spring of 1924, he tried to apply the core theory to the problem of X-ray doublets, but met with only mixed success:[98] the core theory provided the required correction, but that due to the relativity factor could not be eliminated. Reluctantly, he tried the other way round, with similar results: the relativity effect worked splendidly for the complex structure of optical spectra, but only at the cost of abandoning the whole physical basis of Bohr's atomic theory. A battle developed between the advocates of the relativity effect (Pauli, Sommerfeld, etc.) and those of the magnetic core effect (mainly the Americans, with Heisenberg and Landé leaning that way), and the doublet riddle, well described by Forman,[99] was launched.[100]

Late in 1924, Pauli approached the problem with a view to destroying the case for the magnetic core effect. He was able to show that according to the core theory the Zeeman splitting had to depend on the atomic number, which it empirically did not, and he therefore tried assuming that the angular momentum and magnetic moment vanished for the core, transferring what had been the core effect to the valence electron.[101] Instead of counting the core twice in its contribution to atomic properties (as had previously been done), he suggested counting the electron twice:

> "According to the interpretation suggested here, Bohr's 'Zwang' does not manifest itself in a violation of the permanence of the quantum numbers in the coupling of a series electron to the atomic core, but only in a characteristic Zweideutigkeit in the quantum-theoretical characteristics of the individual electrons in the stationary states of the atom." [102]

Soon after having reached this conclusion, Pauli read a paper by Stoner containing a new scheme for the shell structure of the atom, different from Bohr's but in excellent agreement with experiment and completely natural.[103]

One of Stoner's innovations was to assign values of j to each electron, rather than treating j as part of the electron-core interaction; this gave each electron a set of three values (n,l,j) and he found that the number of electrons in each shell was equal to twice the inner quantum number of that shell, $2(j+\frac{1}{2})$. Pauli saw from this that the whole shell structure could be obtained very naturally by giving each electron a fourth quantum number (m_j; $-j \leq m_j \leq j$) and insisting that each state defined by a set of four numbers (n,l,j,m_j) could represent, or be occupied by, only one electron. The Zweideutigkeit was absorbed into these quantum numbers and the whole idea was expressed generally as his famous exclusion principle, that no two electrons should occupy the same state in an atom.[104]

Expounding the principle, Pauli wrote to Bohr that

> "It would be much more satisfying if we could understand directly on the grounds of a more general quantum mechanics (one that deviates from classical mechanics) that these cases do not come into consideration as stationary states",[105]

but he himself provided the first step towards such an understanding by expressing Bohr's Zwang as a general principle of physics rather than as a technical restriction on certain circumstances; and in doing so he cleared up the problem of complex structure enormously.[106]

The exclusion principle became an essential feature of quantum mechanics, but more important than the principle itself in our present context is Pauli's continued progress away from a structural model of the atom and especially, despite the context of his investigation, away from the orbital model. He wrote to Bohr twice in December expounding his new ideas, and the letters contain much of interest.[107] For "weak" people who needed the support of well defined orbits and mechanical models, he suggested that one could argue that electrons in the same orbits would crash, and such cases would therefore have to be excluded;[108] but he consciously avoided the use of such terminology in his paper, and he thought that the future would involve an abandonment of the orbital concept and, moreover, some fundamental changes in kinematic concepts themselves:

> "The relativistic doublet formula appears to me to show unquestionably that not only the dynamic concept of force but also the kinematic concept of motion of the classical theory shall have to undergo fundamental changes (it is for this reason that I have avoided entirely in my work the designation 'orbit').
> Because this concept of motion also lies at the base of the correspondence principle, its clarification is above all else worth the efforts of the theoreticians.
> I think that the energy and momentum values of stationary states are something much more real than 'orbits'. The (still unattained) goal must be to deduce these and all other physically real, observable characteristics of the stationary states from the (fixed) quantum numbers and quantum theoretical laws. However we should not want to clap the atoms into the chains of our preconceptions (to which in my opinion belongs the assumption of the existence of electron orbits in the sense of the usual kinematics), but must on the contrary adjust our ideas to experience."[109]

He thought that the solution to the whole problem would come through the simple case of the hydrogen atom, for he saw an analogy ("no doubt very childish") "between the exclusion of the case k=0,m=0 for the H atom [which had for some time been a puzzling factor] and my exclusion rule."[110]

In the context of the later development of the theory, these suggestions are remarkable. The solution was indeed to come through the hydrogen atom, through a new kinematics, and with the abandonment of orbits and the restriction to observables; ideas would be adjusted to experience, and everything would be deduced from quantum numbers. So what contributed to this development of Pauli's thought? In the first place, of course, it was very much a 'development' of his earlier ideas, being very much in the same vein as these only expressed more strongly and with greater assurance. He had already rejected the concept of electron orbits, and it was this that enabled him to consider the possibility of an electronic Zweideutigkeit in the first place: he had already considered the electron as a highly complex entity, and was not bound by the limitations of the traditional conception of a 'simple' particle. Led by Stoner's work to the exclusion principle, he naturally found this, as a general and simple principle, appealing, and it led him, as he said, to recognise the need for a new kinematics. This was closely linked with the complexity of the electron and it may well have been in his mind before, but he now had firm grounds on which to base it, for it was clear from his results that the motion of the electrons was controlled through a dimension outside that of the usual kinematics: the exclusion principle operates in the dimension of a fourth quantum number (or degree of freedom), whereas classical kinematics allows for only three dimensions.

Particularly interesting is the explicit reference to observable characteristics, apparently the first such in the context of quantum mechanics.[111] It has clear roots in Pauli's scientific thought, for in 1919, in his first paper on relativity theory, he had written that

> "one should accordingly hold fast to the idea that in physics only quantities which are observable in principle should be introduced.",[112]

but this was clearly a more natural statement in the context of relativity than in that of the old quantum theory. This draws attention to one interesting point, that, of the physical thinkers who played the greates part in the origins of matrix mechanics (namely Heisenberg, Pauli and Bohr), only Pauli had been involved with the basic problems of relativity theory. The idea of observability criteria was thus familiar to him in a way that it was not to the others; but observability in relativity was not necessarily the same thing as observability in quantum theory,[113] and it would be unwise to read too much into this. The reference in the letter to Bohr strongly suggests a definite underlying philosophical position, in which the property 'physically real' was equated with that 'observable',[114] and this may well have been the case, but again we cannot be sure. Pauli had adopted an explicitly phenomenalist approach to quantum theory before,[115] but this was justified by practical positivism rather than by any epistemological or ontological claim, and although we know he did adopt a phenomenalist philosophy, we do not know whether he had already done so before the developments of quantum mechanics later in the decade. It has been suggested that he was influenced by Mach, who was his Godfather, but one could add that he was born a Jew, christened a Catholic, and was a self-confessed mystic: a study of the combined effect of such influences is beyond the scope of our present work. Probably the most revealing of these factors is his mysticism, for this trait, the value of which was well appreciated by Heisenberg,[116] may be linked with a consistent refusal to back up his ideas with logical argument.

Heisenberg on fluorescent polarisation

Heisenberg shared Pauli's interest in the problems of complex structure and returned to these problems in 1925, but late in 1924 he was still concentrating on the virtual oscillator theory and concurrently with his work with Kramers he considered its application to a new field, that of flourescent light polarisation. Concern with this subject followed some experiments by Wood and Ellett, who found that if a polarised light source was used to stimulate fluorescent resonance radiation from mercury the stimulated radiation showed a remarkable 100% polarisation in a magnetic field, while for sodium the figure was 60%.[117] These results stimulated a resonance publication on both theoretical and experimental aspects of the problem, and in late 1924 the most successful treatment was an ad hoc extension of the classical theory;[118] the situation being connected with that of the anomalous Zeeman effect, the orbital quantum theory ran into apparently insuperable problems with the statistical weighting of the stationary states, the natural assumption of equal weighting clashing with the requirement of spectroscopic stability. In the absence of a magnetic field, an unpolarised light source gave rise to unpolarised resonance radiation and one had to assume that the same was true in a magnetic field also, despite the fact that, with the more complex structure of the stationary states, this was not natural within the orbital theory.

In a short paper on the subject, Bohr insisted that the results did not contradict his theory of atomic structure, provided only that the "formal nature" of this theory was respected.[119] The pseudo-classical explanation, he claimed, could be linked to this theory by the CP, applied through the theory of virtual oscillators (which he used in explicit preference to the stationary state electron orbits as the basis for his treatment). When Heisenberg took up the problem from Bohr, his course had been largely plotted for him, but his approach was nevertheless quite different from Bohr's. For Bohr gave no sign of giving up the orbital model, whereas Heisenberg took a large step in that direction: together with Pauli he had already shown a tendency to treat the orbital model and CP as rival approaches (a tendency not shared by Bohr), and he now compared explicitly the effectiveness of the two approaches in the given situation, and found the former to be lacking.[120]

Heisenberg based the CP approach upon some work by Dorgelo, Ornstein and Burger,[121] who had been studying the problems of multiplet structure and the anomalous Zeeman effect by analogy with the classical theory. Consideration of this work led him to seek a solution of the statistical weighting/ spectroscopic stability problem by assuming that the multiplet degeneracy revealed in the presence of a magnetic field was a permanent feature of the atom, the multiplet states being present (though indistinguishable) even in the absence of a field. His use of this idea (and with it the suggestion of solving a problem in the absence of a field by introducing a field, applying the usual quantisation, and then setting the field strenght at zero) seems to have led to an argument with Bohr, who criticised him for not stopping to ask whether it was compatible with the fundamental principles of the quantum theory,[122] but it was undoubtedly a major breakthrough in the treatment of complex structure. He introduced it in his fluorescent polarisation paper, together with the orbital model approach, in the context of the polarisation problem with unpolarised incident light and no field;

then, considering the introduction of a weak field, he used the new situation to decide between the two approaches. Classically, the resonance radiation remained unpolarised and this conclusion was carried through by the CP, being interpreted statistically as equal intensities of parallel and perpendicular polarised components. On the orbital theory, however, the field acting on the orbit would be expected to produce some polarisation effect and, as Heisenberg said, "we have every reason to assume that the polarisation is not present."[123] Having thus justified the use of the CP and rejected that of the orbital model, he reproduced the Wood and Ellett results for a polarised light source, obtaining through the CP predictions that were in full agreement with experiment.

In respect of the development of Heisenberg's thought, two features of his work on fluorescent polarisation stand out. The first is the explicit contrast of CP and orbital approaches, which had previously been restricted to quite distinct problem areas and had not therefore been contrastable. The CP had been applied to intensity problems and the orbital model to frequency problems: the problem of relative polarisation intensities was the first to be examined that fell within the scope of both theories, and Heisenberg made full use of this fact. He still could not abandon the orbital model, for it was still absolutely necessary for the calculation of frequencies, but he could now conclude that a new theory should be based upon the CP and not upon the orbital model. The second feature of interest is his treatment of the virtual oscillator concept. We have already discussed at length the problems that this entailed, and we have noted that Kramers had relegated it to a mere terminology, while Heisenberg and Kramers in their joint paper talked of the oscillators in a classical sense, justifying this in terms of the CP. Heisenberg on his own laid greater emphasis upon this justification than he did when working with Kramers, whose physical mode of thinking tempered Heisenberg's concern with the subtlety of the problem, and he stressed that the "virtual oscillators can only reproduce the motion of electrons in stationary states in a very symbolic way."[124] But this did not imply that they should be seen as less important than the stationary states, for

> "This circumstance [the symbolic nature] stands out particularly clearly when we have to deal with a degenerate problem, for then, as Bohr has indicated, the virtual oscillators approach a higher level of freedom than the stationary state motions. When it is shown in this way that the virtual oscillators determine the radiation of the atom independently, in a certain sense, of the motion of the electrons, one can see that the analogy of the virtual oscillators with classical radiation levels is often conveyed more sharply than is possible with stationary states."[125]

The symbolic nature of the virtual oscillators was also reflected in the status afforded them by Heisenberg, for although he used them as terminology (following Bohr's paper on the subject, perhaps) he indicated that he was really talking about the CP:

> "The aim of the following work is to show how various empirical rules on intensity and polarisation of spectral lines will be able to be comprehended as sensible sharpenings of the correspondence principle and how this viewpoint can be used to describe the quantitative results of experiments on the polarisation of fluorescent light."[126]

Heisenberg on the anomalous Zeeman effect

In the spring of 1925, intrigued no doubt by his fluorescence results, disturbed perhaps by Sommerfeld's recent discovery that the use of half-integral quantum numbers failed after all for helium,[127] and incensed by Pauli's treatment of the anomalous Zeeman effect, Heisenberg turned back to the last problem.[128] Since this was basically a problem of frequencies, he had no alternative but to stick to the orbital model, despite having decided against it in his paper on fluorescence. There was therefore limited scope of advancement of his ideas, but he was able to use the new problem crystallise them, and to put them into a clearer perspective. His response to Pauli's treatment was to shift the Zweideutigkeit halfway back whence it had come, to the core-electron interaction which he linked with Bohr's Zwang:

> "The electron acts on the atomic core through an unmechanical Zwang, which appears to double the stationary state of the core , or ...
> The atomic core acts on the electron through an unmechanical Zwang, which appears to double the stationary state of the electron." [129]

This was halfway between Bohr's theory and Pauli's, but it was more than a straight compromise, for by emphasising the interaction between core and electron, rather than the intrinsic properties of either, Heisenberg moved yet again towards the behaviour of the atom and away from its structure. Moreover, it was in terms of this behaviour (Bohr's "coupling mechanism") that Bohr had expressed the link between the CP and the orbital model. Heisenberg in fact considered the possibility of applying the CP to the Zeeman intensities in this paper, and was able to draw an important programmatic conclusion, for

> "The application of the correspondence principle to the derivation of the selection rules and intensities is legitimately only possible through possession of unequivocal mechanical models. Concerning the calculation of line intensities, corresponding to the jumps of the electron, we shall have to seek the Fourier components of the motion of the electron in that scheme in which the outer electron is unambiguously defined." [130]

The choice of an atomic model, he implied, should be made by working backwards from the CP, and although he admitted that this was very difficult, and he could not go far towards it himself, he thus formalised the conclusion that he had been led to by his earlier work on fluorescence.

The above quotation reveals Heisenberg's objection to Pauli's treatment, namely that in giving the electron an element of ambiguity this stood in the way of the application of the CP. In expounding his ideas, Pauli had expected little sympathy from physicists, "least of all" from Heisenberg,[131] and in the latter point at least he was quite right; but despite this, the general tendencies of these two physicists, and their ultimate aims, were much the same. Pauli had explained in a letter to Bohr (which he knew would be read by Heisenberg) that his ideas were a necessary nonsense:

> "But I think that what I am doing here is <u>no greater</u> nonsense than the previous conception of complex structure. My nonsense is conjugated with the nonsense which has been customary. Just for that reason I believe that this nonsense is necessary in the present state of the problem. That physicist who succeeds in adding these two nonsenses will get the truth."[132]

Heisenber replied that

> "Today I have read your recent paper [enclosed with the letter] and sure enough I am the man who most of all <u>rejoices</u>, not only because you have pushed the swindle to a previously unsuspected height and thereby have easily broken all previous records, for which you revile me (in that you

> suggest single electrons with 4 degrees of freedom), but above all I rejoice in the triumph that you too (et tu Brute!) are returned with a bowed head to the land of formal philistines. But do not be unhappy. You will be welcomed with open arms. And if you think you have written something against the previous sort of swindle, that is naturally a misunderstanding, because swindle times swindle gives nothing correct -- therefore two swindles can never contradict each other. So congratulations!!!!! Happy Christmas!!"[133]

Thus Pauli wrote of his own nonsenses, and Heisenberg included himself among the formal philistines. Both were turning to a more formal, symbolic approach, and still trying to move away from the orbital model of the atom.

Collision effects and matter-waves

While Heisenberg was investigating the anomalous Zeeman effect, his colleagues at Göttingen and Copenhagen were generally more concerned about some very important developments bearing on the wave-particle problem and the theory of collision effects. Two collision effects in particular occupied the attention of physicists. In the BKS paper, Bohr had included a footnote[134] on Ramsauer's results on the scattering of slow electrons by atoms,[135] and these continued to trouble both him and Franck; Born and Franck meanwhile had become interested in the results of Davisson and Kunsmann on electron scattering by crystals.[136] The Ramsauer effect was the first example of the barrier penetration phenomenon in quantum physics, a slow electron incident on an atom appearing sometimes to pass right through it, without undergoing any change in motion, and as if the atom offered no resistance; Davisson's results showed a periodic variation of scattering intensity with scattering angle, to be interpreted eventually as electron diffraction.

The virtual oscillators had proved a very useful conceptual tool for periodic phenomena, but Bohr saw that the test of a quantum theory lay in its ability to explain collision effects such as Ramsauer's, and early in 1925 he concentrated upon this problem. What bothered him was that there was a clear departure from the normal expected behaviour of particles, but it was difficult to see where such a departue could lie: there seemed to be no doubt that the collision processes preserved strict energy and momentum conservation, but a departure from these laws, analagous to that of the BKS theory, seemed to offer the only hope of a solution.

Collision effects thus posed a problem, and one that occupied Bohr's mind, but there was an even greater problem on the horizon, for the experiments by Geiger and Bothe (and also by Compton and Simon), designed to test energy conservation and the light-quantum hypothesis in the wake of the BKS theory, were nearing completion. As early as January 1, Born could write to Bohr that although Geiger and Bothe were not committing themselves everybody in Berlin was already talking of their work as a triumph for Einstein.[137] Bohr's conviction was still sufficient for him to speak out against energy conservation in a talk delivered on February 20,[138] but he knew what was coming and on April 18 he wrote to Heisenberg that he was prepared for all eventualities,

> "even for an acceptance of a coupling process in distant atoms. The costs of this acceptance are of course so great that they cannot be measured in the usual space-time description."[139]

He suspected that the results would be disastrous, but he was clearly well enough prepared for them to maintain his sense of humour; this was as well, for they were already in the post, in a letter from Geiger.[140] Three days later, Bohr replied to this letter:

> "I was completely prepared that our proposed point of view on the independence of the quantum processes in separated atoms should turn out to be incorrect....
> Not only were the objections of Einstein very unsettling; but recently I have also felt that an explanation of collision phenomena, especially Ramsauer's results on the penetration of slow electrons through atoms, presents difficulties to our ordinary space-time description of a nature similar to those presented by a simultaneous understanding of interference phenomena and a coupling of the change of state in sparated atoms through radiation. In general I believe that these difficulties so far exclude the maintaining of the ordinary space-time description of phenomena, that in spite of the existence of coupling, conclusions concerning an eventual corpuscular nature of radiation lack a satisfactory basis."[141]

The results effectively reduced the two problems (of radiation phenomena and of material collision effects) to one and the same problem, so that Bohr could console himself that, having one unsolved problem, he was no worse off than when he had one solved and another unsolved; it was also reasonable to argue as he did that, when even material particles displayed a tendency to go through each other, one could draw no naive conclusions regarding the nature of light. Still, he had to admit that the departure now required from the usual conceptions was even more radical than that he had been seeking so far, and we may imagine into what a turmoil of speculation he must have been thrown. This can only have been heightened by the presence of Pauli (on a short visit) who had long argued the need for something more radical, while condemning the virtual oscillator picture that had now been destroyed. Pauli had an unprecedented opportunity to press his point of view and he seems to have taken it, for Bohr wrote to Heisenberg that

> "Particularly stimulated by discussion with Pauli, I torture myself these days with all my strength, to accustom myself to the mysteries of nature and to attempt to prepare myself for all eventualities."[142]

The same day that he wrote to Geiger, Bohr also wrote to Franck, with whom he had already been in communication on the subject of collision effects:

> "I have long been intending to write to you again, for the uncertainty about the correctness of my reflections on collision processes, to which I gave expression in the postscript to my last letter, have been more and more strengthened since. There are especially the Ramsauer results on the scattering of slow electrons through atoms, which do not appear to fit in with the accepted point of view. Indeed these results must offer difficulties for our usual space-time description of nature of a similar kind to the coupling of transitions in distant atoms through radiation. But then there's no more ground to doubt in such a coupling and in conservation laws generally. This is scarcely very satisfactory for, as you stress, so many of the collisions would in that way become so extraordinarily simple. Also the thermodynamic conciderations of Einstein become very disturbing indeed. Already this morning I have written to Fowler to withdraw an English work on the slowing of α-rays, and will write the same to Scheel concerning the work he has outstanding. Moreover I've just now heard from Geiger that his experiments on the coupling have been decisive, and there is really nothing to do but take our attempted revolution as painlessly as possible into oblivion. We cannot, still, forget our goals that easily, and in the last few days I've been tormented by all sorts of wild speculations, in order to find an adequate foundation for the description of radiation phenomena. On this I've talked a lot with Pauli, who is still here, and who was long unsympathetic to our "Copenhagen Putsch"."[143]

Three days after this letter was sent, Born saw it and sent a reply[144] in which he wrote that Bohr's abandonment of the virtual oscillator theory had shaken him (as well it might) but that eight days previously he had started a letter to Bohr in which he came to the same conclusion. He had been travelling in Switzerland, which had given him time to think of things, and he too had concluded that the theory was impossible.

By April 1925, the virtual oscillator theory was dead, and the quantum theory of radiation once again presented an insurmountable problem. As a result of developments over the previous year it now shared this status with the theory of matter. That electrons were not the simple particles they were generally taken to be had long been suggested by Pauli; it had been a feature of the virtual oscillator theory, and it had been highlighted by Pauli's attribution of four quantum numbers to an electron and by his exclusion principle, which revealed a non-independence of the electrons analogous to that of light-quanta. A similar conclusion could be drawn from the Ramsauer effect, or from Einstein's thermodynamic considerations as referred to by Bohr.

Einstein's work followed a new derivation of Planck's law, by Bose in 1924.[145] This had entailed no great conceptual advance, but it had reduced the problem to the the essential hypotheses of light-quanta controlled by statistics (analogous to Planck's counting) for their distribution in phase-space.[146] The concept of localised particles interacting through phase-space (introduced by Bose apparently indepent of de Broglie's earlier work), together with the simple form in which it was expressed, appealed to Einstein, to whom the derivation was sent; he had himself been trying to reconcile the non-independence of light-quanta with their localisation, and although he would have preferred the interaction to take place through real space the simplicity of Bose's derivation (and the fact that it did not draw at all on the classical theory) compensated for this. Einstein also saw the possibility of applying Bose's method to the thermodynamics of an ideal monatomic gas, and this application,[147] which was successful, disturbed Bohr. Planck's law had been applied to the specific heats problem as early as 1907,[148] but only in terms of the resonators supposed to produce the black-body radiation; Einstein now applied the statistics of the radiation itself to the gas particles, and found that it gave empirically the correct results. This application was a tremendous success, in that it partly solved the longstanding problem of reconciling gas thermodynamics with the quantum theory, but it achieved this only at great cost. For matter as well as light became subject to the wave-particle dilemma.

This extension of the wave-particle duality to matter had first been suggested, without any real substantiating evidence, by de Broglie. I have already suggested that his ideas were behind the virtual oscillator theory, and they seem also to have been behind the reaction, in Göttingen, to the demise of that theory. In the letter to Bohr already discussed, Born described a "new" treatment being discussed in Gottingen:

"Our main point is to retain the value of the BKS theory: namely the emission of wave radiation <u>during</u> the stationary states. But now there are besides these the periods of the jumps which may be taken here as elementary processes. When one attempts to calculate generally the order of the processes in space and time, one must connect the stationary states with the wave

emission, the jumps with the light-quantum emissions."[149]
The wave, he went on to explain, carries the light-quantum, and creates interference effects by controlling the absorption of radiation by matter (not, we may note, by guiding the quantum).

Born's treatment could be seen as an adaptation of the BKS theory to take account of the now established (except in Bohr's eyes) light-quanta, but it is remarkably like de Broglie's theory, especially in respect of the rather unusual role attributed to the waves. Born did not attribute the theory to de Broglie, and we have no direct proof that he knew of his work until three months later, when Elsasser published a paper (from Göttingen) referring to it and when Born wrote to Einstein that "I now consider the wave theory of matter could be of very great importance",[150] referring explicitly to de Broglie. There seems little doubt, however, that he actually knew of de Broglie's work much earlier, for he recalled that Einstein had told him of the work, and if this was so it must have been soon after Einstein had received a copy of de Broglie's thesis from Langevin at the end of 1924; even if Born was introduced to de Broglie's work only through Einstein's second paper on the ideal gas (in which it was favourably reviewed), this was published in February and is likely to have reached Born's attention by April.[151]

Einstein had been strongly attracted to de Broglie's work, offering his opinion in the February paper that "I believe it involves more than merely an analogy."[152] Since, in adopting the hypothesis of light-quanta interacting through phase-space, this work was closely related to Bose's, since in applying the same treatment to matter it paralleled Einstein's application of Bose's theory, and since the ideas of de Broglie and Einstein on the construction of light-quanta from ultimate wave forms were also remarkably similar, Einstein's appreciation of de Broglies work was quite natural. It must also have been influential, and it was natural that Born, who admired Einstein, who knew that the virtual oscillator theory was doomed but that something like it was needed, and who was still concerned with the mathematics of a theory rather than with any physical problems it might face, should be led to look at de Broglie's work. Bohr, however, replying to Born's suggestion, was less than enthusiastic:

> "The content of your note has naturally interested us, but I must confess that I do not believe that a frankly contradictory description of the phenomena can be reached in the proposed way. It seems to me that according to your image, the binding of the light-quanta with the waves is not close enough. On the one hand I do not understand how, according to your presentation, you can get the path of the light-quanta to coincide with sufficient accuracy with the propagation of the wave. If the interaction between a quantum and a scattering atom depends only on the classically calculated movement of its virtual resonators, it can scarcely be avoided that the quantum, e.g. by reflection or refraction, will be separated completely from the originally connected wave train. On the other hand it seems to me that your image can scarcely reproduce the quantitative relations of light absorption, for the suggestion, that the probability of the collection of a light-quantum through an atom should be proportional to the wave intensity, leads indeed to completely different conformity than would correspond to either the wave theory or the corpuscular theory of light. The rough agreement of these two theories, so far as the direct propagation of light is concerned, rests precisely on this, that the number of corpuscles which go through the plane surface is proportional overall to the wave intensity, and that therefore the absorption must be described through the assumption of a constant effective cross-section of the atom."[153]

De Broglie had by this time changed the role of the waves to overcome Bohr's objection on absorption,[154] but Born would naturally have worked from the theory as originally presented. In general the theory was too mechanistic for Bohr, and his own ideas on the subject, which followed, indicate the direction of his thought:

> "Wholly apart from the question of the correctness of such objections against your theory, might I willingly stress that I am of the opinion that the assumption of the coupling between stationary states in different atoms through radiation excludes a simple descriptive possibility of the physical situation with intuitive pictures. By the utterances in my letter to Franck about the coupling I meant only that I had come to suspect already that for collision processes such models had shown a more inferior application than usual. This is indeed chiefly a purely negative conclusion, but I feel, especially if the coupling should really be a fact, that one must then take refuge, in an even higher degree than so far, in symbolic analogies." [155]

Thus Bohr, like (and probably influenced by) Pauli, and also like Heisenberg, was moving towards a more symbolic treatment in his search for a solution to the problems of quantum theory.

As his letter to Einstein shows, Born was not put off the wave theory of matter by Bohr's response, and the theory was in fact much discussed in Göttingen that spring. A young physicist, Elsasser, had arrived in Göttingen the previous year wanting to work as an experimentalist with Franck; Franck, however, thought that he would make a better theoretician and in search of a suitable research topic they discussed de Broglie's theory with Born.[156] Elsasser was fascinated by this theory and in the course of the discussions he suggested that it would be nice if one could test it (he still wanted to be an experimentalist) by showing that electrons, as well as radiation, produced diffraction phenomena. At this suggestion, Franck suddenly realised that the Davisson-Kunsmann results, which were well-known to Born and himself,[157] could be interpreted as just this. Working from this idea, Elsasser showed that de Broglie's theory could indeed be applied not only to these results but also to those of Ramsauer, the waves circumventing a barrier as the particles could not.[158]

Summary

During the spring of 1925, Heisenberg again travelled between Copenhagen (where Pauli paid a visit at the same time) and Göttingen, and he must have been well aware of the crisis into which Bohr had been thrown, and of the type of theory being explored in Göttingen. In addition he knew of Pauli's suggestion of a new kinematics, and of his feeling that the solution to the quantum problem would be found through the study of hydrogen. He may have been influenced also by Pauli's phenomenalism, and the idea of an observability criterion seems to have been generally discussed: Bohr in his February 1925 talk used such a criterion to justify his emphasis upon interference phenomena and the wave nature of light, while the previous year Kramers had justified his dispersion formula on the grounds that it contained "only such quantities as allow of a direct physical interpretation"; there was as strong interest in relativity theory in Göttingen, and Heisenberg recalled that this had been linked with observability, which he claimed to have been a feature of his paper with Kramers; Born claimed the same of his paper with Jordan.[159] In addition to experiencing these influences, Heisenberg shared with Pauli the feeling that a more symbolic treatment of phenomena was needed (a feeling that could have been strengthened by the study in Göttingen of non-classical logics, following the Hilbert-Brouwer controversy)[160] and he had, finally, established a program of working from the sharpened CP (not itself disproved, though its application in the BKS theory was) towards a model of the atom.

III.4. HEISENBERG'S NEW KINEMATICS

Writing to Bohr in 1924, Pauli had discussed Heisenberg:

> "If I think about his ideas, they seem monstrous and I curse to myself a lot about them. Because he is so unphilosophical, he pays no attention to clear presentation of the basic assumptions and their relationship to previous theories. But if I talk to him, he srikes me as all right, and I see that he has all sorts of new arguments — at least in his heart. I therefore think of him — aside from the fact that he is also personally a very nice fellow — as very thoughtful, even a genius, and I think he will once again greatly advance science." [161]

As usual, Pauli's prediction was quite correct. In March 1925, as Heisenberg was completing his paper on the anomalous Zeeman effect, Pauli visited Copenhagen and the two physicists had a few weeks to discuss their ideas. Pauli presumably expanded on the ideas contained in his letters to Bohr the previous December, and the result seems to have been that, while retaining their differences of opinion, they recognised their points of agreement; the main difference seems to have been that Heisenberg still sought a model of the atom, whereas Pauli sought merely a symbolic scheme,[162] but Heisenberg recognised, as he indicated in his paper, the difficulty of his own aim, and set himself the task of deriving from the sharpened CP a suitable symbolic scheme, hoping to proceed from there to a physical model. He apparently arrived in Göttingen in April (as a new Privatdozent) armed with a book on Bessel functions, with which he hoped to improve the mathematics of the CP; he then intended, apparently, to "guess" the required scheme for hydrogen. In the third week in May, he switched his attention to the anharmonic oscillator;[163] at the beginning of June, having had trouble with hay fever, he escaped with Born's permission to the grassless island of Heligoland where, free from obligations and distractions, he quickly sorted out his ideas. Within five or six weeks he had completed his famous paper on "A theoretical reinterpretation of kinematic and mechanical relations",[164] the aim of which was

> "to establish a basis for theoretical quantum mechanics founded exclusively upon relationships between quantities which are in principle observable." [165]

The basic idea was to preserve the classical dynamical problem (as expressed in the equations of motion), but to change the underlying kinematics. Arguing that

> "It is well known that the formal rules which are used in quantum theory for calculating observable quantities such as the energy of the hydrogen atom may be seriously criticised on the grounds that they contain, as basic element, relationships between quantities that are apparently unobservable in principle, e.g. position and period of revolution of the electron ", [166]

he proposed to express such kinematical quantities in terms of the observable transition intensities and frequencies of the atom, as the quantum equivalents of classical Fourier series. Such expressions could not be related to the classical concepts of kinematic properties, for a single electron was treated as performing a whole range of independently quantised oscillations, but he argued that the frequency condition of the old quantum theory

> "represents such a complete departure from the classical mechanics, or rather (using the viewpoint of the wave theory) from the kinematics underlying this mechanics, that even for the simplest quantum-theoretical problems the validity of classical mechanics simply cannot be maintained." [167]

Heisenberg described his paper as a continuation of those on the virtual oscillator theory[168] and he began it by introducing the expression for radiation in that theory, describing it as the quantum equivalent of a classical Fourier expansion; $\mathcal{R}\{a(n,n-\alpha)\,e^{i\omega(n,n-\alpha)t}\}$, corresponding to the classical $\mathcal{R}\{a_\alpha(n)\,e^{i\omega(n)\alpha t}\}$.

Considering a general classical quantity represented by a Fourier series, he supposed that an equivalent quantum quantity was obtained in the same way as for radiation. The question then arose as to how one manipulated the expressions, especially as regards multiplication: one knew how to multiply two classical Fourier series, but for the quantum expressions the frequencies would behave oddly, and this would have to be taken into account. Thus, classically, $e^{\nu_1} \cdot e^{\nu_2} = e^{\nu_1 + \nu_2}$, but quantum frequencies combined differently: since $\nu(n, n-\alpha) = \frac{1}{h}\{W(n) - W(n-\alpha)\}$, they had to be added according to $\nu(n, n-\alpha) + \nu(n-\alpha, n-\alpha-\beta) = \nu(n, n-\alpha-\beta)$, and since one frequency had to 'start' where the other one 'ended' the order was important. If in a state n one had two quantum expressions, $x(n,t) \sim \sum_\alpha A(n, n-\alpha)\, e^{i\omega(n,n-\alpha)t}$ and $y(n,t) \sim \sum_\alpha B(n, n-\alpha)\, e^{i\omega(n,n-\alpha)t}$, one was forced to take for the product $x \cdot y \sim \sum_\alpha \sum_\beta A(n, n-\alpha)\, B(n-\alpha, n-\beta)\, e^{i\omega(n,n-\beta)t}$, which did not necessarily commute. The situation was the same if Fourier integrals replaced the series.

In the second part of his paper, Heisenberg looked at the dynamical problem and at the determination of the constants for periodic motion, which had been performed in the old quantum theory through the action variable, $\oint m\dot{x}^2 dt = \int p\, dq = J = nh$. Arguing that "the J are determined only up to an additive constant as multiples of h", he suggested replacing the condition by $\frac{d}{dn}(nh) = \frac{d}{dn}\left(\oint m\dot{x}^2 dt\right)$, or, for $x(n,t) = \sum_\alpha a_\alpha(n)\, e^{i\omega_n \alpha t}$, by $h = 2\pi m \sum_\alpha \alpha \frac{d}{dn}(\alpha\omega_n \cdot |a_\alpha|^2)$ in place of the old form $nh = 2\pi m \sum \alpha^2 \omega_n \cdot |a_\alpha|^2$ ... [*]. Following Born's quantisation procedure, this had a direct quantum equivalent,

$$h = 4\pi m \sum_{\alpha=0}^{\infty} \left\{ |a(n, n+\alpha)|^2 \omega(n, n+\alpha) - |a(n, n-\alpha)|^2 \omega(n, n-\alpha) \right\}, \quad \ldots [\dagger]$$

and armed with this Heisenberg successfully derived the energy levels of the anharmonic oscillator and the rotator.[169]

The differentiation by n was an interesting innovation, for it could hardly be legitimate: were n large enough to be treated as continuous the quantum constant h would not be significant anyway. The differentiation could be justified only be the result it gave, and that result was presumably why it was introduced. Given that [*] had somehow to be quantised according to Born's theory, we may surmise that Heisenberg introduced the differentiation as the only way of putting it into a form to which Born's quantisation procedure could be applied. This was the most natural procedure to adopt, and having obtained [†] Heisenberg would have been convinced that it was all right, for [†] was akin to the Thomas-Kuhn sum rule,[170] which had been derived by Kuhn in Copenhagen that spring, and which was itself closely connected with Heisenberg's work on fluorescence. He recalled, in fact, that

> "Then I saw if I write down this and try to translate it according to the scheme of the dispersion theory, then I got the Thomas-Kuhn sum rule. And that is the point. Then I thought, 'well, that is apparently the way how it is done'." (7)

But how was the rest of it done?

Having talked with Pauli in March, and having finished his anomalous Zeeman effect paper early in April, Heisenberg set himself the task of "guessing" from the CP a symbolic scheme for the reaction of hydrogen to an external field. His approach was to eliminate, for the time being, the concept of electron orbit and to work solely from the information that the behaviour was due to a single electron under a Coulomb force and that it was given by the observed transition probabilities and frequencies. This ties in with his conclusion to the anomalous Zeeman effect paper, with Pauli's suggestions as put to Bohr, and with a letter Heisenberg wrote to Pauli after completing his new theory in which he claimed that

> "all my wretched efforts are aimed at the complete elimination and suitable replacement of the concept of orbits which cannot be observed." [172]

This statement was made after the event, and Heisenberg's ideas were no doubt far less clear when he started his investigation, but his prime aim must have been to find a scheme for the derivation of transition probabilities in an external field from those in the absence of such a field, avoiding the intermediary of orbits. There seems little doubt that he originally hoped to deduce from this scheme a new structural model of the atom, but he put aside this aim for the moment. In effect he followed Pauli's advice of two years previously and treated the electron of the hydrogen atom as a set of oscillators, despite the fact that there was no room in the simple structure of the atom for a mechanical derivation of such a complex behaviour. Some progress towards this treatment had already been made in the virtual oscillator theory, but even in Born's hands this had still been concerned with complete mechanical systems, and the extension to a single electron was Heisenberg's (or Pauli's) first innovation.

From this first innovation followed a second; for whereas the BKS theory had treated the atom as a set of simple oscillators, Heisenberg, concerned with a single electron, treated it as a single but very complicated oscillator. In the case of hydrogen, however, it was far too complicated for him to be able to follow through his ideas, and he therefore switched in the second half of May to the anharmonic oscillator, and on June 5 he wrote to Kronig (the first sure evidence we have of his thoughts) outlining most of the mathematical content of the final theory.[173] In this letter, which supports the above analysis, he gave as the source of his inspiration that

> "one can really reduce all interactions between the atom and external influences to the transition probabilities",[174]

and asserted that

> "The basic idea is: in the classical theory it is sufficient to attribute <u>everything</u> to the knowledge of the Fourier components of the motion." [175]

In the new theory too, the transition probabilities and frequencies (Fourier components) might be interpreted as providing the sole description of the electron.[176]

If we were to interpret Heisenberg's first statement as meaning that all experiments on the atom boil down to the observation of transition intensities, we might see in it a consciousness of the observability criterion. But there was no explicit mention of this criterion, and it seems fairly certain that it arose as a later justification of the theory rather than as part of its original foundations. On June 24, Heisenberg wrote to Pauli that

"To myself everything is still unclear, and it is only by chance, I suspect, that it works, but perhaps the basic ideas are right. The basic statement is: From the computations must come only connections between the in principle observable quantities, of which the most important are energy, frequency, etc., .. ." [177]

Since Heisenberg had talked to Pauli only a week before, when he dropped in on his way back from Heligoland to Göttingen,[178] this suggests that the observability criterion as an explicit foundation for the theory was a new development: otherwise why tell Pauli of it? We may guess that the criterion may in fact have emerged from the discussion with Pauli; once it had emerged, it provided a clear physical justification for the theory, and naturally grew to dominance in the final presentation.[179]

As for the other central idea of this final presentation, that of a new kinematics, this also seems to have taken some time to emerge. It had not been formalised when Heisenberg wrote to Kronig, when he found "the physical meaning of the above sheme ... very strange",[180] but had not begun to work out this meaning. On June 24 he wrote to Pauli that

"Our theoretical viewpoints differ in so far as you regard the splitting as given and apply suitable oscillators to its representation; while I constantly try to retain the mechanics of the model." [181]

But in the same letter he also wrote that

"I would like to understand what the equations of motion really mean, when one treats them as relations between transition probabilities",[182]

and this suggests that the idea of a new kinematics was emerging, again perhaps from the discussion with Pauli. Five days later, Heisenberg wrote again to Pauli, reporting some progress and a new conviction that "this quantum mechanics is really right",[183] and after another ten days he sent him a draft of the final paper, reporting a growing conviction in his ideas, and the

"opinion that our interpretation of the Rydberg formula in the sense of spheres and ellipses in classical geometry has not the slightest physical meaning." [184]

The idea of a new kinematics was established and Heisenberg, his opinions getting "daily more radical",[185] had succeeded in his programme.

One final question we must ask in respect of Heisenberg's work is how it tied in with that on collision effects, and especially with Geiger's results. Apart from an unfavourable comment on Born's use of de Broglie's theory,[186] he made no comment on these topics either in his paper or in his correspondence, but he must have been well aware of the developments taking place. Those concerning the wave-particle duality of matter may well have encouraged him in his non-classical treatment of the electron, but they do not seem to have been crucial. Geiger's results had to be taken into account, but they did not pose a serious problem. He scorned Born's attempt to "reform radiation" (in terms of de Broglie's theory) as being too trivial, and this reminds us that he had never been that concerned about radiation: his attention had always been concentrated on matter. By April 1925, he was anyway much closer to Pauli in his ideas than to Bohr, and he had long treated the BKS theory as a visualisation of the CP, ignoring the physical elements of that theory. The insistence upon the wave nature of light had in fact been responsible for the division in the BKS theory between two models of the atom, and Heisenberg's achievemnt was to remove this division, and the non-conservation inherent in it. In his theory, the stationary states were derived by quantisation of a classical dynamical system (anharmonic oscillator or rotator), and energy was naturally conserved.

III.4. SHRÖDINGER'S WAVE MECHANICS

By late 1925, de Broglie's theory had had a somewhat chequered career. It had been adopted (if I am correct) by Slater and had thus led to the BKS theory; this itself failed, but it led fairly directly to Heisenberg's new kinematics. It had been reviewed favourably by Einstein (and in private also by Dirac) and it had been adopted in Göttingen, first as a possible replacement for the BKS theory and then for its application, by Elsasser, to collision phenomena. But Einstein did not develop the theory and Born, though he would otherwise probably have done so, was diverted by Heisenberg's work. However, Elsasser's work, published in the summer of 1925, must have made an impact, especially in the context of Einstein's earlier comments, and our analysis suggests that (contrary to the view expressed by Forman and Raman in their study of the subject)[187] it was fairly inevitable that someone should try and develop de Broglie's theory. In November 1925, Schrödinger wrote to Landé that[188]

> "It pleases me greatly to hear that your paper is intended to be a "return to the wave theory". I also am very much inclined to do so. Recently I have been deeply involved with Louis de Broglie's ingenious thesis. It's extremely stimulating but none the less some of it is very hard to swallow."

In some ways Shrödinger was in a unique position to develop de Broglie's theory. As recently analysed by Hanle he had been involved in debate with Planck over Einstein's recent thermodynamic considerations,[189] and some of his work had shown a striking similarity to de Broglie's interpretation of the Bohr orbits; in general, his interests (quantum statistics, theoretical spectroscopy, and relativistic mechanics) were remarkably close to those of de Broglie. Perhaps most important of all, he was a keen student of Hamilton's work, of which de Broglie's theory could be seen (and was seen by Schrödinger) as a development.[190]

Schrödinger retained his preference (expressed in connection with the BKS theory) for a return to the classical theory, but in 1925 he had come out of his Exner-inspired phase and had readopted a belief in strict causality.[191] As it stood, de Broglie's theory was far from being classical, but he saw in it the possibility of a completely classical treatment of quantum theory, a classical wave theory in which quantum discreteness was imposed by natural physical conditions rather than by the apparently arbitrary means of Heisenberg's theory. De Broglie had suggested that for the motion of an electron round a nucleus the wave should, so to speak, come back to itself in phase (i.e. after a whole number of oscillations), and Schrödinger re-expressed this by requiring that the atom should set up stationary proper vibrations; this requirement led to a familiar stationary integral eigenvalue problem. Drawing on Heisenberg's abandonment of electron orbits, the real existence of which he found to be "very much in question today",[192] he viewed a transition process as a switch from one proper vibration to another rather than as a jump between orbits, and he likened the situation to the occurrence of beats in acoustics, whose frequencies were equal to the differences between pairs of simultaneous characteristic frequencies in analogy with Bohr's frequency condition.

In his first paper on the subject,[193] Schrödinger started with the equations of motion in the Hamiltonian form, $H(q_i, \frac{\partial S}{\partial q_i}) = E$ where $S = \sum S_i(q_i)$, and substituted $S = k \log \psi$ to standardise the equation into a form that could always be transformed to

$$\text{Quadratic fn.}(\psi, \partial\psi/\partial q_i) = 0.$$

The problem was to find ψ such that the integral of the quadratic form was stationary: by then imposing reality, finiteness and single-valuedness upon ψ, and, as he emphasised, simply by this, the quantum behaviour was obtained. As an example he derived the Bohr energy levels of hydrogen.

In fact, the requirements generally necessary and sufficient for ψ were not worked out for many years, and that of reality, for example, was quickly dropped. All Schrödinger wanted in the present case was to eliminate the positive energy solutions for hydrogen (since without boundary conditions the problem had solutions for all positive energy but for only a discrete range of negative energies), and he chose the obvious boundary conditions to satisfy this requirement. The reason why this thoroughly classical treatment worked, for hydrogen at least, was that whereas classically one obtained an infinite series solution, the terms being related to the proper frequencies of vibration, the boundary conditions imposed restricted one to finite series solutions. There was thus a discrete spectrum of possible solutions, that in which the series terminated at the n^{th} term corresponding to the n^{th} proper vibration (combined, as Schrödinger noted, with all the lower ones).

Schrödinger's theory as presented in his first paper looked classical, but this was deceptive, for it had as yet no physical meaning: the symbols had no mechanical interpretation, and the theory was based, no less than was Heisenberg's, upon a symbolic scheme.[194] Schrödinger noted that his equation for hydrogen, $\nabla^2\psi + \frac{2m}{K^2}\left(E + \frac{e^2}{r}\right)\psi = 0$, suggested strongly that ψ should be connected physically with a vibrational process in the atom, and with one that "more nearly approached reality than the electron orbits",[195] but he decided to stick to a neutral mathematical form of exposition so as to bring out more clearly the origins of the discreteness in the theory. In his second paper in February, however, he did analyse the physical interpretation of his scheme, and the essential feature of this analysis was an analogy with optice. He suggested that classical particle mechanics corresponded to geometric optics and the new "wave mechanics" to wave optics, precisely as had de Broglie in fact, though this was not acknowledged. He pointed out that the analogy was well-known to Hamilton, writing that

> "The intrinsic (inner, spiritual) connection between Hamilton's theory and the process of wave propagation is neither small nor new. It was not only well-known to Hamilton but it also served him as the starting point for his theory of mechanics, which grew out of his Optics of inhomogeneous media."[196]

And he reacted to Hamilton's ideas precisely as had de Broglie, seeking an undulatory mechanics in analogy to the undualtory optics. The particles, he thought, would appear in a macroscopic view of his thory as wave packets of small dimensions;[197] he recognised the problem inherent in such a conception, that over a period of time the wave packets might spread out in space beyond particulate dimensions, but he hoped (in vain, as it turned out) that this could be solved.

As a result of his considerations, Schrödinger conjectured that the true mechanical process was given by the waves, and not by the image points of the wave packets, and he deduced that the theory must therefore be one of waves. The simplest wave equation of the second order was $\nabla^2\psi - \frac{1}{u^2}\ddot{\psi} = 0$,

where u was the wave velocity, and for a process dependent on time as $e^{2\pi i\nu t}$ this gave the equation he had originally assumed, namely $\nabla^2\psi + \frac{8\pi^2}{h^2}(E-V)\psi = 0$.
[V potential]

Schrödinger did not suggest a physical interpretation for ψ until a paper on the equivalence of his own and Heisenberg's theory, published between the second and third papers on his own theory, in March.[198] He then suggested that $R\{\psi \frac{\partial\psi^*}{\partial t}\}$ might correspond to the charge density. Only in the fourth paper on his theory did he discuss the subject at length, and by then he had related more simply to the charge density, $\rho = e\psi\psi^*$:

> " $\psi\psi^*$ is a kind of weight function in the system's configuration space. The wave-mechanical configuration of the system is a superposition of many, strictly speaking all, point-mechanical configurations kinematically possible. Thus each point-mechanical configuration contributes with a certain weight to the real wave-mechanical configuration." [199]

We shall return in the next chapter to Schrödinger's interpretation of his theory; for now we must ask how the theory arose, and what relation it bore to Heisenberg's. The first question is easily answered, for the vast majority of Schrödinger's ideas appear to have come straight from de Broglie.[200] Schrödinger simply replaced de Broglie's phase requirement by that of stationary proper vibrations; once he had thus brought the theory into the classical form he desired he had only to follow it through, this being a relatively easy matter in the new formulation. While de Broglie was aware of the quantum paradox and the subtleties of the situation, Schrödinger sought an essentially classical solution, ignoring (following Heisenberg) the complexities of electron orbits and thus reducing the theory to a simple workable form.[201]

Schrödinger found, in March,[202] a formal mathematical identity between his theory and Heisenberg's. Since this was related to the development of Heisenberg's theory as a matrix mechanics, which we have not yet discussed, we shall postpone the treatment of this identity to the next chapter, but the general physical relationship between the two theories is best looked at now. Schrodinger found the agreement between the two theories "very strange", in view of the fact that Heisenberg's appeared to aim at a discrete and his at a continuous formulation, and this verdict has so far been shared by historians. But from our investigation it is clear that the two theories had much more in common than is generally realised.

There was a basic difference in approach between the two theories, but it was not quite the difference suggested by Schrödinger; the mathematical formulations did differ in respect of continuity and discreteness, but this was not of prime importance. Let us first look at the similarities between the theories. Both theories rejected the idea of electron orbits, and both replaced the classical electron with a vibrational process; both, [illegible] on my construction, [illegible] both were developments of de Broglie's ideas (in Heisenberg's case through the virtual oscillator theory). Both theories resulted from attempts to return to the classical theory (in Heisenberg's case through the CP) and (a tenuous but fascinating link) both were developments of

Hamiltons work: Schrödinger's work was based on the optical-mechanical analogy, while Heisenberg's turned out to be based on matrix multiplication, matrices being a simplified generalisation of Hamilton's quaternions.[203] There has been some confusion in the past resulting from Heisenberg's being linked with Bohr, well-known to be opposed, especially on the crucial light-quantum question, to Schrödinger's ally Einstein. But, as we have seen, Heisenberg was by 1925 in a way closer to Pauli's conceptions than to Bohr's, and Pauli was, with Schrödinger and Einstein, a strong adherent to the light-quantum concept. Moreover Schrödinger and Bohr had themselves been in agreement at one time (on the BKS theory); in 1925, Schrödinger reverted to a belief in causality and conservation, but so did Bohr, coming much closer to Heisenberg and Pauli in the process.

Where Heisenberg and Schrödinger did differ was on the the very basic question of classical concepts in general. Schrödinger sought a completely classical theory, and tried to construct the peculiarities of quantum behaviour out of it — hence his continuous formulation. Heisenberg, on the other hand, accepted the need for an abandonment of classical concepts and so emphasised these peculiarities -- hence the discrete formulation. Schrödinger, and with him Einstein, saw light and matter as <u>both</u> wave-like <u>and</u> particulate, deriving one from the other. Heisenberg, and with him Pauli, saw them as <u>neither</u>; the concepts, they felt, were simply not applicable. Thus Schrödinger was repelled by Heisenberg's "transcendental algebra" while Heisenberg saw Schrödinger's work as a misleading delusion,[204] and both were quite justified in their criticiam. For Heisenberg seemed to abandon any hope of a structural visualisation, which in Schrödinger's classical ideology was simply 'not physics'; and Schrödinger claimed as classical a theory that was not, the wave forms in multi-dimensional configuration space being as difficult to visualise as were the elements of Heisenberg's equivalent theory.

III.5. DISCUSSION

The developments discussed in this chapter were very complicated and conceptually difficult and confused. In such a situation it is easy to lose sight of the main theme and in this last section I shall therefore highlight the aspects that were most significant in the context of the change from a classical to a quantum ideology.

Three elements of the BKS theory presented a clear and radical challenge to the classical ideology: the concept of an intermediate ontological status, the abandonment of energy conservation, and the abandonment of causality. But conservation was re-established by the Geiger-Bothe results in 1925 and the other two elements were almost unanimously rejected by physicists: even those who developed the theory did so in a spirit of positivism, taking the new technique independent of the radical elements. The introduction of these two elements was important, for they reappeared later when the new quantum mechanics came to be interpeted, but the conceptual context then was in some ways closer a theory of de Broglie's type than to the virtual oscillator theory, and the continuity seems to have been through Bohr's developing ideas rather than through the quantum mechanics in general.

In some ways, a more significant element of the BKS theory was a far less obvious one, namely that in his efforts to avoid a wave-particle for light Bohr introduced a form of duality for matter. He introduced a new picture of the atom based on the CP (corresponding to continuous radiative processes), but had to retain the orbital model (corresponding to discrete transitions). By introducing the radical innovations to which we have referred, and by ignoring problems such as the motion of the centre of the virtual oscillators relative to the matter with which they were connected, Bohr was able to argue that the two approaches were compatible (especially considering the "formal nature" of the theory), but this was not something that could be accepted by others. Born and Kramers simply relied on positivism to ignore this problem, but Heisenberg was more concerned with it and was led first to compare the two approaches and then to abandon (as he had been inclined to do for some time) the structural orbital model.

This dual model of the atom was not exactly a wave-particle duality, but it was closely related to such a duality, which itself became apparent at about the same time. First proposed by de Broglie, the wave-particle duality of matter became generally recognised through Einstein's application of Bose's radiation statistics to the ideal gas problem, through the problem of the Ramsauer effect, and through Elsasser's work on electron diffraction. It had also featured less explicitly, however, in Pauli's concept of a single electron as a set of oscillators, and this concept gained a new relevance when he saw from his work on the anomalous Zeeman effect (Zweideutigkeit, exclusion principle) that the classical kinematics of the electron would have to be abandoned. The need for an abandonment of classical mechanics had already been widely recognised, but this idea of a non-classical kinematics was apparently original to Pauli; it formed the basis for Heisenberg's new theory, and was one of the most important developments of the period.

Bohr's ideas on the rejection of causality and conservation did not survive in Heisenberg's theory, but the third of his suggestions during the crisis period before 1924, eclipsed in the BKS theory, did survive: this was the abandonment of a classical description of phenomena in space and time which, like Pauli's ideas, found expression in Heisenberg's new kinematics. This theory also contained a second departure from the classical ideology in the insistence (again apparently sponsored by Pauli) on an observability criterion. There was no hint as yet of a phenomenalist ontology, but the criterion was a clear statement of a phenomenalist methodology (with suggestions even of an epistemology), and a first challenge in quantum theory to the classical ideal of an objective world.

Finally, Schrödinger's theory should be mentioned. Schrödinger himself, reconverted to causality, clearly pursued a classical ideology, but his theory, being mathematically equivalent to Heisenberg's, naturally shared something of its radical nature. The wave packets turned out not to stay together (producing an essential wave-particle duality for both matter and light) and the waves were in multi-dimensional phase-space, again preventing a classical description in space and time. Although Schrödinger did not adopt an observability criterion he, like Heisenberg, rejected the concept of electron orbits and replaced them, also like Heisenberg, with a symbolic scheme, [illegible]
expressed [illegible]

IV.

THE DEVELOPMENT OF QUANTUM MECHANICS

IV.1. MATRIX MECHANICS

Born and Jordan

Born found Heisenberg's new kinematics fascinating and recalled that

> "Heisenberg had taken up the idea of transition amplitudes and developed a calculus for them, by following up the correspondence with the coefficients of the classical expression of a vibrating quantity into its harmonic components. ... Now Heisenberg suggested forgetting everything about the series and considering the set of coefficients which represented the physical quantity in question; then one has a multiplication rule for those sets of coefficients."[1]

He also recalled that he had discussed with Heisenberg and Jordan — before Heisenberg's innovations — the possibility that transition amplitudes "might be the central quantities and be handled by some kind of symbolic multiplication."[2] Heisenberg's theory was indeed based upon "sets of coefficients", and in his correspondence these were explicitly linked with transition amplitudes. The connection was only implicit in the paper however, and although much space was devoted to the new multiplication this does not seem to have beena central idea; the sets of coefficients were not clearly distinguished, moreover, from the summed series of the classical analogy.[3] Born's recollections therefore indicate his own perspective rather than Heisenberg's, and this perspective, combined with his mathematical approach, enabled him to re-express Heisenberg's theory explicitly as a theory of transition amplitudes and symbolic multiplication.

Born also clarified another crucial point of the theory, namely its concern with an atom[4] in a <u>general</u> state. The virtual oscillator theory had been concerned with an atom in a <u>particular</u> stationary state, a restriction that led as we saw to some difficulties. Heisenberg's theory did not suffer from this restriction, but although he noted that the two indices $n, n-\alpha$ of a coefficient $A(n,n-\alpha)$ were of equal standing, he did not emphasise this point, which remained obscured by his notation. By emphasising the role of individual amplitudes $A(m,n)$ (rather than the series or sum $\sum A(n,n-\alpha)$), Born may well have seen Heisenberg's innovation more clearly: instead of one index applying to a stationary state and the other to a transition, as in the old theory, both could be identified with stationary states, neither having priority over the other, and the atom being treated generally as a two-way-infinite set of coefficients.

This generalisation must have helped Born identify the amplitudes, as he did, with matrix elements, and the symbolic multiplication with ordinary matrix multiplication. In his recollections, he made much of this identification as the solution to a great problem, writing that

> "I began to ponder about his symbolic multiplication, and was soon so involved in it that I thought the whole day and could hardly sleep at night. For I felt there was something fundamental behind it. ... And one morning ... I suddenly saw the light: Heisenberg's symbolic multiplication was nothing but the matrix calculus, well known to me since my student days."[5]

Matrix multiplication is given by $C(n,m) = \sum A(n,r)B(r,m)$ as compared with Heisenberg's $C(n,n-\beta) = \sum A(n,n-\alpha)B(n-\alpha,n-\beta)$, and the connection may not have been obvious. But Born was very well aquainted with matrices indeed, and only the previous year his assistance had been acknowledged in the first volume of Hilbert and Courant's <u>Methods of mathematical physics.</u>[6] The first chapter of this famous Göttingen work was on matrices, and if Born was indeed trying to identify Heisenberg's multiplication, he would have found the identification more easily than the recollections suggest. He is more likely, however, to have been studying not the multiplication itself, but its meaning in terms of the behaviour of physical quantities such as position and momentum. This would account for the delay in his recognition of the matrices and would tie in with his recollection that, having identified the multiplication, he recognised "at once ... that the two matrix products pq and qp are not identical."[7] Although Heisenberg had suggested, in the light of non-commutation, that the formula for differentiation should be revised as $\sigma(t)\dot{\sigma}(t) \rightarrow \frac{1}{2}(\sigma\dot{\sigma} + \dot{\sigma}\sigma)$, he had not considered the implications for position, q, and momentum, p. Assuming that Born was thinking about these quantities when the light dawned on him, he would have seen immediately that the diagonal elements of $(pq - qp)$ were all $h/2\pi i$, for this was precisely Heisenberg's quantisation rule. He could not apparently prove that the off-diagonal elements were zero, but he soon convinced himself[8] that they were, obtaining the commutation relationship $pq - qp = \frac{h}{2\pi i}\mathbf{1}$.

Having put forward these ideas, Born retired to Switzerland "for a somewhat tiring health cure",[9] leaving Jordan to work through the elaboration of the ideas and prepare their joint paper, which was completed after his return and received on 27 September.[10] In this paper, Heisenberg's theory was expressed in matrix notation. In order to preserve Bohr's frequency rule (which required $\nu(nm) = -\nu(mn)$) and the reality of the transition amplitudes ($|q(nm)|^2$ for the matrix $q = q(nm)e^{2\pi i\nu(nm)t}$), the two-way-infinite matrices $A(n,m)$ that replaced Heisenberg's one-way-infinte series $\sum A(n,n-\alpha)$ were assumed to be Hermitian, and the commutation relationship, the frequency law and energy conservation were derived. In respect of the latter points, it was postulated that $h\nu(nm) = W(n) - W(m)$ with W arbitrary: W had then to be linked with the Hamiltonian energy H so that $h\nu(n,m) = H(nn) - H(mm)$ with H a diagonal matrix. Assuming that no transition was possible unless accompanied by radiation, $\nu(n,m) \neq 0$; $n \neq m$ (e.g. for a non-degenerate system), the time derivative of a matrix $q(nm)e^{2\pi i\nu(nm)t}$ was given by $\dot{q} = 2\pi i\nu(nm)q(nm)e^{2\pi i\nu(nm)t}$, and it followed that $\dot{q} = 0$ for q diagonal.[11] In particular, H diagonal implied that energy was conserved, $\dot{H} = 0$.

Following Heisenberg in preserving the classical equations of motion, Born and Jordan introduced the canonical form of these equations for the Hamiltonian $H = \frac{1}{2m}p^2 + U(q)$, namely $\dot{q} = \frac{\partial H}{\partial p}$, $\dot{p} = -\frac{\partial H}{\partial q}$; for a general Hamiltonian $H(p,q)$ they introdued the action principle $\int_{t_0}^{t_1}\{p\dot{q} - H(p,q)\}dt = \int_{t_0}^{t_1} L\,dt = \text{extremum}$. For a Fourier expansion of L and a sufficiently long time interval, they observed that only the constant term of L would contribute; since in matrix notation this term was the diagonal sum, $D(L) = D(p\dot{q} - H(p,q))$, it followed that the conditions for an extremum with respect to p and q were:

$$\frac{\partial}{\partial p(mn)} D(p\dot{q}) - 2\pi i\,\nu(nm)\,q(nm) = \frac{\partial D(H)}{\partial p(mn)} ,$$

$$\frac{\partial}{\partial q(mn)} D(p\dot{q}) = 2\pi i\, \nu(mn)\, p(mn) = \frac{\partial D(H)}{\partial q(mn)} .$$

To bring these into the canonical form Born and Jordan needed

$$\frac{\partial H}{\partial p}(nm) = \frac{\partial D(H)}{\partial p(mn)} \quad , \quad \frac{\partial H}{\partial q}(nm) = -\frac{\partial D(H)}{\partial q(mn)} \quad ,$$

and they adopted a "symbolic differentiation" for the derivative of one matrix with respect to another that gave them this result.[12] Again using this differentiation, Jordan showed that if $d = pq - qp$ then $\dot{d}$, expressed in terms of $p, q, \frac{\partial H}{\partial q}, \frac{\partial H}{\partial p}$, was zero: d was thus diagonal, and the commutation relationship followed directly from Heisenberg's quantisation rule.[13] From this relationship it followed that, for a Hamiltonian $H = \sum a_s p^s + \sum b_s q^s$, $Hq - qH = \left(\frac{h}{2\pi i}\right)\frac{\partial H}{\partial p}$ etc., giving $\dot{q} = \frac{2\pi i}{h}(Hq - qH)$, $\dot{p} = \frac{2\pi i}{h}(Hp - pH)$; from this it was easy to show that in general $\dot{g} = \frac{2\pi i}{h}(Hg - gH)$. Putting $g = H$ gave energy conservation, and the frequency law followed immediately.[14]

In the last section of the paper, Jordan noted Heisenberg's implicit assumption that $|q(nm)|^2$ determined the transition probabilities (Heisenberg had referred to these probabilities as being derived from the quantisation rule, which incorporated the coefficients of position) and proceeded

> "to see in what way this assumption can be based upon general considerations. An investigation of the question is necessary, since the fundamental equations of electrodynamics have to be reinterpreted in accordance with the new theory."[15]

Applying the new matrix mechanics to the electromagnetic field, Jordan found that the mean radiation (diagonal sum) was indeed determined by the $|q(nm)|^2$.

Born, Heisenberg and Jordan

Heisenberg made no contribution to the Born-Jordan paper, but he kept in touch with its developments and before it was even completed the three men were developing the theory further.[16] Heisenberg received details of the Born-Jordan formulation by September 13 and his first reaction was apparently to test its applications. Within a few days he had produced an incomplete derivation, based on 1st order perturbation theory, of Kramers' dispersion formula, and he included this in a letter to Pauli on September 18.[17] In this letter, the fundamental importance of the commutation relationship as a basis for the whole theory was also made clear for the first time; this may have been Heisenberg's own idea, or it may have been due to Born and Jordan, who noted in their paper that the canonical equations of motion and Heisenberg's quantum condition could be replaced as bases of the theory by the commutation relationship and energy conservation. Anyway, Heisenberg realised that any p, q satisfying the commutation relationship, and for which the Hamiltonian was diagonal, gave a solution to the perturbed problem, and he sought to derive such a p, q from the unperturbed solution, p_0, q_0. In analogy with the classical introduction of a canonical transformation S, he introduced a perturbation transformation function G, defined as canonical if it left $pq - qp$ invariant; since the unperturbed variables had, as solutions, to satisfy the commutation relationship, this ensured that the perturbed ones would also. The problem was then to find a G such that the Hamiltonian became diagonal, $H=W$. Expressing G in the usual way as $G=\lambda G_1+\lambda^2 G_2+\cdots\cdots$, Heisenberg wrote down the transformation for q, $q=q_0+\lambda(G_1q_0-q_0G_1)+\lambda^2\{\frac{1}{2}(G_1^2q_0-2G_1q_0G_1+q_0G_1^2)+G_2q_0-q_0G_2+\cdots$,... [†]

and similarly for p, for general functions $F(p)$, $F(q)$, and for H.
He noted that the terms could be expressed as iterated commutators, (Gq_0-q_0G), $\{G(Gq_0-q_0G)-(Gq_0-q_0G)G\}$, etc., and showed that the problem of finding the required matrix G could be solved in principle as in the classical theory, by calculating successively the G_1, G_2, ..., but he gave no derivation or origin for his expressions.

On September 13, however, in a letter to Jordan,[18] he had put

$$\left[\begin{array}{lll} H_0=\sum(a_sp^s+b_sq^s) & q=q_0+\lambda q_1 & H=H_0+\lambda H_1 \\ H_1=\sum\alpha_sp^s+\beta_sq^s) & p=p_0+\lambda p_1 & \end{array}\right]$$

and had derived $\dot{q}_1$, $\dot{p}_1$ by substitution (from $\dot{q}=\frac{2\pi i}{h}(Hq-qH)$ etc.) and, by considering powers of q_0, p_0,

$$[\dot{\mathfrak{F}}=H_1] \qquad q_1=\frac{2\pi i}{h}(\mathfrak{F}q_0-q_0\mathfrak{F})\ ;\ p_1=\frac{2\pi i}{h}(\mathfrak{F}p_0-p_0\mathfrak{F})\ . \ \ldots\ [*]$$

In this case, $\mathfrak{F}$ may be linked with the classical transformation S, for the classical solution is $W=H+\partial S/\partial t$ while here $H=H_0+\lambda\,\partial\mathfrak{F}/\partial t$. From [*] it is clear that $\mathfrak{F}$ was Heisenberg's later G, and his formulae [†] were therefore probably obtained in much the same way as were [*], i.e. by first working out the perturbation coefficient by coefficient and then generalising in terms of iterated commutators.

Whether independently or in reaction to Heisenberg's work, we do not know, but Born quickly produced a simpler transformation, $P=SpS^{-1}$, $Q=SqS^{-1}$, which also preserved the commutation relationship and which reflected his knowledge of matrix operators:[19] it was the standard form for a matrix transformation leaving the matrix equations invariant, and had indeed appeared as such in the mathematical introduction to the Born-Jordan paper. This transformation, which was accepted by Heisenberg as equivalent to his own,[20] formed the core of what was to be known as the three-man-paper.[21] The problem (first stated by Heisenberg and reformulated by Born) was as follows:

Given p_0, q_0 satisfying the commutation relationship, to find S such that $p=Sp_0S^{-1}$, $q=Sq_0S^{-1}$ gave $H(pq)=SH(p_0q_0)S^{-1}$ diagonal.
$[S=1+\lambda S_1+\cdots,\ p=p_0+\lambda p_1+\cdots,\ q=q_0+\lambda q_1\ldots,\ H=H_0+\lambda H+\cdots]$

Born left the applications of the theory, including derivations of Planck's law and Einstein's fluctuation formula, to Heisenberg and Jordan; he himself wrote a mathematical chapter (which we shall consider later), Heisenberg wrote a physical introduction, and they all worked together on the main body of the work. This included the extension of the theory, and thus of the commutation relationship, to several degrees of freedom, the extension being quite natural: the elements of different degrees of freedom were supposed to commute, while those of the same degree obeyed the original relationship. On October 28, Born disappeared again, this time to America, and the paper was finally completed by the others on November 26.

Pauli applied the matrix mechanics to the hydrogen atom[22] and, as Heisenberg commented in his introduction, the prospects for the theory's success were good. The formulation of Born-Heisenberg-Jordan also represented a considerable advance over Heisenberg's original formulation. Energy conservation was established as a general property (at least for non-degenerate systems) and the applicability to an atom in a general state was clarified. Above all, the theory rested upon clear foundations — either the classical canonical equations and Heisenberg's quantisation condition, or the commutation relationships and energy conservation. The latter foundations were particularly important, for although they still relied upon the classical expressions for energy they established the independence of the new theory from classical theory, while at the same time highlighting the difference between the two theories in a new and provocative way: for classical theory all elements naturally commuted, while for quantum theory one had the commutation relationship $pq - qp = \frac{h}{2\pi i}$.

Although this advance was considerable, however, it was also restricted. The problem, already posed by Heisenberg in respect of his original theory, was to determine what it all meant physically. What did the terms position and momentum mean in the new kinematics? What did it mean to say that they did not commute? How was one to interpret the classical energy formulae or equations of motion? And what did it mean when one introduced a quantisation rule or commutation relationship in which the basic element (quantum or commutator) was imaginary?

Disagreement and division

Heisenberg had originally adopted a symbolic approach to the quantum problem as a means to a physically meaningful end, and although he had sent Pauli a draft of his paper in the conviction that it was basically right he had at the same time written that

> "I am completely convinced of the negative part, but I hold the positive part to be insufficient and too formal." [23]

Pauli was naturally delighted with Heisenberg's work, which reflected in almost every aspect suggestions that he had himself made. Heisenberg could write that the two of them were

> "in agreement that even the kinematics of the quantum theory is entirely different from the classical", [24]

and Pauli that he felt far less lonely in his ideas than he had done six months previously; he wrote that

> "On the whole I believe that I am now close to Heisenberg in my scientific opinions, and that our opinions agree in everything as much as is possible on the whole in two independently thinking men." [25]

But this agreement went beyond the conviction in the theory's basic correctness; although Pauli had encouraged Heisenberg in his symbolic approach, thinking it to be the only way to break out of the chains of the classical theory, he now shared his desire for a physical interpretation of the scheme. Before turning to Jordan for assistance on the matrix mechanics, Born had asked Pauli if he would help; but, no doubt remembering his past experiences with Born's approach, Pauli had refused. It was, Born recalled, a "cold and sarcastic refusal", on the lines of[26]

> "Yes, I know you are fond of tedious and complicated formalism. You are only going to spoil Heisenberg's physical ideas by your futile mathematics."

Whether Born spoiled or developed Heisenberg's ideas is a matter for debate, but Pauli was probably not being sarcastic. He wrote to Kronig some months later that

> "one must next attempt to free Heisenberg's mechanics from the Göttingen formal teaching, and expose still further its physical heart!"[27]

Pauli's predictions tended to be uncannily accurate, and in predicting the course of matrix mechanics he was quite right: Born was completely carried away by the mathematics of the new theory, and apparently ignored its physical meaning.

This attitude led to a deep division of opinion at Göttingen, for Heisenberg was completely of Pauli's opinion; he wrote to him, after completion of the three man paper, that

> "I gave myself the whole problem of making the work physical, so that you and I would at least be half pleased with it. But I'm still pretty unhappy about the whole theory, and am so glad that you stand so completely on my side in your views on mathematics and physics. Here I'm in an environment that thinks and feels the exact opposite, and I don't know if I'm not just too stupid to understand mathematics!"[28]

In some ways, Heisenberg managed to put his viewpoint across quite well. He wrote a wholly physical introduction to the paper, and he persuaded Born to include a physically based form of matrix differentiation alongside the physically incomprehensible "symbolic differentiation" that had been introduced by Born and Jordan.[29] But he could not change the overall emphasis upon the mathematical formulation, and a division on this point was inevitable. In the letter quoted above, Heisenberg told Pauli that a general division was in fact developing in Göttingen between physicists and mathematicians. To date the three departments of theoretical and experimental physics and mathematics had worked in close harmony, but now, wrote Heisenberg

> "Göttingen splits into two camps, one which with Hilbert ... speaks of great successes, achieved through the introduction of matrix rules into physics, the other which with Franck says that we still cannot understand the matrices."[30]

Heisenberg himself recognised the success of the new theory, but he was nevertheless in Franck's camp on this issue, and so, outside Göttingen, were Pauli and Bohr. Their programme of research was completely different from that of Born and Hilbert, and while the latter developed the mathematics of quantum theory and largely ignored the physical problems it faced, the former concerned themselves entirely with these problems, first in respect of the theory's application to physical situations (such as Pauli's work on the hydrogen atom), and then in respect of the fundamental problems of physical interpretation. Ironically, however, these problems were too difficult for any progress to be possible, and for the time being the conceptual development of quantum theory followed the mathematical line of progress.

IV.2. BORN'S STATISTICAL INTERPRETATION

A mechanics of bilinear forms

By November 1925, when Heisenberg wrote to Pauli of the division at Göttingen, Hilbert was taking an active interest in matrix mechanics; discussions with Hilbert, or with Courant and the other Göttingen mathematicians, were presumably behind the third chapter of the three man paper, in which Born explored the mathematical possibilities of the new theory. Born's idea was to re-express the transformation of matrices in terms of bilinear forms,[31]

$$A(x,y) = \sum_n \sum_m a(nm)\, x_n y_m \quad \sim \text{ the matrix } \quad a(nm)$$

For Hermitian matrices $a(nm)$, the forms $\sum_n\sum_m a(nm) x_n x_m^*$ were real-valued, and a linear transformation $x_n = \sum \sigma(l,n) y_l$ sent $A(x,x^*)$ to $B(y,y^*)$ such that $b = \sigma a \sigma^*$ was also Hermitian. A transformation for which σ was orthogonal was defined as one leaving the unit form invariant, so that the condition of orthogonality (required for a canonical transformation) was $\sigma^{T*} = \sigma^{-1}$.

There were several advantages to this form of presentation. First, it brought the theory into the realm of known algebraic theory, which could be drawn on for existence and uniqueness proofs: remembering Born's attitude to physics, this may have been what brought him to the new formulation. Born could easily show in the new formulation that for a finite number of variables it was always possible to find, for a given form, an orthogonal transformation to principal axes (i.e., for a given matrix, a transformation to a diagonal one). For infinitely many variables, as in matrix mechanics, the mathematical theory had been developed so far only for bounded forms, and not for the unbounded forms with which matrix mechanics was concerned; but on purely empirical grounds Born was able to assert that "we may nevertheless assume that in the main the rules run likewise",[32] an assumption that was later justified by von Neumann. He also proved that if a diagonal form could be reached in the way described it was unique, and this enabled him to reformulate the matrix-mechanical problem as follows:

> Given any p°, q° satisfying the commutation relationships, to find an (almost certainly existing) orthogonal matrix S such that for $p = SpS^{-1}$ etc. $H(pq) = SH(p^\circ q^\circ)S^{-1} = W$ was a (unique) diagonal matrix.

He also introduced the standard mathematical terminology, calling the W_n ($W = (\delta_{nm} W_n)$) eigenvalues of the problem.

The new presentation also allowed a treatment of degenerate cases and, in principle, of continuous spectra. The problem had arisen in matrix mechanics that, given a set p, q of solutions, $p = Sp^\circ S^{-1}$, $q = Sq^\circ S^{-1}$, $H(pq) = SH(p^\circ q^\circ)S^{-1} = W$, this set was not necessarily unique, even though its energy, W, might be. For non-degenerate systems any alternative solution $p' = TpT^{-1}$, $q' = TqT^{-1}$ could be shown to require $\dot{T} = 0$ so that, since $W' = TWT^{-1} = W$, T was diagonal; the condition that p', q' be Hermitian then ensured that p', p, q', q differed only by an arbitrary phase constant, $|T(nn)| = \text{constant} \;\forall n$. For degenerate cases, however, $\dot{T} = 0$ did not necessarily imply that T was diagonal. Significantly different solutions were possible, reflecting the problem of the old quantum theory that "arbitrary small perturbations can bring about finite changes in coordinates",[33] and Born and Jordan had had to assume non-degeneracy. In the new presentation, degeneracy was associated with a familiar problem, namely the occurrence of multiple eigenvalues, and the mathematical tools were thus available to handle it. The multiplicity of the eigenvalue W_n (i.e. the number of linearly independent solutions of the problem) gave the statistical weight of the degenerate state (justifying, incidentally, Heisenberg's introduction of external fields for the computation of this weight) and Born could show not only that the theory could handle all degeneracies of finite multiplicity, but also that it incorporated the principle of spectroscopic stabitity and solved the statistical weights problem. For two sets of solutions p, q, p', q', the sum of the transition probabilities between the degenerate states and any other state was uniquely determined and independent of the choice of solution.

Continuous spectra (corresponding to aperiodic phenomena) still caused problems, for the theory was still based on the periodic behaviour of electrons in atoms, and uniform rectilinear motion, for example, was completely outside its framework. Born and Jordan had extended the virtual oscillator theory to Fourier integral representations but, as Born explained,

> "in classical theory also, the representation of a function by Fourier integrals is sometimes impossible, as for instance if the respective function increases linearly with time at large times";[34]

they had not been able to go beyond such representations in the virtual oscillator theory, and Born could not do so in the new theory either. He could however treat cases corresponding to Fourier integral representations in his new formulation, whereas the essential continuity of these cases had been quite alien to the discreteness of matrices. In the context of bilinear forms it was easy to convert the summation sign into an integral,[35]

$$\sum_n W_n x_n x_n^* \longrightarrow \int W(\phi)\, y(\phi)\, y^*(\phi)\, d\phi$$

and except for the problem of relating transition probabilities to amplitude densities (instead of to discrete amplitudes) all went through, with a suitable definition of an orthogonal transformation, as in the discrete case. The theory of infinite quadratic integral forms had been developed by Hellinger, for bounded forms, and as in the discrete case Born felt justified in carrying this theory over to unbounded forms, arguing that "Hellinger's methods obviously conform exactly to the physical content of the problem posed."[36]

Born and Wiener: the introduction of operators

At the end of October 1925, Born went as a guest lecturer to M.I.T., where Wiener, who had visited Göttingen the previous year, was professor of mathematics.[37] Wiener recalled that Born arrived very excited and searching for a further generalisation of matrix mechanics;[38] by divine plan, coincidence or whatever, Wiener had only six months previously completed a paper on "The operator calculus",[39] and this provided just the generalisation required. In a sense, this paper was very much in tune with matrix mechanics, for it was another example of a coming-together of mathematics and physics. Operators were the subject on the one hand of a rigorous mathematical theory, developed largely by Pincherle,[40] and on the other hand of an entirely non-rigorous technique used by physicists, most successfully and least rigorously by Heaviside.[41] Wiener's paper was an explicit attempt to bridge the gap between these two facets of the theory, and was very much in tune with the Göttingen approach to mathematics and physics.

Born's main problem was with those aperiodic phenomena that could not be covered by his integral treatment. He had dismissed these in the three-man-paper on the grounds that "the observable effects of the atom do not in general belong to this kind of function",[42] but he had clearly not dismissed them from his mind. They were central to the joint paper he prepared with Wiener, in which he wrote that

> "The representation of the quantum laws by matrices incurs serious difficulties in the case of aperiodic phenomena. For example, in the extreme case of uniform rectilinear motion, since no periods are present, the coordinate matrix can have no elements outside the diagonal $m=n$. However, even if m and n are continuous, this is not possible in any proper sense."[43]

The problem was solved using operators, which shared the non-commutative property of matrices[44] but not their restrictions. In fact, Born and Wiener defined an operator q in a completely general way, as

> "a rule in accordance with which we may obtain from a function $x(t)$ another function $y(t)$, which we symbolise by $y(t) = q\,x(t)$."[45]

Using the function $x(t) = \sum_n \left(e^{\frac{2\pi i W_n t}{h}}\right) x_n$, they showed that an operator q could be derived from any matrix or Hermitian form (q_{mn}) by putting

$$q = \lim_{\tau\to\infty} \frac{1}{2\tau} \int_{-\tau}^{\tau} ds\, q(t,s) \quad , \text{ where } \quad q(t,s) = \sum_m \sum_n q_{mn}\, x_m(t) x_n^*(s) = \sum_m \sum_n q_{mn}\, e^{2\pi i (W_n t - W_m s)/h} .$$

If $y_{mn} = \sum q_{mn} x_n$, then $y(t) = q\,x(t)$.[46] To derive a matrix from the operator q , they applied the operator $\left(e^{-2\pi i Wt/h}\right).q$ to the function $e^{2\pi i Wt/h}$, generating $q(t,W) = e^{-2\pi i Wt/h}\, q\, e^{2\pi Wt/h}$. They then defined

$$q_{VW} = \lim_{\tau\to\infty} \frac{1}{2\tau} \int_{-\tau}^{\tau} q(t,W)\, e^{-2\pi i (V-W)t/h}\, dt = \lim_{\tau\to\infty} \frac{1}{2\tau} \int_{-\tau}^{\tau} e^{-2\pi i Vt/h}\, q\, e^{2\pi i Wt/h}\, dt ,$$

which was the inverse process to the above. Not all such integrals converged, and they showed later that uniform rectilinear motion in fact gave divergent (oscillating) integrals and so had no matrix representation. But since the <u>operators</u> were quite generally defined, this motion was as answerable to the operator theory as was any other motion.[47]

Born and Wiener showed that their operators obeyed exactly the same rules as did matrices for multiplication, etc., and that one could define the time derivative of an operator q by $\dot{q} = Dq - qD$, where D was the differential operator $\partial/\partial t$. In analogy with the matrices, they also obtained the Hermitianness condition for operators and thereafter the application of operator theory to quantum mechanics was straight-forward. The commutation relationships, $pq - qp = h/2\pi i$, appeared as operator equations and since the operators D and $2\pi i W/h$ (energy) acted equally on the function used to connect the matrix and operator representations, $\left(\partial/\partial t\right) e^{2\pi i Wt/h} = \left(\frac{2\pi i W}{h}\right) e^{2\pi i Wt/h}$, the form $\dot{q} = Dq - qD$ replaced the matrix equivalent $\dot{q} = \frac{2\pi i}{h}(Wq - qW)$. The operator equation $\dot{q} = Dq - qD$ thus led to the diagonalisation of the Hamiltonian and the old problem of diagonalising a matrix was expressed as a classically familiar eigenvalue problem.

The origins of the Born-Wiener paper seem to have been quite straightforward, for given that Born was searching for a generalisation of matrix mechanics Wiener would immediately have recognised the linear transformations of Hermitian functions as operators. The paper was, however, very important: it introduced operators to quantum mechanics (before Schrödinger), and extended this mechanics to cover all aperiodic phenomena.

It also raised an interesting point concerning an aspect of quantum mechanics that is not often treated, namely the intrusion of the imaginary constant, 'i'. Born and Wiener noted that their theory seemed to attribute a motion to a particular state of the form $q_k(t) = \sum_k q_{nk} e^{2\pi i \nu(nk)t}$,

and that this was complex, even when the matrix (q_{mn}) was Hermitian; from this they deduced that

> "There are then two real motions belonging to every state, corresponding respectively to the real and pure imaginary parts of the line of the line of the matrix."[48]

Unable to make anything of this, they did not pursue it, but Dirac had already noticed the same sort of thing in October, and had followed it slightly further, though without publishing his considerations.[49] In the virtual oscillator theory, the emission and absorption oscillators had each been defined by the real part of a form '$Ce^{i\omega t}$', and although they did not carry energy they were each related (probabilistically) to a transition process. In the new quantum mechanics, however, the restriction to the real part was dropped, and an atom described by complex oscillators of the form '$Ce^{i\omega t}$'. As Dirac said, "The imaginary exponential is essential and fundamental in the new theory",[50] but what did this mean? He noted that one needed a combination of $e^{i\omega t}$ and $e^{-i\omega t}$ oscillators to get any radiation at all, but he was essentially no better able to interpret the situation than were Born and Wiener.[51]

In one sense,[52] the inclusion of an 'imaginary' motion might be taken as reperesenting the 'potentiality' aspect of quantum mechanics, akin to Einstein's ghost waves, and the later interpretation of Schrödinger's wave function (by Born) in terms of probability; the imaginary motion, by interfering with the real motion, gives the wave side of the wave-particle duality. From a wave-theoretical point of view, on the other hand, it expresses the discreteness of the theory, as in Heisenberg's quantisation rule. The central feature of matrix mechanics, the commutation relationship $pq - qp = h/2\pi i$, can thus be seen either as a wave interference between particles, or as a discreteness property of waves, or, neutrally, as an interaction between real and imaginary motions. If in the classical theory two oscillators are multiplied, a real answer ensues, $\mathcal{R}(e^{i\omega_1 t})\mathcal{R}(e^{i\omega_2 t}) = \cos\omega_1 t \cos\omega_2 t$. But in quantum theory the imaginary oscillators enter, and the result is far more complex,

$$e^{i\omega_1 t} e^{i\omega_2 t} = \{\cos\omega_1 t \cos\omega_2 t - \sin\omega_1 t \sin\omega_2 t\} + i\{\cos\omega_1 t \sin\omega_2 t + \sin\omega_1 t \cos\omega_2 t\}$$

Usually there are also negative oscillators, $e^{-i\omega_1 t}$, $e^{-i\omega_2 t}$, so that four such products have to be considered, and if the ω_1 oscillators are independent of the ω_2 oscillators all the terms due to imaginary motions cancel out, leaving the classical product; if, however, the oscillators are related to each other through an operator, as momentum is related to position through the differentiation operator, the cancellation does not occur, the imaginary motions contribute to the product, and commutation relationships follow.

From the above analysis we may deduce that the inclusion of imaginary motions is at the very core of quantum mechanics but we still cannot interpret these motions physically, and this is important. The new quantum mechanics was not only lacking a structural interpretation; it also showed every sign, from the very beginning, of being unable to support such an interpretation.

Born's reaction to wave mechanics and his statistical interpretation

After his visit to M.I.T., Born spent January through March 1926 travelling across the United States on a lecture tour. On his return to Germany, he went straight to Frankfurt where his wife was convalescing having been taken ill in America at the beginning of the year. Thus, by the time he got down to work again in Göttingen, Schrödinger's wave mechanics had already been published. Born was naturally attracted to the new theory. It had been developed from the wave theory of matter to which he had been attracted the previous year, and, using operators, it was coextensive with the theory he had just developed with Wiener;[53] since it was expressed in terms of familiar classical mathematical physics, however, it was physically far more suggestive (albeit misleadingly so) than this theory. Heisenberg and Bohr saw the wave mechanics as a physically misleading diversion away from the central paradoxes of quantum theory, and thought that attention should rather be focussed on these paradoxes. But Born reasoned that any successful theory would have to include the paradoxes and that it did not matter on these grounds which mathematical formulation was adopted. He based his own choice upon utility, and since he had himself been led virtually to the wave mechanics in his attempt to master aperiodic phenomena, for which the matrix formulation was inadequate, this choice was obvious: he considered aperiodic phenomena, in the form of collision processes, and concluded that

> "Of all the different forms of this theory only Schrödinger's has proved suitable here, and I may directly on these grounds take it as the most profound comprehension of the quantum problem."[54]

The schism between 'physicists' and 'mathematicians' widened as Born, treating profundity as a function of utility, asserted in his first paper on the wave mechanics the total superiority of the mathematical over the physical viewpoint. Encouraged by the enormous success of the mathematical theory, he insisted that the whole physical theory must be contained within it:

> "Many take it that the problem of the transitions of quantum mechanics ... cannot be comprehended, but that new concepts will be needed. I myself came, through the impression of the compactness of the logical foundations of quantum mechanics to the opinion, that this theory is complete and that the transition problem must be contained in it."[55]

Born's paper also included the first statement by a major figure in the development of quantum mechanics in which causality was explicitly and emphatically rejected and the introduction of the most fundamental feature of the interpretation of quantum mechanics, according to which the wave form in phase-space (Schrödinger's ψ) gave a measure of the probability of the existence of a particle in the state it described. To understand how these remarkable developments arose and were connected, we must look more closely at this paper and at those that followed it.

In 1925, Bohr had started to look at collision theory on the grounds that it combined the main problems of quantum theory, namely aperiodic effects and transition processes. Born now studied it for the same reason, and considered the collision between an atom and a free electron. Noting that matrix mechanics could not cope with this situation, he applied Schrödinger's wave mechanics and obtained an asymptotic solution at infinity,

$$\psi'_{nz}(\underbrace{x,y,z}_{\text{electron}};\underbrace{q_k}_{\text{atom}}) = \sum_m \iint_{\alpha x+\beta y+\gamma z>0} d\omega \, . \, \Phi_{nm}^{z}(\alpha,\beta,\gamma)\, \sin k_{nm}(\alpha x+\beta y+\gamma z+\delta)\, \psi_m^{(0)}(q_k) \, ,$$

where $\psi_m^{(0)}(q_k)$ was the m th eigenstate of the undisturbed atom, $\Phi_{nm}^{z}(\alpha,\beta,\gamma)$ the wave function for the electron (originally incident from the z direction with energy τ) and $W_{nm}^{z} = h\nu_{nm}^{0} + \tau$ the solution for the final energy of the electron. He wrote that

> "That means the perturbation is comprehended at infinity as a superposition of solutions of the unperturbed motion"[56]—

but what did it mean physically? Considering the physical implications, he claimed that

> "To interpret the result in terms of particles, only one interpretation is possible: Φ_{nm}^{z} [changed in a footnote to $|\Phi|^2$] indicates the probability that an electron from the z direction will be sent in the $\alpha\beta\gamma$ direction (and with phase δ), whereby its energy τ has been increased by a quantum $h\nu_{nm}^{0}$ at the cost of the atom's energy.
>
> "Schrödinger's quantum mechanics thus gives a complete answer to the questiom of the effect of a collision: but it concerns no causal relationship. One cannot answer the question "what is the state after the collision", but only the question "what is the probability of a given effect of the collision" (whereby the quantum-mechanical energy levels must naturally be preserved).
>
> "Here the whole problem of determinism presents itself. From the standpoint of our quantum mechanics there is no quantity which remains causal in the case of an individual collision effect; but also in practice we have no grounds to believe that there are inner eigenstates of the atom that stipulate a determined collision path. Should we hope to discover such eigenstates later (such as phases of internal atomic motions), and to determine them for the individual case? Or should we agree that the agreement of theory and experiment on the impossibility of giving a stipulation of the causal lapse is a preestablished harmony, which rests on the non-existence of such stipulations. My own inclination is that determinism is abandoned in the atomic world. But that is a philosophical question, for the physical arguments are not conclusive."[57]

In a second paper, completed four months later,[58] Born re-expressed his interpretation, linking it with Einstein's idea that "the waves should be only to show the light corpuscles the way",[59] and continued to justify it by further applications. He also compared the physical approaches of wave mechanics and matrix mechanics as they had stood before the introduction of his interpretation, and he found both to be lacking. He preferred Schrödinger's approach, because "the retention of the usual presentation in space-time is possible"[60], but he insisted that one was forced to abandon, as Schrödinger had not, the causal determination of individual events. He still felt that the possibility of hidden parameters was unlikely (though Frenkel had told him that they might exist), but he argued that this was not relevant anyway: it could not change the "practical indeterminism"[61] of the theory, and would have to lead to the same formulae as at present.

Born amplified this last point in a short note to <u>Nature</u> the following month,[62] stressing that for practical purposes microscopic coordinates did not exist. Classical theory, he claimed, introduced such coordinates (such as those pertaining to the motions of molecules) only to ignore them and take their statistical aggregate. Quantum theory, on the other hand, did not bother with this charade: one could not dismiss the possibility of microscopic coordinates existing, but they were of no significance unless one could measure them, which one could not.[63]

It was as we have seen natural that Born should have adopted Schrödinger's approach, but the reasons he gave for this were nevertheless interesting. In the first paper he justified his choice on the grounds of utility, which was quite reasonable, but linked this with profundity, which was less so. In later papers he also drew on "the retention of the usual presentation in space-time",[64] and the fact that the quantum behaviour struck him as natural in wave mechanics, but not in matrix mechanics: the commutation relationships had to be imposed upon the latter, but arose naturally as operator equations in the former. I shall discuss these justifications further in the context of causality, later, but we may note immediately the emphasis upon two themes. One was the desire to preserve so far as possible the classical conceptions, and the other was the feeling that his mathematical treatment was more basic than any physical considerations. The two may have been linked, for the mathematical bias had brought him into opposition with Bohr and Pauli, and this may have prompted him to reject the opinions of these physicists, that the classical conceptions were inadequate. Working in Bohr's field, but against his authority, Born felt that he had built upon the wave mechanics a complete theory, which was not susceptible to redevelopment either in terms of its interpretation or in terms of non-classical conceptualisations.

Since there could be no doubt in Born's mind that Schrödinger's interpretation was not feasible and that the waves spread out in space,[65] this interpretation had to be replaced. Born was thus drawn into the physical side of the problem, and he wrote that "to interpret the result in terms of particles, only one interpretation is possible".[66] The situation was not quite that simple, however, and he did not explain why an interpretation in terms of particles, and only particles, was necessary, or why the interpretation that he chose was forced. His emphasis upon a classical space-time description, a part, presumably, of his rebellion against the ideas of Bohr, Heisenberg and Pauli (who tended to reject such a description and look for a physical meaning in Heisenberg's new kinematics), restricted him to a structural interpretation. But there were essentially four different classes of structural interpretation from which he could choose:

[i] the wave, ψ , could be treated as a statistical result of many causally determined individual particle motions;

[ii] the wave could be treated as a guide wave, itself determining (causally) the paths of individual particles;

[iii] the wave could be treated as a guide wave, determining the motions of individual particles only probabilistically: strict causality would be effectively lost in this class of interpretation, but could be assumed to exist at some sub-microscopic level.

[iv] the waves could be treated as physical entities combining statistically to create particles, causality being retained on all levels.

Schrödinger's interpretation (for matter) was in class [iv], and Einstein's attempt at a dual theory for light in class [ii]. De Broglie's theory (for both light and matter) also corresponded to class [ii], although de Broglie had entertained hopes of a class [iv] interpretation. Slater's conception of light was also in class [ii], while Bohr's conception of light in the BKS theory had been closest to [iii], the particles being replaced by classical pulses.

Concerning his choice of interpretation, Born recalled that experiments being conducted by Frank convinced him of the corpuscular nature of the electron,[67] and that during his discussions with Franck and Elsasser on electron diffraction it had already been clear to him that the proper interpretation of this phenomenon was a statistical one.[68] Late in these discussions he had written to Einstein that he was inclined to believe in matter waves,[69] so the latter recollection is of dubious value; but the former is more important. Until about 1925, there had been no reason at all to doubt the corpuscularity of electrons; as we have seen, however, the developments of that year changed the situation radically, and Born became quite enthusiastic about de Broglie's matter waves. According to how he interpreted de Broglie's ideas, this would have placed his views in class [ii] or possibly class [iv], but the interpretation he presented in 1926 was clearly in class [iii]. The (electron) wave was supposed to define a probability according to which the behaviour of a particle was restricted, but within this restriction the particle could move freely, effectively without cause:

"The path of the particle follows probability, but the probability develops causally."[70]

That Born should reject an interpretation of class [iv] was natural, for he saw that the wave packet did not remain localised, but was convinced, as he recalled, of the corpuscular nature of the electron; but why did he change from [ii], and to [iii] rather than to [i]? And why, in his second paper, did he identify his interpretation with Einstein's suggestion that

"the waves should be only to show the light corpuscles the way",[71]

when he must have known that Einstein preferred an interpretation of class [ii]? He could have been taking Einstein's suggestion in the general sense (incorporating both [ii] and [iii]), but he was very dissapointed when Einstein did not agree with his ideas, and seems to have assumed that their interpretations were in fact the same.[72]

With respect to his preference for [iii] over [ii], the answer seems to concern the level of 'reality' afforded the wave in the two interpretations, for the wave in [ii], which 'carries' particles, is far more 'real', structurally, than that in [iii] which only governs them probabilistically. Schrödinger's waves were not in real physical space but in multi-dimensional phase space, and although this was true of de Broglie's waves also Born may have come to see its importance only gradually.

Another point is that in his work with Wiener Born had noticed, apparently for the first time, the importance of the essential inclusion of imaginary motions in the new theory. The fact that 'i' entered into quantum theory was already obvious, but Wiener may have prompted him to give it more thought than he had done previously. Since the wave motions were necessarily in part imaginary he may have concluded that they could not possibly correspond to physical reality.

Since these arguments were based on the mathematical formalism, Born would have found them quite convincing, and it may have been that he expected Einstein to follow him in this, assuming that the latter would not hold to a view that he saw to be quite discredited. A third possible factor, which would not have had the force of the others but which may have exerted some subtle influence, is the way in which Schrödinger's wave function was introduced, through the equation "$S = k \log \psi$". The similarity with the entropy formula $S = k \log W$ is quite striking, and may conceivably have helped encourage the identification of ψ with a probability.

Concerning class [i], to which Einstein in fact resorted when he was eventually forced to abandon [ii], there seem to be two major points. First, the mathematical formulation treated the wave as primary, and since Born took the formulation as incorporating the physical interpretation he would naturally have expected the wave to be primary in a physical sense also, and not a mere statistical manifestation. Secondly, class [i] interpretations had long been discredited for light (Taylor's low intensity interference experiments) if not for matter. As I have stressed, quantum mechanics was concerned primarily with matter rather than light, and Born himself was concerned mainly with the electron. Einstein's considerations, on the other hand, referred to light. I have discussed the whole question of interpretations as if the situations for photon and electron were completely analogous, and this is justified because Born himself did this, referring in the second paper to the "well-known analogy" between light and matter.[73] But was this analogy really well-known, so soon after the introduction of matter waves? Outside de Broglie's work, Born's is the first reference to it that I can find, and it may have been that he called the analogy well-known in order to avoid having to elaborate on it (a common practice). It was, however, well-known to him, and with his interpretation it became firmly established.

Causality

One of the most striking features of Born's interpretation was his rejection of causality. In 1924, in the context of the BKS theory, he had declined to comment on this issue,[74] but the fact that he did so explicitly suggests that he had not then given up the belief in causality that we know him to have held at the beginning of the decade,[75] and which was threatened by this theory. Thereafter there is no extant reference to the subject at all until this astonishingly explicit declaration of March 1926.

He admitted that it was a philosophical decision and that the physics itself was not conclusive, and this strongly suggests an external influence. If there was such an influence we cannot pin it down, but the most obvious possibility is that he may have been influenced (as Forman suggests that others were before him) by the popular Lebensphilosophie of the Weimar intellectual milieu.[76] There are two facts in support of this view. First, his wife certainly was influenced by the milieu: very concerned with the poetry and literature of the age, she mocked Born's scientific determinism, and her illness placed her in a good position to press her point of view.[77] Secondly, he emphasised strongly the central role of statistics in his theory, referring to "the close connection between mechanics and statistics",[78] and even to "a fusion of mechanics and statistics".[79] In the Decline of the West, which Born had read and which was a major influence upon the milieu, Spengler[80] predicted that the science of the future would be based increasingly upon statistics, and this prediction may have struck Born as he considered the interpretation of his theory.

Another factor to be noted is that Born had just spent a long time in the United States. While there he was probably asked repeatedly about the Weimar milieu, and also about the impact of [illegible] on mathematics, since this had been most felt in Göttingen; he may have been prompted to think more, and more deeply, about causality than he had done previously. A final possibility is that Weyl may have exerted some influence. Despite his strong disagreement with Hilbert, Weyl was popular in Göttingen; he seems to have been a frequent visitor, and at the end of 1925 he took an interest in quantum mechanics, and communicated on the subject with Jordan. He had firmly rejected causality himself (though not in a quantum-mechanical context, particularly) and may well have pressed this viewpoint on Born, either through Jordan or personally, when Born returned to Germany in 1926.[81] The latter possibility is strengthened by the fact that Weyl's help was acknowledged by Schrödinger, in his papers on wave mechanics;[82] he may well have been consulted by Born.

Apart from the possible external factors, there were also internal ones. Quantum theory had long involved an element of uncertainty, in the location of an orbit, the moment of a transition, etc.; but this had always been a case of uncertain conclusions following from uncertain data. In Born's analysis of the atom-electron collision, however, uncertain conclusions (the atom in a superposition of states, the electron spread throughout space) followed from apparently definite data on the motion of the electron and the state of the atom. The existence of hidden microscopic parameters would have changed this, of course, but the theory did appear in some way to create uncertainty, and Born could well have deduced from this that physics was itself uncertain.

But why make a point of this, when he had previously favoured a neutral presentation? Again external influences may have acted, but for this too there was a possible internal source. The rejection of causality was equivalent to the rejection of any relevant microscopic coordinates, and this was a remarkable feature of Born's presentation. It was only in the later papers that he insisted that his theory was final and complete regardless of whether microscopic coordinates existed and were measurable or not, but he was clearly tending towards such a position even in the first paper. Heisenberg had built his theory upon quantities that were in principle observable, but Born restricted himself to those that were in practice observable, at the time of writing, and asserted that no future experiments could change the theory that he had evolved. This attitude seems to have been linked closely with his assertion that the physical interpretation must follow uniquely from the mathematical formulation, for both reflected an extraordinary confidence in the power and correctness of the theory, combined with an element of pride and even posessiveness.[83] Had microscopic coordinates been found, and a causal theory developed, Born's theory would not have been final — and he was convinced, simply, that it was.

Summary

The origins of Born's ideas remain confused. The fact that he had been ill and overworked for many months probably contributed to his innovative powers, and to the somewhat dangerous nature of his assertions (for it is always dangerous to claim that a theory is final), but beyond that we can only put forward possibilities: there appears to be no extant correspondence covering the crucial period while he was in America, and I can find nothing either on such factors as Weyl's involvement. The conclusions Born reached, however, are relatively clear. By adopting a mathematical approach rather than a physical one he had come to a theory which was highly successful and which, although limited in its predictive power, showed a remarkable agreement in its limitations with experiment. It led, moreover, to what he saw as a unique physical interpretation.

In respect of my main thesis, three aspects of the theory were particularly important. First, the discussion of causality was far clearer than it had been in Bohr's work, and the conclusions were far more emphatic. Secondly, the identification of the wave with a probability distribution gave it an intermediate ontological status comparable to that of the virtual oscillators, only again this point was clearer and more strongly emphasised than it had been by Bohr, and was given added edge by the fact that the waves were in general in multi-dimensional phase space. Thirdly, the phenomenology that had entered quantum theory in Heisenberg's methodology reappeared, but it was again emphasised by being raised to an epistemological status: the theory was not based upon observables, but restricted to them.

In many ways Born's theory seems to have stemmed out of his disagreement with the physical ideas of Bohr, Heisenberg and Pauli. Ironically, however, these three aspects were all developments of conceptions that had originated with these physicists, and a conceptual continuity was thus preserved.

IV.3. THE BACKGROUND TO TRANSFORMATION THEORY

Transformation theory in matrix mechanics

Despite Born's conviction to the contrary, his quantum mechanics was still somewhat arbitrary and incomplete. In the transition to wave mechanics the mathematical clarity of matrix mechanics had been lost, and both physical clarity (other than in a very naive sense) and mathematical rigour were lacking; the basis of the theory on Schrödinger's hypothesised wave equation was also unsatisfactory. The solution of these problems and the establishment of a rigorous and definitive theory involved a synthesis of various perspectives on quantum mechanics and eventually emerged in what became known as the transformation theory.

The first expression of the quantum-mechanical problem in terms of transformations was in the paper by Born, Heisenberg and Jordan, whose treatment was made rigorous by Jordan in 1926.[84] Starting with the canonical equations of motion for H and Hermitian p°, q° satisfying the commutation relationship, they expressed the problem as one of finding a transformation matrix S such that $H(pq) = SH(p^{\circ}q^{\circ})S^{-1} = W$ was diagonal. They also found a method for obtaining the required transformation S in the case of small perturbations, but only in this case.

In the theories of Schrödinger and Born the problem became a classical eigenvalue one, and both the visualisation in terms of transformations and the generality associated with this visualisation were lost: whereas a transformation could be applied to any variable in the system, the eigenvalue problem was restricted to the Hamiltonian or energy function. This loss was restored in September 1926 by London,[85] who set out to superpose the matrix-mechanical transformation theory onto wave mechanics and proved that canonical transformations did preserve the Schrödinger eigenvalues. But neither London's main result nor his suggestion that the transformations could be interpreted as rotations in an infinite dimensional Hilbert space spanned by an orthogonal system of eigenfunctions seem to have had any impact on the main line of quantum-theoretical development.

Lanczos' formulation of quantum mechanics

Meanwhile, at the end of 1925, Lanczos[86] had formulated quantum mechanics in terms of integral equation theory, the close connection of which with matrices we have already noted. He took the Heisenberg-Born-Jordan matrix mechanics as his starting point, and since the matrix indices represented the possible initial and final states of a system he linked them with eigenfunctions for these states, $i \sim \varphi^{i}(s)$. He then derived from the matrix a_{ik} a function,

$f(s,\sigma) = \sum_{i}\sum_{k} a_{ik}\,\varphi^{k}(\sigma)\,\varphi^{i}(s)$, or conversely $a_{ik} = \int f(s,\sigma)\,\varphi^{i}(s)\,\varphi^{k}(\sigma)\,ds\,d\sigma$.

This gave $\dot{f} = Kf - fK$, for K the symmetric kernel, and the matrix-mechanical solutions, $\dot{q} = \partial H/\partial p$, $\dot{p} = -\partial H/\partial q$, were given by

$I = \int\{(pKq - pqK) - H\}(s,s)\,ds$ = extremum. Lanczos' formulation was continuous and analogous to Schrödinger's with integral equations instead of differential equations. It had limited historical significance, as it arrived too late to influence Schrödinger's connection between the wave and matrix theories, but it did have some influence later, upon Dirac.

Dirac's formulation of quantum mechanics

Dirac had meanwhile introduced a more important formulation of quantum mechanics. It was developed in Cambridge, in parallel with (but, after Heisenberg's paper, apparently independent of) matrix mechanics, and it was notable above all for its mathematical elegance. Both the independence and the elegance were typical of Dirac, who was always happy to go his own way and let other people do the same, without worrying about questions of priority. Indeed he recalled that all he did with quantum mechanics was to take up other people's equations and play around with them.[87] His thinking was essentially geometric (his own word)[88] and his playing around was an attempt to interpret the equations geometrically; this naturally produced formulations that were elegant. The problem of physical interpretation, on the other hand, was not one that bothered him much and he dismissed such questions as the reality or otherwise of Schrödinger waves as "metaphysics".[89]

In September 1925, Fowler received a proof copy of Heisenberg's paper on a new kinematics, and passed it on to Dirac, who had been interested in quantum theory for some years. Dirac did not apparently think much of it, and put it aside for a couple of weeks; but then, on reassessing it, he realised that it was "the real thing", and set to work upon it. He completed his first paper on the subject in November, and in this he formulated the theory in terms of Poisson brackets.[90]

He first asked what the most general operation 'O' was that could be applied to quantum-mechanical quantities (as defined by Heisenberg), and that was both associative and distributive. He found this to be $Ox = xa - ax$, with 'a' a quantum-mechanical quantity, and wrote the operation as $\partial/\partial v$, so that $\partial x/\partial v = xa - ax$. He then used the correspondence principle to compare the quantum quantity $xy - yx$ with the classical theory of action and angle variables, and he came to the conclusion that $xy - yx$ corresponded to a classical Poisson bracket,

$$xy - yx \sim \frac{ih}{2\pi}(x, y)$$

[J, ω the classical action, angle variables]

$$= \frac{ih}{2\pi}\left\{\frac{\partial x}{\partial \omega}\frac{\partial y}{\partial J} - \frac{\partial y}{\partial \omega}\frac{\partial x}{\partial J}\right\}$$

For classical canonical variables p, q, $(p,q) = 1$, so that $pq - qp \sim h/2\pi i$ and the commutation relationships were recovered; Dirac found that he could formulate the whole of Heisenberg's mechanics along the lines of classical theory, merely by replacing the classical commutator with the quantum one, (x, y) with $[x,y] = (xy - yx)2\pi/ih$. Then, for canonical variables, $[Q_r, Q_s] = [P_r, P_s] = 0$, $[Q_s, P_s] = \delta_{rs}$ while $[x, H] = \dot{x}$ leading to the frequency condition, and $[J_r, H] = 0$, $[J_1, J_2] = \text{const.}$

A problem for the historian with Dirac's elegant formalisms is that the elegance hides the creative process. The order of the paper is no guide to the order of his investigation, and the only evidence we have as to his thought process is that of his recollections, which are not extensive. All we know in this case is that it suddenly occurred to him, after a few weeks, that $xy - yx$ might correspond to a Poisson bracket, but that he was not quite sure what a Poisson bracket was, and had to look it up. Once he had established this connection, the rest of the paper would have fallen out naturally from a consideration of the important classical relationships, but we do not know what sparked off his initial insight.

There were two main advantages to Dirac's formulation. In the first place it simplified the presentation of the theory, and in the second it clarified the relationship between the quantum and classical theories. Dirac could formulate a problem in classical theory and move simply and directly across to quantum theory, or vice-versa: the two theories were differentiated merely by their different commutator expressions. He pursued this dualistic approach in a second paper in January,[91] when he introduced the concepts of q-numbers (quantum quantities, obeying quantum rules) and c-numbers (classical numbers):

"The fact that the variables used for describing a dynamical system do not satisfy the commutation law means, of course, that they are not numbers in the sense of the word previously used in mathematics. To distinguish the two kinds of number, we shall call the quantum variables q-numbers and the numbers of classical mathematics which satisfy the commutation law c-numbers."[92]

He admitted that

"At present one can form no picture of what a q-number is like",[93]

and made the point that experimental results were measured in c-numbers, so that to compare a quantum-mechanical prediction with experiment was only possible if the q-numbers could be represented in terms of c-numbers. For a multiply periodic system, he recovered the Heisenberg representation of a quantum variable as $x(nm)\,e^{i\omega(nm)t}$, with the elements of x and ω c-numbers; he observed that whereas the representation was used as a definition in matrix mechanics it was derived from general properties in his formulation.

Dirac referred to the Heisenberg-Born-Jordan solution in terms of transformations, but argued that "these formulae do not appear to be of great practical value",[94] while the technique of action and angle variables, which could be carried over from the classical theory to his own, clearly was.[95] When the next major development of quantum mechanics came, however, it was as a synthesis of both these (and other) approaches, in which the theory of transformations was dominant. This development was also largely due to Dirac himself.[96]

IV.4. TRANSFORMATION THEORY, UNCERTAINTY, AND QUANTUM STATISTICS

By the autumn of 1926, there were many formulations of quantum mechanics, of which three were particluarly important:

(i) The Born-Heisenberg-Jordan formulation was founded upon commutation relationships and energy conservation; the mathematical solution was obtained by diagonalising the energy matrix H through a suitable transformation S of the canonical variables p, q.

(ii) The Schrödinger-Born formulation was founded on the Schrödinger wave equation; the mathematical solution was obtained through a classical eigenvalue problem, and a physical interpretation was provided through Born's statistical interpretation.

(iii) The Dirac formulation was founded on a substitution of quantum for classical commutators in the classical theory; the mathematical solution was that of matrix mechanics and a requirement for a physical interpretation was specified, namely that the mathematical q-number solution should be represented by c-numbers, but no means were given of obtaining such an interpretation.

As we have seen, connections had been made between the formulations, but only in a piecemeal fashion. A proper fusion of the formulations was however achieved in transformation theory, which was based upon two independent papers, one by Dirac[97] (completed by December 2) and the other by Jordan[98] (completed by December 18).

Dirac's transformation theory

Dirac expressed the problem of finding a physical solution (i.e. a testable prediction — he was not interested in metaphysical interpretations) as follows:

> "When one has performed all the calculations with the q-numbers and obtained all the matrices one wants, the question arises how one is to get physical results from the theory, i.e. how can one obtain c-numbers from the theory that one can compare with experimental values."[99]

Having reviewed the situation to date, he introduced a new idea, which he attributed to Heisenberg. This was that the time mean of any variable was given by the diagonal elements of its matrices, and that one could use this to calculate the proportion of a total time for which a variable, g say, was between any two values g', g''.

Dirac knew that quantum mechanics did not give complete results, and having noted in particular that p and q could not be specified accurately simultaneously he decided to find out what questions could be asked within quantum mechanics and what answers could be obtained, expecting that the latter would turn out to be of the type considered by Heisenberg. He realised that he would need physical assumptions such as Born's (on probability) or Heisenberg's (on mean value), but he emphasised that

> "The notion of probabilities does not enter into the ultimate description of mechanical processes; only when one is given some information that involves a probability (e.g. that all points in η-space are equally probable for representing a system) can one deduce results that involve probabilities."[100]

Since it appeared that quantum mechanics could give only probabilistic results, and not precisely determined ones, Dirac deduced that it must involve probabilistic postulates, but he kept these well apart from the mathematical theory.

Given any variable g as a function of any pair of canonical coordinates ξ_r, η_r, Dirac asked what questions could be posed within quantum mechanics, and insisted that the only possibilities were those of the form:

> Given ξ_r, what do we know about g as a function of η_r, the canonical conjugate of ξ_r?

Generalising this question, he expressed the problem as one of transforming from a given scheme of matrices (e.g. that in which the ξ_r were determined, i.e. diagonal) to another scheme, and he proceeded to investigate this problem.

Before commencing the investigation proper, he noted that a matrix representation could be continuous, discrete, or a mixture of both. He thought the last too complicated to work with, and chose the continuous rather than the discrete as being more general, but in order to work with continuous matrices he needed some mathematical tools: what, for example, was the unit continuous matrix? To solve this and other problems he had to introduce the δ-function, $\delta(x-y) = 0, x \neq y$; $\int_{y-\alpha}^{y+\alpha} \delta(x-y)\, dx = 1$. This was familiar in electrical theory, and Dirac himself had been brought up with it, but it was anathema to pure mathematicians, who did not consider it a true function and criticised Dirac's work accordingly.

Dirac considered a general transformation from a scheme in which g was diagonal to one in which G was diagonal, where $G = bgb^{-1}$, or in continuous matrix terminology, $G(\alpha'\alpha'') = \iint b(\alpha'\alpha''')\,d\alpha'''\,g(\alpha'''\alpha^{(4)})\,d\alpha^{(4)}\,b^{-1}(\alpha^{(4)}\alpha'')$.

Since there was in general no one-to-one correspondence between the rows and columns in the two schemes, he rewrote this as

$$G(\xi'\xi'') = \iint b(\xi'\alpha')\,d\alpha'\,g(\alpha'\alpha'')\,d\alpha''\,b^{-1}(\alpha''\xi''),$$

which would have been rather clearer, had he not changed once again to a "better but equivalent notation" that dispensed with the G as unnecessary, namely

$$g(\xi'\xi'') = \iint (\xi'/\alpha')\,d\alpha'\,g(\alpha'\alpha'')\,d\alpha''\,(\alpha''/\xi'')$$

Here (ξ'/α'), (α'/ξ') were mutually orthogonal and normalised systems of functions, such that $\int(\xi'/\alpha')\,d\alpha'\,(\alpha'/\xi'')$ = 0 when $\xi' \neq \xi''$, or "a certain kind of infinity when $\xi' = \xi''$", and similarly with respect to $d\xi'$. $g(\alpha'\alpha'')$, $g(\xi'\xi'')$ were two matrices representing the same variable g in two different schemes, the first of which made the α diagonal, and the second of which made the ξ diagonal. Primed letters represented c-numbers, and unprimed ones q-numbers.

Due largely to the notation,[101] Dirac's paper is fairly difficult to follow, but he also expounded the theory in a letter to Jordan (written when he heard of Jordan's similar work), and this was marginally clearer:[102] since the paper is anyway readily available, I shall follow the letter here. For "a matrix representing g according to a new scheme in which the rows and columns refer to different dynamical variables",[103] Dirac put

$$g(\xi'\alpha') = \int g(\xi'\xi'')\,d\xi''\,(\xi''/\alpha') = \int(\xi'/\alpha')\,d\alpha''\,g(\alpha''\alpha')$$

and he found that in this new scheme the main elements of the ξ were $\xi_r(\xi'\alpha') = \xi_r'(\xi'/\alpha')$, and their canonical conjugates $\eta_r(\xi'\alpha') = \frac{-ih\partial}{\partial\xi_r'}(\xi'\alpha')$. More generally, "if $f(\xi_r,\eta_r)$ is any function of the ξ s and η s its matrix elements in this scheme are easily verified to be $f(\xi_r,\eta_r)(\xi'\alpha') = f(\xi_r', \frac{-ih\partial}{\partial\xi_r'})(\xi'\alpha')$"[104],

and for a diagonal matrix in the α scheme, $f(\xi_r,\eta_r)(\xi'\alpha') = f(\alpha')(\xi'/\alpha')$ with $f(\alpha')$ the diagonal elements. But then $f(\xi_r', \frac{-ih\partial}{\partial\xi_r'})(\xi'/\alpha') = f(\alpha')(\xi'/\alpha')$,

and even in Dirac's notation this was recognisable as the Schrödinger wave equation, $f(\xi_r,\eta_r)$ being the Hamiltonian $\sim H(p,q)$. He concluded that

> "The quantities (ξ'/α') of the transformation theory [which transform from a scheme in which $\xi \sim q$ is diagonal to one in which $f \sim H$ is] are thus the eigenfunctions of Schrödinger's theory."[105]

Dirac had thus solved the general mathematical problem of matrix theory, and shown that this included Schrödinger's theory as a special case. He continued to examine the problem of physical interpretation:

> "If (g) denotes another scheme of matrix representations, in which $g_1, g_2, \ldots$ are diagonal matrices, then $\int_{g'}^{g''}(\xi'/g')\,dg'\,(g'/\xi'')$, considered as a matrix with elements defined by $(\xi'.\xi'')$, may be shown to be the matrix that represents that function of the dynamical variables which is equal to unity when each g_r satisfies $g_r' < g_r < g_r''$ and zero when these conditions are not fulfilled. Its diagonal elements determine the average value of this function over the whole of η-space, which is the same as the fraction of η-space for which $g_r' < g_r < g_r''$. If all points in η-space are equally probable (and only when this is so) these diagonal elements determine the probability of each g_r lying in the range g_r'-g_r'' for given values (ξ_r') of the (ξ_r) s."[106]

In his paper, Dirac showed that this was equivalent to Born's interpretation if one assumed that "the coefficients that enable one to transform from the one set of matrices to the other are just those that determine the transition probabilities."[107] In Born's theory, if ψ_0 represented the unperturbed system and ψ_t the perturbed system at time t, then $\psi_t(\alpha') = \int \psi_0(\alpha')\,d\alpha'\,c(\alpha'\alpha')$, where $|c|'^2$ was the probability. In Dirac's theory, the perturbed system was given by $(q_t'/\alpha_0') = \int (q_t'/\alpha_t')\,d\alpha_t'(\alpha_t'/\alpha_0')$ and the probability by $(\alpha_0'/\alpha_t')\,d\alpha_t'\,(\alpha_t'/\alpha_0') = |(\alpha_t'/\alpha_0')|^2$

Jordan's transformation theory

Jordan's paper was completed a fortnight after Dirac's and it too was based on a suggestion as to the physical connection between quantum theory and experiment, and motivated by a desire to connect the solutions of wave mechanics and matrix mechanics. Jordan set himself the problem:

> "[If] in place of p, q, new variables P, Q may be introduced by a canonical transformation such that $H(pq) = \bar{H}(P,Q)$ ... we wish to construct the new wave equation with $\bar{H}$."[108]

If the old equation were $\{H(\frac{\varepsilon\partial}{\partial y}, y) - W\}\phi(y) = 0$ and the new $\{\bar{H}(\frac{\varepsilon\partial}{\partial x}, x) - W\}\psi(x) = 0$, he asked how the new function $\psi(x)$ was related to the original function $\phi(y)$, and he based his answer on a suggestion of Pauli's that

> "If $\phi_n(q)$ is normalised, then $|\phi_n(q)|^2 dq$ is the probability that when the system finds itself in the state n, the coordinate q takes a value in the interval $(q, q+dq)$. ... If q, β are two Hermitian quantum-mechanical quantities, which we shall take here for convenience as both constantly varying, then there will always exist a function $\phi(q_0, \beta_0)$, such that $|\phi(q_0, \beta_0)|^2\,dq$ gives the (relative) probability that for a given value β_0 the quantity q will take a value in the interval (q_0, q_0+dq). The function $\phi(q,\beta)$ of Pauli denotes the probability amplitude."[109]

Whereas Dirac had developed a quantum-mechanical transformation theory and then related it to experiment through a statistical postulate on the interpretation of the mathematical functions, Jordan began with a set of statistical postulates and then built up a transformation theory based upon functions that satisfied these postulates. Whereas Born had based the interpretation of the theory upon the formalism, and Dirac kept interpretation and formulation distinct, Jordan based the formulation upon the interpretation. The theory was not rigorous, as he admitted, and the interpretation only related the formulation to observations and not to anything that might underlie these, but it was the first time that the new quantum mechanics had come anywhere near the usual methodology of physics; for the first time, a physical interpretation of the symbols used preceded the mathematical theory of these symbols. A second major achievement of Jordan's theory was that it included as special cases Dirac's formulation and those of matrix mechanics, wave mechanics, and the Born-Wiener operator theory.

From Pauli's suggestion, Jordan deduced that two postulates should be expected: the function $\phi(q,\beta)$ should be independent of the mechanical nature (Hamiltonian) of the system and dependent only upon the kinematic relations between q and β; and the functions should combine as $\Phi(Q_0, \beta_0) = \int_{\text{all } q} \psi(Q_0, q)\,\phi(q, \beta_0)\,dq$. He noted that it was the probability amplitude ϕ (and not the probability itself, $|\varphi|^2$) that followed the usual combination law for probabilities, and related this to the interference of probabilities in quantum mechanics.

Guided by the above considerations, Jordan suggested a set of postulates to act as the statistical foundations of quantum mechanics. For any q, β standing in a completely determined kinematic relationship with each other, he he postulated the existence of a "probability amplitude" $\varphi(x,y)$ and a "complementary amplitude" $\psi(x,y)$,such that $\varphi(x,y)\psi^*(x,y)$ gave the probability that, given $\beta = y$, q should lie in the range $(x, x+dx)$. To ensure that the probability of β given q was equal to that of q given β , he related the corresponding functions for the interchanged pair, $\bar{\varphi}, \bar{\psi}$, to the original φ, ψ by postulating that $\bar{\varphi}(x,y) = \psi^*(y,x)$, $\bar{\psi}(x,y) = \varphi^*(y,x)$ (so that $\varphi\psi^* = \varphi\bar{\varphi}^T$ etc.) He then postulated that the probability amplitudes should combine interferingly: if φ_1, φ_2 were amplitudes for the facts F_1, F_2 , then that for F_1 OR F_2 (F_1, F_2 exclusive) was $\varphi_1 + \varphi_2$, and that for F_1 AND F_2 (F_1, F_2 independent) was $\varphi_1\varphi_2$. This led to orthogonality relations between the φ, ψ , and to[110]
$\Phi(x,y) = \int \chi(x,z)\,\phi(z,y)\,dz$

Jordan then defined two variables p, q as canonically conjugate if $\rho(x,y)$, the probability amplitude of p, q , was given by $\rho(x,y) = e^{-xy/\varepsilon}$; from this he deduced that for a given value of q all values of p were equally probable. He postulated that, for any variable q , there should exist a canonical conjugate P , and this gave immediately $\left\{x + \varepsilon\frac{\partial}{\partial y}\right\}\rho(x,y) = 0 = \left\{-\varepsilon\frac{\partial}{\partial x} - y\right\}\rho(x,y)$

and $\varphi(x,y) = \int \Phi(x,z)\,\rho(z,y)\,dz$ for the probability amplitude $\varphi(x,y)$ of Q, q . Writing this as $\varphi(x,y) = T\rho(x,y)$, with $T = \int dx\,\Phi(y,x)$, $T^{-1} = \int dx\,\psi^*(x,y)$, he deduced that $\left\{-TxT^{-1} + \varepsilon\frac{\partial}{\partial y}\right\}\varphi(x,y) = 0$, $\left\{T\varepsilon\frac{\partial}{\partial x}T^{-1} - y\right\}\varphi(x,y) = 0$.

Jordan next associated the quantum-mechanical variable q with the operator $T\varepsilon\frac{\partial}{\partial x}T^{-1}$, and defined the addition and multiplication of variables by that of their operators. Identifying the variables Q and q gave $\left(\varepsilon\partial/\partial x + \varepsilon\partial/\partial y\right)\varphi(x,y) = (x-y)\varphi(x,y) = 0$, or in terms of $Q \sim x$, $P \sim 1\varepsilon\frac{\partial}{\partial x}1^{-1}$, the commutation relationship $PQ - QP = \varepsilon$. For any canonical coordinates $\alpha = f(p,q) = TpT^{-1}$, $\beta = g(p,q) = TqT^{-1}$, he developed the equations

$$\left\{f\left(\varepsilon\frac{\partial}{\partial q}, q\right) + \varepsilon\frac{\partial}{\partial \beta}\right\}\varphi(q,\beta) = 0 \quad ; \quad \left\{g\left(\varepsilon\frac{\partial}{\partial q}, q\right) - \beta\right\}\varphi(q,\beta) = 0$$

and similarly for ψ , and by associating g with the Hamiltonian and β with the energy W he showed that the latter of these was the Schrödinger wave equation.

The continued development of the theory, bringing in other formulations as special cases, was long and complicated, but it need not concern us here. We can see from the above how Jordan's theory was founded and how it incorporated, in general terms, the other theories. The theory was not capable of an autonomous existence independent of classical theory, for the Hamiltonian still had to be classically described and the quantum-mechanical terms still had to be associated with classical concepts. But these restrictions were inevitable, and the theory was by far the most satisfactory to date.

In respect of their mathematical content, the transformation theories of Dirac and Jordan were very close, and Dirac wrote to Jordan on December 24 that

> "Dr. Heisenberg has shown me the work you sent him, and as far as I can see it is equivalent to my own work in all essential points. The way of obtaining the results may be rather different though. ... In your work I believe you consider transformations from one set of dynamical variables to another, instead of a transformation from one scheme of matrices representing the dynamical variables to another scheme representing the same dynamical variables, which is the point of view adopted throughout my paper. The mathematics would appear to be the same in the two cases however."[111]

The two theories were developed quite independently, and Dirac, the first to finish, wrote to Jordan that

> "I hope you do not mind the fact that I have obtained the same results as you",[112]

so it is natural to ask how this coincidence arose. A key point is that both authors attributed the physical ideas behind their theories to others, Dirac to Heisenberg and Jordan to Pauli. Since Pauli and Heisenberg were in very close communication, this prompts us to look at their ideas in this period, and it turns out in fact that the development of transformation theory can only really be understood by considering the interplay of ideas between a whole group of physicists: Bohr, Heisenberg and Dirac in Copenhagen, Born and Jordan in Göttingen, and Pauli.[113]

Heisenberg's uncertainty principle

In autumn 1926, this group of physicists were in close contact, and they were all thinking about much the same things: Schrödinger's theory, Born's interpretation, and quantum statistics, in particular. From their thoughts evolved the uncertainty principle, the transformation theories, and Bohr's notion of complementarity. This last was sufficiently personal for discussion on it to be postponed, but the uncertainty principle was intimately linked in its origins with the transformation theories, and I shall therefore consider that now.

Heisenberg's uncertainty principle, which was expounded in a paper of March 1927,[114] applied to any pair of conjugate variables; but Heisenberg was particularly concerned with the relations for energy and time, $\Delta E . \Delta t \sim h$ and position and momentum, $\Delta q \, \Delta p \sim h$. There were three aspects to his conclusions:

(i) Mathematically, the quantum-mechanical formalism allowed the joint determination of p and q (or any other pair of conjugates) with only limited precision.

(ii) Experimentally, the joint determination was limited in the same way.

(iii) Philosophically, this could be interpreted as an essential uncertainty, either of nature itself or of our possible perception of it.

The first two conclusions were fairly simple: Heisenberg showed that the formalism gave, for a statistical error of δq in the determination of q , one of δp in that of p , where $\delta q \delta p = \frac{h}{2\pi}$. Thought experiments, such as the famous one with the γ-ray microscope (which was faulty as originally described, but could easily be corrected) gave results of the same order.[115] More interesting is the interpretation that Heisenberg put upon these first two conclusions. This stemmed from the viewpoint that to clarify the definition of a physical quantity it was necessary to describe an experiment by which this quantity could be measured:

> "If one wants to clarify what is meant by 'position of an object', an electron for example, one has to describe an experiment by which 'position of an electron' can be measured. Otherwise this word has no sense."[116]

Discussing position and momentum as an example, Heisenberg went on from this to the far stronger assertion that since they could not be observed together they could not exist together; the mathematical uncertainty reflected this impossibility, and the uncertainty was "the essential reason for the occurrence of statistical relations in quantum mechanics."[117] The strong assertion that p and q could not exist together seems originally to have been an absolute, almost ontological one,[118] but Heisenberg modified this in his paper. Neither from experiment nor from the formalism, he said, could precise values of position and momentum be determined together; such values were not excluded (in this weaker assertion) from existing in some hidden world, but speculation on this world was deemed

> "useless and meaningless. For physics has to confine itself to the formal description of relations among perceptions."[119]

In his paper, Heisenberg thus expressed uncertainty in terms of epistemology rather than ontology, and this moderation was also apparent in his conclusions on the causality issue, namely that

> "We have not assumed that the quantum theory ... is essentially a statistical theory in the sense that from exact data only statistical conclusions can be inferred. ... However, in the strong formulation of the causal law 'If we know the present exactly we can predict the future' it is not the conclusion but rather the premise that is false. We cannot know, as a matter of principle, the present in all its details."[120]

This description of the causal situation is that which best characterises quantum mechanics, and it was clearly a straight deduction form the theory. Neither causality nor anticausality were suitable to describe the situation, and Heisenberg accepted this effective conclusion without imposing his own philosophical prejuduces, be they causal or anticausal.

Reactions to Schrödinger and Born

In the spring of 1926, Schrödinger had put forward his wave mechanics, interpreting the electron wave as physical reality; in the early summer, Born had developed his statistical interpretation of this formulation, which he preferred to matrix mechanics on account of its utility and its more classical nature. He had related the Schrödinger wave function ψ to a probability $|\psi|^2$ of the system occupying the state described by that function. The function ψ thus represented our knowledge of the system, and at that time it seems also to have had for Born a physical significance, intrinsic in the system.[121] He was impressed by what he saw as a preestablished harmony between experiment and theory, and he inclined to the view that both represented reality: should this not be the case, he then argued, the theory would still be final so far as our knowledge of the sytem was concerned. Born explicitly abandoned causality in individual processes but noted that this was a personal decision and emphasised that the probabilities, $|\psi|^2$, were still causally determined.

While Born was developing his interpretation, Jordan was still working on the matrix-mechanical transformation theory; but he was naturally familiar with Born's work, and shared his rejection of Schrödinger's attempt to interpret wave mechanics classically. Indeed he wrote to Schrödinger that

> "All of the personally known quantum mechanics people are convinced that the basic assumptions of Bohr are still to be retained as generally correct."[122]

Heisenberg recalled that he (and also Bohr) originally found Schrödinger's formulation, as well as his interpretation, misleading and dangerous.[123] He agreed with Born's interpretation, but was nevertheless angry with him for relapsing into the pseudo-classical framework introduced by Schrödinger,[124] and although Born could write to Schrödinger that "your wave mechanics signifies more physically than our quantum mechanics", he had to add that "Heisenberg was from the beginning not of my opinion."[125] At the end of July, however, writing to Jordan that he was "firmly convinced of the incorrectness of the Schrödinger exposition of the physical interpretation of quantum mechanics", Heisenberg admitted that "that Schrödinger's mathematics signifies a great progress is clear."[126]

Heisenberg attended a lecture by Schrödinger in Munich in July, which may have led to the above concession, and which did lead to Schrödinger's being asked to lecture in Copenhagen. Responding to this lecture, Bohr took a line very similar to Heisenberg's, writing to Kronig in October that, although Schrödinger thought that he had got rid of quantum mechanics,

> "This appears, however, to be a misunderstanding, as it would seem that Schrödinger's results so far can only be given a physical application when interpreted in the sense of the usual postulates. Indeed they offer a most welcome supplement to the matrix mechanics in allowing to characterise the stationary states seperately."[127]

In reaction to Schrödinger's ideas, Bohr and Heisenberg were prompted to devote their own attentions to the problem of interpreting quantum mechanics, and they apparently spent the last quarter of the year arguing over the problem; while Bohr was pessimistic and convinced that radically new concepts were still necessary, Heisenberg was optimistic that something might be made of the theory as it stood, so long as inadmissable concepts were avoided.[128]

Dirac must have criticised Schrödinger's attempt to reach a pure wave theory, for Heisenberg wrote to him in May that

> "I quite agree with your criticism of Schrödinger's theory with regard to a wave theory of matter. This theory must be inconsistent, just like the wave theory of light."[129]

He followed this up in a letter to Pauli a fortnight later, arguing that Schrödinger's theory was not a proper development of de Broglie's ideas (a criticism we can probably attribute to Dirac) and that he did not like the idea of a spinning electron smeared out over multi-dimensional space.[130]

Quantum statistics

Alongside the concern with the theories of Schrödinger and Born, another focus of attention in 1926 was the subject of quantum statistics. There were three main papers on this subject, all apparently independent. The first, by Fermi,[131] was published in Zeitschrifft fur Physik in March, but Fermi was working in Florence, well out of the centre of things, and his paper, on ideal gas theory, apparently escaped notice for some months. In June and August respectively, Heisenberg[132] and Dirac[133] approached the application of quantum mechanics to many body problems.

Fermi's paper was concerned specifically with the statistics of an ideal gas, but he did not even mention the Bose-Einstein statistics. Instead, he worked from

> "only the assumption first made by Pauli, and founded on numerous spectroscopic facts, that no two equally valid elements can ever be admitted in a system for which the integral quantum numbers agree. With this hypothesis the state equations and the inner energies of the ideal gas are derived."[134]

The results were, as he showed, in agreement with the Stern-Tetrode values for entropy at high temperatures. He argued that for an ideal gas the molecules each had three degrees of freedom, so that the molecular energy could be decomposed as $sh\nu = h\nu(s_1+s_2+s_3)$, and applying Pauli's exclusion principle the maximum number of molecules in the system with energy $sh\nu$ was therefore restricted to $Q_s = \frac{(s+1)(s+2)}{2}$ [e.g., for $s=0$, $(0+0+0)$; for $s=1$, $\left\{\begin{matrix} 0+0+1 \\ 0+1+0 \\ 1+0+0 \end{matrix}\right\}$ etc.]. He showed that the probability that the number of such molecules was N_s was $P_s = \binom{Q_s}{N_s}$ and so derived the formula for the number N_s, [α, β, constants]

$$N_s = \frac{Q_s \alpha e^{-\beta s}}{1+\alpha e^{-\beta s}} \quad \left[= Q_s \cdot \frac{1}{\left(\frac{1}{\alpha}\right)e^{\beta s}+1}\right].$$

The first to compare the implications of the exclusion principle with those of Bose-Einstein statistics was Heisenberg, but he came to the surprising (and wrong) conclusion that they were equivalent. Attempting to extend the new quantum mechanics to many-body problems, he considered the problem of statistical weights; noting that neither the exclusion principle nor the Bose-Einstein statistics had been derived from the mechanics, he found, by considering a characteristic resonance phenomenon, that both were compatible with it. He then considered two identical bodies, and argued that for any solution to the problem there must be a second solution, obtained by switching the bodies round. He argued that

> "If only one of the two systems occurs in nature, then on the one hand this admits a reduction of the statistical weights [as in the Bose-Einstein statistics]; ... [but] on the other hand Pauli's exclusion of equivalent orbits is of itself fulfilled."[135]

Generalising to a system composed of n identical particles, he noted that there was again a reduction of statistical weights, from $(n!)$ to 1, and he again found the Bose-Einstein statistics to be "in harmony with Pauli's exclusion".

Heisenberg later admitted that he had still been very confused about quantum statistics when he wrote this paper, and we can reconstruct the probable source of this confusion. Given two parts of a system, I & II, he considered two possible solutions: either I was in state X, and II in state Y, say (X,Y), or the reverse, (Y,X). Bose had treated these possibilities as a single state, so reducing the statistical weight from 2 to 1 as Heisenberg said. The exclusion principle forbad the joint existence of (X,Y) and (Y,X) (and also the existence of (X,X) or (Y,Y)), but Heisenberg was confused and inferred that it forbad one of the two states altogether.

Dirac approached a similar problem to Heisenberg, but more mathematically, and from the standpoint of observability. His idea was simply to extend Heisenberg's original observability criterion, and he claimed that

> "[Heisenberg's theory ... enables one to calculate just those quantities that are of physical importance, and gives no information about quantities such as orbital frequencies that one can never hope to measure experimentally. We should expect this very satisfying characteristic to persist in all future developments of the theory."[136]

The criterion of observability became relevant when, for an atom of several electrons, "the positions of two of the electrons are interchanged" and "the new state of the atom is physically indistinguishable from the original one."[137] If the two states were labelled (mn) and $(m'n')$, then Dirac observed that one could not measure the intensity of the transition $(mn) \rightarrow (m'n')$, but only the sum of the intensities in both directions; the two transitions were physically indistinguishable, and only their combined occurrence was therefore measurable. In other words, the possibilities (mn) and $(m'n')$ could only be observed as a single state; . symmetry required that both electrons should have the same coordinates, so only symmetric functions of the coordinates could be observed or represented by the theory. If the eigenfunction for the whole system was $\phi = \psi_m(1)\psi_n(2)$ then $\psi_n(1)\psi_m(2)$ must represent the same state, so there must exist a complete set of eigenfunctions, $\psi_{mn} = a_{mn}\psi_m(1)\psi_n(2) + b_{mn}\psi_n(2)\psi_m(1)$, with $A\psi_{mn} = \sum_{m'n'} \psi_{m'n'} A_{m'n'mn}$ for any operator A. Dirac showed that there were precisely two solutions to this problem, $a_{mn} = \pm b_{mn}$. If $a_{mn} = -b_{mn}$ it followed that $\psi_{mn} = 0$, and the two elements could not exist in the same state, which was Pauli's exclusion principle. If $a_{mn} = b_{mn}$, then $\psi_{mn} = \psi_{nm}$ and the possibilities (mn) and (nm) referred to one and the same state: extended to many particles this was the condition for Bose-Einstein statistics.

Having thus derived the exclusion principle[138] and the Bose-Einstein statistics as the only possible solutions to the quantum problem, and having distinguished between them in terms of symmetry, Dirac considered an ideal gas and derived the statistics appropriate to the exclusion principle just as Fermi had done. If N_s molecules had energy E_s, and if A_s was the number of de Broglie waves (or cells in phase-space) appropriate to this energy, then the probability of the situation occurring was $W = \prod_s A_s / N_s!(A_s N_s)! = \prod \binom{A_s}{N_s}$, and maximising entropy in the usual way gave $N_s = \frac{A_s}{e^{\alpha} e^{\beta E_s} + 1}$. Dirac compared this with the equivalent Bose-Einstein form, $N_s = \frac{A_s}{e^{\alpha} e^{\beta E_s} - 1}$, and thus established the comparison between the two sets of statistics.

Dirac naturally discussed this work with Heisenberg when he arrived in Copenhagen in the autumn, and so cleared up Heisenberg's confusion. Writing to Pauli on November 15,[139] Heisenberg discussed Dirac's work and also an idea of his own, namely that the different statistics were determined, through the symmetry requirement on the overall wave function, by the spin of the particles being considered. Half-integral spins, he said, gave anti-symmetric space functions and Fermi-Dirac statistics, while integral spins gave symmetric space functions and Bose-Einstein statistics. This established the usual characterisation of a particle's appropriate statistics.

I have not been able to look at any correspondence behind the work of Fermi, Heisenberg and Dirac, but the problems were natural ones to tackle, and the approaches of Heisenberg and Dirac reflect the former's attachment to physical reasoning and the latter's mathematical precision and elegance. Conceptually, the most important contribution was Dirac's, and that was very important. In the first place, it provided the first justification of the statistics of light-quanta. These had been associated with confused notions of indistinguishable particles and 'molecules' of light, but Dirac was the first to give the notion of indistinguishability a basis, in the observability criterion, and a characterisation, in terms of observability.[140] In the second place, Dirac's work related both the exclusion principle and the Bose-Einstein statistics to the new quantum mechanics, again for the first time.[141]

Dirac had found the agreement between theory and experiment, in terms of observability, to be "very satisfactory", and his emphasis upon observability (already apparent in his formulation of quantum mechanics) reflects a strong element of phenomenalism. It is not clear what importance he attached to this philosophically, or what status he afforded it, but his work reinforced immensely the importance of the observability criterion in quantum mechanics and, more important still, as a foundation for this theory. In taking the generalised Pauli principle as one of its foundations, quantum mechanics is necessarily a theory of phenomena, and of phenomena only, and this is partly due to Dirac's work. More immediately, this work was also a very important element of the background to the transformation theories, the uncertainty principle, and complementarity.

The origins of transformation theory

The first move towards the transformation theories of Dirac and Jordan appears to have been that contained in a letter from Pauli to Heisenberg, the contents of which were recalled by Heisenberg:

> "Born's interpretation may be viewed as a special case of a more general interpretation: thus, for example, $|\psi(p)|^2 dp$ may be interpreted as the probability that the particle has a momentum between p and $p+dp$."[142]

This was the first part of the idea that Jordan attributed to Pauli, and on which he built his transformation theory. It was eventually published by Pauli as a footnote to a paper on magnetism,[143] which was received on December 16, but the letter to Heisenberg seems to date from the end of October. Writing to Pauli on October 28, Heisenberg thanked him for restoring some physical sense to Born's formalism,[144] and in another letter, on November 4, he went further into the subject:

> "But after this unsatisfactory preamble may I again write you that I am more and more inspired by the content of your last letter every time I reflect on it. So one should say in general: every scheme that satisfies $pq-qp=h/2\pi i$ is correct and physically useful. So one has a completely free choice as to how to fulfill this equation, with matrix operators or anything else. Moreover, your wave functions χ in p-space seem to me to be the Laplace transforms of the Schrödinger functions ψ in q-space. ... But your functions in other spaces, e.g. J and w, are naturally something else. The problem of canonical transformations in the wave representation is thereby as good as solved."[145]

We can now see why there was such a close connection between the physical ideas of Pauli and Heisenberg. Their concerns were naturally similar, namely to understand the Schrödinger-Born theory, to establish its connection with matrix mechanics, and to explore the problem of physical interpretation that it raised. This theory had been restricted to energy solutions and the spatial distributions with which these were linked, but by working the problem in momentum space Pauli extended the probability interpretation to cover momentum distributions and generalised this to get the interpretation used by Jordan. Inspired by Pauli's work, Heisenberg saw that the different formulations of quantum mechanics could be linked with the matrix mechanics, with which he still preferred to work. In particular, the matrix-mechanical transformations could be interpreted in these formulations, including that of wave mechanics. In a paper received on November 6 he noted the connection between transformation matrices and Born's probabilities,[146] and reinterpreting this connection in terms of mean values he presented Dirac with the physical foundation for his transformation theory.

Dirac recalled that he had been looking at the connections between different formulations of quantum mechanics and that in this context, influenced by Lanczos' continuous integral formulation, he had developed the continuous matrix formulation.[147] His basic approach, in which formalism and interpretation were kept separate, was in line with his earlier work on c- and q-numbers, and his basic question, as to what questions could be posed and what answers given in quantum mechanics, was a natural development of this work. From Heisenberg's ideas on the connection between wave and matrix formulations and on the generalisation of Born's interpretation must have come the two elements still needed for the transformation theory: that a general theory could be founded upon transformations, and that the answers provided by this theory would be of the type considered by Heisenberg. The expression of the latter part in terms of observability reflected his concentration upon this notion in the paper on quantum statistics, but may also have arisen in part from discussions with Heisenberg. In particular, Heisenberg had also come to the conclusion, mentioned by Dirac, that p and q were not simultaneously measurable; this idea may have been due to Dirac (as a deduction from the fact that they could not be simultaneously diagonalised), but Heisenberg claimed it as his own idea, and there is no reason to doubt this.[148]

Jordan, who had been working on the transformation theory in matrix mechanics but found himself in an environment dominated by the wave theory, was well placed to build up a general theory as he did, and needed no motivation other than that of Pauli's idea. His reference to the fact that given p all q were equally probable would seem to have been a straight deduction from his theory. It is not unfortunately clear what significance should be attached to his choice of methodology. It may have been a conscious attempt to bring quantum mechanics into line with the usual structure of scientific theories, but he did not discuss the point, and it is more likely that he adopted the methodology he did as a result of his wish to connect the various formulations; the physical predictions were their only obvious point of contact. It was at any rate a significant improvement on Born's methodology, according to which an assertion that the theory reflected (in its limitations) the limitations of nature had been founded on an ad hoc formalism, with no grounding in observation.

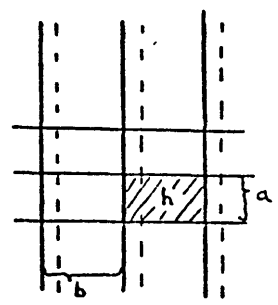
Early in 1927 Jordan discussed the problem of interpretation further in a paper on the physical foundations of quantum theory,[149] and he displayed a strong positivism, but without phenomenalist element that characterised the ideas of Born, Heisenberg, Pauli and Dirac. He concluded that in terms of the formalism there was incomplete causality, and he linked this with the entry of the imaginary constant into the time description. Were the equations describing the laws of nature expressible in real terms, he suggested, we should have complete causality, and were two or more of the coordinates imaginary we should have a lesser degree of causality than we did. As it was, he pointed out that the probability functions were still subject to determinism but that for individual processes one was faced with the "purely statistical nature of the present quantum-theoretical laws."[150] Causality, he observed, could no longer be taken as a priori — but he would not commit himself as to his own expectation on how things would turn out. The current formalism gave incomplete causality, and true to his positivism he would neither accept this as final nor speculate on what might lie behind it.[151]

The origins of Heisenberg's uncertainty principle

Meanwhile, Heisenberg had developed his uncertainty principle. He recalled that it stemmed from the realisation that nature allowed only describable situations, and linked this with an idea he attributed to Einstein, that "it is the theory which describes what we can observe."[152] Einstein had meant this as a limitation we ourselves place upon our observations, but Heisenberg took it more directly, as we shall see.

His thought was provoked by Schrödinger's lectures, and on October 28 he wrote to Pauli about the problem of attributing a joint position and velocity to a particle.[153] He found this to be meaningless since in contradiction with the commutation relationship, and he argued that the very concepts of position and momentum were only meaningful if not specified too precisely. By November 15, he had developed this idea to one that clearly foreshadowed the uncertainty principle:

> "The general division of the phase space into cells of volume h is certainly a correct principle. But, and now comes the thought I have, if you sharply advance the cell walls, and thereby determine how many particles are in each cell, can one then not by choosing adjacent cell walls find the number of atoms in as small cells as one likes? I mean, is the choice of a <u>determined</u> cell wall physically meaningful?"[154]



Heisenberg illustrated this point with a diagram that made it much clearer. If one could determine the distribution with respect to one set of cells, and then, by moving the walls, with respect to another set overlapping the first, could one in that way determine it for the smaller cells created by the overlap? He thought that one could not, and that

> "Perhaps it is so that one can only, e.g., give the ratio of the two cell walls a/b, but not the position of a fixed cell wall."[155]

In other words, one could perhaps determine the shape of a cell of volume h, but not its position: the volume would then be irreducible. He continued to argue that

"The same objection also holds now for the E-t distribution. But for the special case of determined t, again all is in order. I hold it to be very reasonable to study even the special case more closely, out of which will come perhaps something of the kinematic meaning of the matrices.

"I am also in conclusion of your opinion that at the end of the obscure point will be a very clear point. I mean: when space-time is already somehow discontinuous, it makes no sense to talk, e.g., of a velocity $\dot{x}$ at a determined point x. To define velocity, surely one needs at least two points, which may lie in a discontinuous relationship, but not infinitely close. When we talk of the path or velocity we require that these are properly defined in an evidently discontinuous way."[156]

In this letter, Heisenberg progressed well beyond the assertion that p and q were not simultaneously measurable or defineable. His concern with the distribution over the cells in phase space appears to have two possible origins. Having started with the information that simultaneous measurement was restricted by the commutation relationships, he may have studied the unit cells as being the only other physical realisation of the constant h. But the letter also dealt with Fermi-Dirac statistics, which he had been discussing with Dirac and which may well have drawn his attention to the cell distribution, upon which quantum statistics depended. Dirac's work was also paralleled in the extension of Heisenberg's ideas to pairs of canonical conjugate coordinates other than p and q, which followed Pauli's reformulation of wave mechanics in momentum space.

Heisenberg's conclusion can be closely linked with the uncertainty principle by identifying the sides of the cells with Δp, Δq, but he was still some way from a clear formulation of the principle. He seems to have let the problem rest for a while, but he wrote to Pauli again on February 5 1927 with the information that[157] he was "occupied with the logical foundations of the whole $pq-qp$ swindle." He gave no further details, but he did comment on the proofs of Jordan's philosophical paper. He thought this "right handsome", but was worried that Jordan talked of such things as the "probability of an electron being at a determined point" when "the concept 'path of an electron' is not properly defined."[158] This could be taken as a criticism of Born's interpretation also, and it explains why Heisenberg was so delighted at Pauli's bringing some physical sense to this interpretation some months earlier: with Pauli he accepted an interpretation that treated the probability of a coordinate q being in a given range, but not one that referred to the kinematic notion position being determined. Throughout the period under consideration, Heisenberg was very much concerned with the problem of defineability of quantum quantities, and in particular with the problem of an electron path, which had been central to his thoughts for some years.

Throughout the autumn and winter, Bohr and Heisenberg had argued about the problem of interpreting quantum mechanics, and Heisenberg recalled that it was only when Bohr took a holiday in February and left him in peace that he managed to sort out his thoughts sufficiently to formulate the uncertainty principle.[159] [160] The main ingredients of the principle had all been there since before Christmas, but he needed a certain isolation, just as he had done to create his new kinematics. He had realised for a long time that p and q were not simultaneously describeable by the theory, and had equated this with their not being simultaneously measurable; their joint determination was limited by the commutation relationship, and he had connected this with an essential irreducibility of unit cells in phase space. A clear head was all that was needed to express this in terms of uncertainty and to connect it on the one hand with a statistical uncertainty (easily obtainable from the Dirac-Jordan statistical transformation theory, once this had been absorbed), and on the other hand with an experimental uncertainty.

The result was the subject of a long letter to Pauli[161] written on February 23. Once again Heisenberg concentrated on the problem,

> "What does one understand by the words 'position of the electron'? This question can be replaced, according to the well-known rule, by the question 'how does one determine the position of the electron?'"[162]

He then analysed the second question much as in his published paper, arguing that (to quote the paper)

> "At the moment of the position determination,i.e. when the quantum of light is being diffracted by the electron, the latter changes its momentum discontinuously. This change is greater the smaller the wavelength of the light, i.e. the more precise the position determination. Hence, at the moment when the position of the electron is being ascertained its momentum can be known only up to a magnitude that corresponds to the discontinuous change."[163]

This change was related to the commutation relationship, confirming that this was the basis of Heisenberg's thinking. He continued to argue that

> "So, it has no meaning when we talk, e.g., of the 1S "orbit" of the electron in the hydrogen atom. ... The word 1S "orbit" is thus as it were purely experimental, i.e. meaningless without knowledge of the theory."[164]

Later in the letter, Heisenberg also analysed the experimental determination of velocity, which again led to the uncertainty relation, and extended the idea to all pairs of canonical conjugate coordinates. He worked out the statistical uncertainty according to the Dirac-Jordan theory, which made this possible for the first time.

The letter to Pauli is particularly interesting in respect of the interpretation that Heisenberg put upon the uncertainty principle. We have seen how Dirac and Jordan had taken ideas from Heisenberg and Pauli, and Heisenberg now drew from them, writing that

> "One can, like Jordan, say that the laws of nature are statistical. But one can, and this seems to me essentially more profound, say with Dirac that all statistics are first introduced through our experiments."[165]

As for assigning definite values, e.g. to the path of the electron, Heisenberg concluded that

> "The solution can now, I believe, be precisely expressed in the proposition: the orbit arises primarily from the fact that we observe it."[166]

It is not absolutely clear what Heisenberg believed when he wrote this letter. He explained to Pauli that the whole point of writing the letter was to clarify his thoughts, and he may not have been sure what he thought himself. The opinion ascribed to Jordan was presumably that already discussed, namely that the laws that described nature in the current theory were statistical, and that one could not go beyond that.[167] Dirac had asserted that statistics were a part neither of theory nor experiment, but that they had to be introduced to compare the two; we have no conclusive evidence that he thought them to be a necessary, irremovable, feature , but his remarks suggest a tendency in that direction, and towards a phenomenalist epistemology. In the letter to Pauli, Heisenberg claimed agreement with Dirac, but he seems to have gone further than we know

Dirac to have done,[168] in implying that there was no such thing as a fixed electron path unless fixed by our observations. This was a result of his continued assertion that one could not apply the concept 'electron path' to nature; he deduced that it was a concept created by man, and one that could only be imposed by man, through his observations. Naively, it seems to suggest a phenomenalist ontology, that nothing exists beyond our observations, but we can see that it was more subtle than that. Heisenberg was prepared to accept the existence of a world independent of observation, but insisted that it was not a world to which such concepts as 'position' applied. His phenomenalism was still on an epistemological level, but it was extended to exclude ontological speculation, if not objective existence itself.

This extension was not clear in the paper itself, but the basic epistemology was reflected there in his discussion of causality. A causal world was not excluded but merely irrelevant, and even without such a world causality was not abandoned but again irrelevant. Heisenberg was at pains to emphasise that his theory was not causal, but that it was not anticausal either, and we may note that this situation, characterised by the absence of a space-time description, was that which had from the beginning characterised quantum mechanics.

In upholding the above interpretation, Heisenberg was in disagreement with Jordan, who remained philosophically neutral, and may have gone slightly further than Dirac. Born's position, as expressed the previous summer, was similar but different. He too took phenomenalism beyond epistemology, and suggested that if a 'real' world did exist (which he, tending to deduce from the success of his formulation a phenomenalist ontology, doubted) the current theory was nevertheless final. Preferring to stick to classical concepts, however, he abandoned causality rather than speculation, and continued to equate these concepts with reality. Bohr, on the other hand, was even more strongly opposed to the classical concepts than Heisenberg; he objected to Heisenberg's concentration upon the particle concept, and to the idea that particles could somehow be created, and it was this attitude that led to his principle of complementarity.[169]

IV.5. COMPLEMENTARITY

After Geiger's results had wrecked his long held position against light-quanta, Bohr seems to have retreated into pessimistic contemplation. He took no part in the developments of quantum mechanics that followed, and he was only spurred into action, Heisenberg recalled, after Schrödinger had lectured in Copenhagen. Even then, as Heisenberg took part optimistically in the developments we have discussed, Bohr remained pessimistic and the relationship between these two physicists grew a little tense.[170] Bohr concentrated on the general philosophical problem of observation rather than on the quantum-mechanical formalism, and seems to have criticised Heisenberg on much the same grounds as Heisenberg himself had criticised Born and Jordan, for concentrating on the formalism rather than on the underlying physical problems. Heisenberg was trying to regain the physical situation from the formalism that seemed to describe it so perfectly, but Bohr felt that the formalism could only evade the central physical paradoxes, and in particular the wave-particle duality which still dominated his thought. Through his study of the paradoxes and of the problem of observation, Bohr eventually derived his principle of complementarity. Heisenberg recalled that he thought it out while on holiday in February,[171] but the roots go back well beyond that, and the principle was not publicly announced until a conference in Como in September.[172] Since complementarity seems to have been a last resort to Bohr, with its positive aspects dawning on him only slowly, his annunciation of it on his return from holiday would seem to have been almost certainly a response to Heisenberg's ideas on uncertainty. Heisenberg's interpretation forced Bohr's hand, for he was convinced that it was wrong (or at least not wholly right) and could only counter Heisenberg's conviction to the contrary by providing an alternative, which he had so far refrained from doing. Since this alternative was very much a last resort, it took him many more months to convince himself that it was necessary, but once convinced he built his whole philosophy upon it.

Complementarity grew to have a very much wider meaning than when it was first eenunciated at Como, but at that time Bohr could sum it up by claiming that

> "The very nature of quantum theory then forces us to regard the space-time coordination and the claim of causality, the union of which characterises the classical theories, as complementary but exclusive features of the description, symbolising the idealisations of observation and definition respectively."[173]

This sentence contained the essence of the complementarity interpretation, and it also contained the claim that the interpretation was "forced" by the very nature of the quantum theory, i.e. that it was more than an interpretation in the usual sense. The reason for this was, according to Bohr, that

> "our usual description of physical phenomena is based entirely on the idea that the phenomena concerned may be observed without disturbing them appreciably. ... [But] the quantum postulate implies that any observation of atomic phenomena will involve an interaction with the agency of observation not to be neglected."[174]

The classical assumption was that one could observe a system precisely, while interfering with it only to an extent that could be taken in the limit to zero; according to the quantum postulate, however, the interference had to be quantised, and so could not be taken to this limit. Hence one could not at the same time both observe a system and define it:

> "On one hand, the definition of the state of a physical system, as ordinarily understood, claims the elimination of all external disturbances. But in that case ... any observation will be impossible, and , above all, the concepts of space and time lose their immediate sense. On the other hand, if in order to make observations possible we permit certain interactions with suitable agencies of measurement, not belonging to the system, an unambiguous definition of the state of the system is naturally no longer possible, and there can be no question of causality in the ordinary sense of the word."[175]

This was, Bohr realised, the familiar problem of the observer and the observed. If we observe the system, we interact with it. To define the system, we have to know the exact extent of the interaction, and therefore have to measure the effect of this interaction on our measuring apparatus. This involves an observation of the extended system including the original 'observer' or apparatus, and so on. Finally, the chain must reach the mind of the observer, which must itself be inside the system if this is to be defined, and outside if it is to be observed. Where we draw the line between observer and observed is, Bohr realised, purely a matter of convenience, so long as none of the interactions can (as in the classical theory) be neglected.

Having described his complementarity interpretation, Bohr deduced from it some conclusions on the physical situations of quantum theory, in particular the wave-particle duality and Heisenberg's uncertainty principle. He derived the latter in two ways, once from the former, and once from the observation that velocity was an abstraction from our experience of position, and could not itself be measured but had to be deduced from a pair of position measurements. Heisenberg had made a similar observation, and had deduced that it was meaningless to speak of the velocity of a particle at a fixed point. Bohr now argued that

> "The fixation of [a particle's] position means [since we must observe it] a complete rupture in the causal description of its dynamical behaviour, while the determination of its momentum always implies a gap in the knowledge of its spatial propagation. Just this situation brings out most strikingly the complementary character of the description of atomic phenomena which appears as an inevitable consequence of the contrast between the quantum postulate and the distinction between object and agency of measurement, inherent in our very idea of observation."[176]

He also took the wave-particle duality as an example of the situation arising from complementarity, writing that

> "The two views of the nature of light are ... to be considered as different attempts at an interpretation of experimental evidence in which the limitation of the classical concepts is expressed in complementary ways."[177]

From this he deduced uncertainty much as had Heisenberg, the wave being associated with space-time coordination and the particle with causality (through enrgy conservation, etc.). In the wave language, the measurement of one canonical variable required a very short wavelength, and that of the other a very low frequency.

Bohr's discussion was confused by the fact that different concepts represented the same idealisations in different situations, but the complementarity principle may be expanded as follows:

> The quantum theory describes that which may be both defined and observed; since precise observation and definition are in general not possible together, it involves an uncertainty, and apparent contradictions follow from our attempts to provide exact pictures. In the case of duality, we can arrive neither at an exact particle picture nor at an exact wave picture: these are however the abstractions upon which our "interpretation of experimental facts ultimately depends."[178] The occasions where quantum theory provides an exact description are "limited to just those problems, in which in applying the quantum postulate the space-time description may largely be disregarded, and the question of observation in the proper sense therefore placed in the background."[179] The quantities treated as directly observable in matrix mechanics are, said Bohr, just those that do not press observation too far.

Bohr claimed that his interpretation was forced, and his arguments were convincing: given the quantum postulate as a final, essential restriction upon observation, complementarity seemed inevitable. Like Born and Heisenberg, he argued that, regardless of any reality beyond that described by the quantum theory, our observations would remain restricted by that theory, but he went beyond both Born and Heisenberg. Born had assumed a reality in terms of classical concepts, but had imposed a restriction as to what was observable.

Heisenberg had contended that reality was not subject to classical concepts until observed, when a classically conceivable phenomenon was created, subject to the uncertainty principle. Bohr argued that reality was not and could not be made subject to classical concepts, and that the two ideals of (classical) defineability and (classically interpreted) observability were mutually incompatible. Whereas Heisenberg had equated observability and defineability, he saw them as opposites. Thus, though the interpretation may have been forced, it clearly contained a strong philosophical element; and if it was forced, we might ask how it was that he reached it only when he did, for the quantum postulate, on which it was based, had been around for a long time.

Bohr's reference to "a renunciation as regards the causal space-time coordination"[180] is strongly reminiscent of his earlier discussions on the impossibility of a causal space-time description, and also reminiscent of his earlier ideas is the suggestion that

> "The quantum theory is characterised by the acknowledgement of a fundamental limitation in the classical physical ideas when applied to atomic phenomena ... Our interpretation of the experimental material rests essentially upon classical concepts."[181]

In 1923 he had written that

> "From the present point of view of physics, however, every description of natural processes must be based on ideas which have been introduced and defined by the classical theory",[182]

but that

> "A description of atomic processes in terms of space and time cannot be carried through in a manner free from contradictions by the use of conceptions borrowed from classical electrodynamics."[183]

In 1924, he had rejected a "causal description in space and time"; but he had not then drawn the conclusion of complementarity.[184]

The crucial point seems to be that he had not then accepted the wave-particle duality; he still believed in the pure wave nature of light, and did not see the quantum postulate as an essential limitation. He had abandoned the causality associated with light-quanta in favour of the space-time description afforded by waves, but had not been forced to accept the problem of satisfying <u>both</u> causality <u>and</u> the need for a space-time description. He was only prompted to investigate this problem after Geiger's results had forced him to accept the necessity of the particle picture alonside the wave one.[185] Having accepted this, the crucial ingredient still needed for complementarity was the realisation that the problem was one of observation, and this would have arisen out of the discussions in Copenhagen in autumn 1926, for both Dirac and Heisenberg based theories upon the notion of observability. Finally, the rejection of Heisenberg's ideas as philosophically unsatisfactory and dependent too much on the formalism would have left the way open for the complementarity principle.

Having said this, it would be remarkable if Bohr had developed his ideas independent of any philosophical influences. Certainly such influences would have helped him accept the notion of complementarity, even if they had no part in its original inception. This is important because, as we have noted, there was a considerable delay between the private and public presentations of the idea; and it cannot have been an easy one to accept, for it implied that a final limitation on the scope of physics had been reached, and that our observations had reached a point where the interaction with the observed system stood as an insuperable barrier to further progress. As we have already noted in respect of Born and Heisenberg, it is a dangerous step indeed to assert that science has reached its limits, and Bohr, with his interest in philosophical matters, would have been very aware of this. He could perhaps justify his conclusions rationally, but some external support may well have been necessary for him to accept them emotionally.

We have noted that external influences may well have played a part in some of Bohr's earlier ideas, and in respect of complementarity he observed that the situation

> "bears a deep-going analogy to the general difficulty in the formation of human ideas, inherent in the distinction between subject and object",[186]

so we may be fairly confident that philosophical and psychological ideas did have some bearing on his acceptance of complementarity. In fact the possible influences in this respect are so many that some influence would seem to have been inevitable, while to isolate a particular one is impossible: even Shakespeare, in Richard II, discussed the situation.[187] In these circumstances, and within the limitations I have set on the present treatment, I shall not pursue the question at any length; but three possibilites should be noted in particular. One is Høffding's discussion of Kierkegaard's philosophy as being one of thesis and antithesis but without any synthesis. He wrote that the

> "leading idea was that the different possible conceptions of life are so sharply opposed to one another that we must make a choice between them, hence his catchword either-or."[188]

This idea must have been familiar to Bohr, for Høffding was his philosophical mentor, and Kierkegaard one of his favourite philosophers. The second possibility is William James' famous work on the principles of psychology, in which James wrote that

> "It must be admitted, therefore, that in certain persons, at least, the total possible consciousness may be split into two parts which coexist but mutually ignore each other, and share the objects of knowledge between them. More remarkable still, they are complementary. Give an object to one of the consciousnesses, and by that fact you remove it from the other or others."[189]

On the subject of observation, James wrote that

> "If the passing thought be the directly verifiable existent which no school has hitherto doubted it to be, then that thought is itself the thinker."[190]

We know that Bohr read this, though there is some dispute as to whether was before or after the inception of his complementarity idea. Since Høffding had visited James in 1904, and had elicited from him a preface for a book of 1905, it seems very likely that Bohr would have read James' work around this time, when his philosophical studies were at their peak. Finally, James' second idea is closely related to one expressed in Møller's Tales of a Danish student, a book that we know gave Bohr great pleasure. Møller wrote that

> "On many occasions man divides himself into two persons, one of whom tries to fool the other, while a third one, who in fact is the same as the other two, is filled with wonder at this confusion. In short, thinking becomes dramatic, and quietly acts the most complicated plots with itself, and the spectator again and again becomes actor."[191]

IV.6. THE FORMULATION AND INTERPRETATION OF QUANTUM MECHANICS, CIRCA 1927

The final formulation

The interpretations of Bohr and Heisenberg were based upon the Dirac-Jordan transformation theory, but this theory still required further development. Hilbert had taken an interest in quantum mechanics late in 1925, but the following year he had become very ill with pernicious anaemia. It was already a few years since he had done much active research and his death was now more or less expected,[192] but a miracle cure was proclaimed, a transfusion performed, and with some of Courant's blood inside him he recoverd remarkably[193] — just in time to take up the transformation theory. By a fortunate coincidence, one of his two assistants (who prepared a paper on the subject under his direction) was J. von Neumann, who was probably the most able mathematician of the century, and who was able to follow Hilbert's work through to a conclusion with a speed and accuracy that could not have been matched by anyone else. A part of von Neumann's treatment was not completed until 1929, but in essence his formulation, which remains to this day the most complete, was published in 1927.

The paper by Hilbert, von Neumann and Nordheim was prepared in the spring of 1927, and was a clarification and elaboration of the Dirac-Jordan theory.[194] The emphasis was on Jordan's work (it was more general, and he was there in Göttingen), but the authors combined the different approaches of Jordan and Dirac: they established a set of physical axioms in analogy with Jordan's statistical postulates, but they developed the mathematical transformation theory independently, associating the two sides as Dirac had through an explicit interpretation. This was a retreat from Jordan's classical methodology to one that was more typical of quantum theory, but they wanted maximum freedom to develop the formalism, and presumably felt that statistical postulates might be restrictive in this sense. In this they were right, for it has since proved difficult to equate the set of functions allowed by the formalism with that of physically realisable ones: Jordan's was the first and last use of classical methodology in quantum mechanics.

The physical axioms adopted were essentially Jordan's statistical postulates. The authors required that for any two mechanical quantities, $F_1(pq)$ $F_2(pq)$, there should exist a function $\phi(xy; F_1 F_2)$ such that $\phi\bar{\phi} = \omega(xy; F_1 F_2)$ was the relative probability that, given $F_2 = y$, F_1 was in the range $(x, x+dx)$. They defined ϕ as the relative probability amplitude. They required that the probabilities should not depend upon the nature of the mechanical system, or upon the coordinate system; they insisted that the probability $p(x;y) = \phi(xy;xy)$ should equal $p(y;x)$; and they specified the probability combination law:

$$\phi(xz; F_1 F_3) = \int \phi(xy; F_1 F_2)\, \phi(yz; F_2 F_3)\, dy$$

Quite independent of these physical axioms, the authors then developed a theory of transformation operators along the lines of Jordan's theory, but simplifying his treatment. To get a quantum theory of physics, the operator theory and the physical axioms had to be related to each other, and for this they postulated that $\phi(xy; qF)$ should be associated with the kernel of the canonical transformation $q \rightarrow F(q)$. This satisfied all the physical axioms. The reality condition on $\phi\phi^{-1}$ led to the requirement that the operators should be Hermitian, and the authors suggested that "we suppose that only Hermitian operators have a significance as associated with physical realities." The various formulations of quantum mechanics followed as in Jordan's theory.

Hilbert's presentation was far clearer than either Dirac's or Jordan's, both in respect of the physical content and in respect of the mathematics. It represented a retreat from Jordan's methodology, but this was necessary so as not to upset the rigour of the mathematical theory, and although conceptually significant it did not involve a return to Born's practice (and Heisenberg's) of basing the interpretation completely upon the mathematical formalism.

In Hilbert's theory, the different formulations of quantum mechanics were still connected as they had been in the Dirac-Jordan theory, through the individual variables. In particular, the connection beteen continuous and discrete matrix formulations still depended on the δ-function. Von Neumann, as a pure mathematician, had a strong aversion to this function[195] (which he did not consider to be true function at all), and in his development of the theory he avoided it by connecting not the variables themselves but the spaces on which they were defined. Both the continuous function space and the discrete sequence space were, he showed, particular examples of a more general Hilbert space. By constructing quantum theory as a theory of functions defined on this general space he brought all the previous formulations into one completely general, and completely rigorous, structure.[196]

Von Neumann's work is a very important part of the technical history of quantum theory, and even more so of the history of pure mathematics. It also provided the foundation for later discussions of the conceptual niceties of quantum theory, but at this stage it had no contribution to make to the conceptual development of quantum mechanics. Far more important in this respect were the problems of interpretation being discussed at the same time.

The 1927 Solvay Congress and the interpretation of quantum mechanics

The various strands of the development of quantum concepts were all brought together at the fifth Solvay Congress, in October 1927.[197] After the dramatic developments of the previous years, this was the first occasion on which almost all the main participants came together: the atmosphere was retrospective, as in a dressing room after a play, and Heisenberg recalled[198] that those present felt it marked the end of an era. I shall also take it as the end of my study, and I shall conclude my survey of the conceptual development of quantum mechanics with the discussions at the Congress and the related correspondence. But first we must consider those interpretative developments prior to the Congress that came from outside the Copenhagen-Göttingen circle on which we have been concentrating.

Schrödinger's interpretation

Schrödinger could not hope to defend a wholly classical interpretation of quantum mechanics, but he was determined to eliminate the notion of an essential discreteness, and in November 1926 he complained to Born that the Göttingen school were

"too deeply under the spell of those concepts (e.g. stationary states, quantum jumps, etc.) that have woven themselves securely into our thoughts in the last 12 years." [199]

He preferred to interpret quantum phenomena as classical resonance effects, and he pursued this aim in a paper on "energy exchange according to wave mechanics" in 1927.[200]

In this paper, Schrödinger gave a perturbation theory treatment of the interaction between two microscopic systems, and this provided the technical background he needed to give public utterance to views he had long held in private on the irrelevancy of energy as a microscopic concept. As early as May 1926, he had written to Planck that

"The concept 'energy' is something we have derived from macroscopic experience and really <u>only</u> from macroscopic experience. I do not believe that it can be taken over into micromechanics just like that, so that we may speak of the energy of a single particle oscillation. The energetic property of the individual particle oscillation is <u>its frequency</u>." [201]

The following month he had written at great length on the same subject to Lorentz,[202] and he now concluded from his calculations that

"We thus find that without assuming discrete energy levels and quantum exchange of energy, and even without having to consider any meaning for the proper values other than <u>frequencies</u>, we can give a simple explanation of the fact that physical interaction chiefly takes place between <u>those</u> systems in which, according to the older conception, "the same energy element occurs"." [203]

Thus Schrödinger refused to accept a necessary discreteness of energy, or to abandon wholesale the classical ideology: instead he simply introduced a new concept to replace that of energy in microscopic situations.

<u>De Broglie's theory of double solution</u>

De Broglie's interpretation is often linked with Schrödinger's under the heading 'semi-classical', but this is misleading, for it was only classical in so far as it sought to retain causality: otherwise it was radically new. After two short preliminary papers in August 1926[204] and January 1927,[205] de Broglie presented his theory of double solution in a paper of May 1927.[206] We may recall that his original thesis had been an attempt to reconcile the wave and particle pictures of light; the particle picture had initially dominated, but after he had associated each particle with a wave motion, he came to see the latter as fundamental and the particles as secondary manifestations of basic wave forms. In August 1926, he began to look more closely at how the particles might be derived. Looking at the wave equation, $\Box u \equiv \nabla^2 u - \frac{1}{c^2}\frac{\partial^2 u}{\partial t^2} = \frac{4\pi\nu_0^2 u}{c^2}$, he found, as well as the usual continuous solutions, other solutions with singularities which, he suggested, might correspond to light-quanta. By January 1927, he was sufficiently encouraged to restore the particle picture to its original position of dominance, arguing that

"In micromechanics as in optics continuous solutions of the wave equation provide merely statistical information; an exact microscopic description undoubtedly requires the use of singularity solutions representing the discrete structure of matter and radiation." [207]

By May, his new ideas had been fully worked out, and he had settled back into a dualist position on the wave-particle problem, writing that

> "The object of the wave mechanics is to create a synthesis embracing both the dynamics of a material particle and the theory of waves as conceived by Fresnel. On the one hand, the effect of this synthesis must be to introduce the idea of points of concentration of radiant energy into optics ...; on the other hand it must introduce the conception of the theory of waves into our picture of material particles."[208]

Although this dualism had always been at the back of de Broglie's work, this was the first time that he had expressed it so explicitly. In part this may reflect a change in the experimental situation, for both the wave nature of matter and the particle nature of light had received strong confirmation since his original thesis, but it seems primarily to reflect the erratic evolution of his ideas.

The mathematical basis of the theory of double solution was in line with the work in the previous two papers. De Broglie sought solutions of the wave equation of the form

$$u(x,y,z,t) = f(x,y,z,t)\cos 2\pi\nu\left[t - \frac{z}{V} + \tau\right]$$

$$[V = c/\beta\ ;\ \nu = \nu_0/\sqrt{1-\beta^2} = W/h\ ] \qquad = f(x,y,z,t)\cos\frac{2\pi}{h}\phi(x,y,z,t)$$

and he found first a group of solutions that "correspond to the old mechanics in this sense, that the phase [ϕ] is proportional to the Hamiltonian action."[209] These were singularity solutions corresponding to $\Box f = 0$. He also found solutions corresponding to non-zero values of $\Box f$, and not therefore to solutions in the old dynamics, and he deduced that the wave equation was much richer in content than this dynamics. Looking more closely at the case of a swarm of material particles with the same velocity and phase, he found a continuous solution of the wave equation that corresponded to the individual particle singularity solutions, namely $\Psi(x,y,z,t) = a\cos 2\pi\nu(t - vz/c^2)$, with a constant, and he concluded, moreover, that the swarm of particles could be looked upon (if one related $a^2 d\tau$ to probability rather than to density) as "consisting of the series of possible positions of a single particle."[210]

He next extended his analysis to include a constant external field, and for the wave equation

$$\Box u + \frac{4\pi i}{h}\frac{F(x,y,z)}{c^2}\frac{\partial u}{\partial t} - \frac{4\pi^2}{h^2}\left[m_0^2c^2 - \frac{F^2}{c^2}\right]u = 0 \qquad \ldots [X]$$

he found singularity solutions

$$u(x,y,z,t) = f(x,y,z,t)\cos(2\pi\phi/h) \qquad , \ldots [\dagger]$$

and the continuous solution

$$\Psi(x,y,z,t) = a(x,y,z)\cos 2\pi\left(\nu t - \frac{1}{h}\phi_1'(x,y,z)\right) \qquad . \ldots [\ddagger]$$

The former gave the relation

$$\frac{1}{f}\Box f = \frac{4\pi^2}{h^2}\left[\Sigma\left(\frac{\partial\phi_1}{\partial x}\right)^2 - \frac{1}{c^2}(W-F)^2 + m_0^2c^2\right] \qquad , \ldots [*]$$

$[\phi_1 = Wt - \phi\ ;\ W = \frac{\partial\phi}{\partial t} = h\nu\ ]$

and the latter gave

$$\frac{1}{a}\nabla^2 a = \frac{4\pi^2}{h^2}\left[\Sigma\left(\frac{\partial\phi_1'}{\partial x}\right)^2 - \frac{1}{c^2}(W-F)^2 + m_0^2c^2\right] \qquad . \ldots [\S]$$

In the case of the first terms of the expressions [*], [§] being negligible, [†] gave the formula of classical mechanics and [‡] that of classical optics: ϕ_1 and ϕ_1' were thus identical, being Jacobi's function. De Broglie next proposed that ϕ_1 and ϕ_1' should still be identical when these terms were not negligible, i.e. that $\frac{1}{f}\Box f = \frac{1}{a}\nabla^2 a$:

> "We shall call this postulate the "principle of double solution" because it implies the existence of two sinusoidal solutions of [X] with the same phase factor, the one involving a point singularity and the other, on the contrary, a continuous amplitude."[211]

De Broglie extended his analysis to the case of a variable field, and considered the Newtonian approximation of his theory, finding that

> "Jacobi's equation can be written in its usual form <u>without loss of rigour</u>, provided that we attribute the variable proper mass $M_0 = \sqrt{m_0^2 - h^2 \Box a / 4\pi^2 c^2 a}$ to the moving body equivalent to introducing besides the classical field a potential energy term εc^2, $M_0 = m_0 + \varepsilon$."[212]

The idea of a variable mass gained importance in de Broglie's later worked, while the quantum potential became an important feature in many later interpretations of quantum mechanics.

De Broglie's interpretation thus introduced several important innovations, but of these one was particularly significant. When two types of solutions existed to a set of equations it was traditional to choose one of them as correct. It was unheard of to insist, as did de Broglie, that <u>both</u> should be simultaneously correct and physically meaningful. The classical ideology of a consistent structural description did not allow a physical system to take two different forms at the same time, and even Bohr insisted that complementary solutions were idealisations and not true simultaneous existents. De Broglie realised the conceptual problem that his interpretation faced, and he suggested that the connection between the two solutions

> "might be broadly expressed as follows: the continuous solutions would give a statistical representation of the displacement of the singularities corresponding to real solutions, and they would consequently enable the "probabilty of presence" of a singularity in a given volume of space where the motion takes place to be calculated."[213]

Thus one could visualise the wave as having a purely statistical significance, and de Broglie encouraged such a visualisation by discussing the material particle as an "essential reality", its motion "completely determined as that of a singularity in the amplitude of a wave which is propagated",[214] and by talking of "the probability that would be obtained by considering continuous waves" as resulting when the "initial conditions are ignored."[215] But he also visualised the wave as having an active role, as being a guiding wave:

> "But if we do not wish to appeal to the principle of double solution, it is admissable to adopt the following point of view: assume the existence of material particles and of the continuous wave represented by the function Ψ as distinct realities, and postulate that the motion of the particle is determined as a function of the phase of the wave. ... The continuous wave is then thought of as directing the motion of the particle: it is a guiding wave."[216]

He suggested that if one had to introduce a physical visualisation other than that of the double solution (in which wave and particle were on an equal footing), then one should think of the wave as guiding particles rather than as resulting statistically from them; but both visualisations were lacking in symmetry and ultimately unsatisfactory. He admitted that one could adopt the guiding wave picture provisionally and, by simply postulating the mathematical equation used to get from wave to particle,

> "avoid having to justify it by the principle of double solution; but his, I believe, can be only a provisional attitude. The corpuscle will doubtless have to be <u>reincorporated</u> into the wave phenomenon, and we shall probably be led back to ideas analogous to those above."[217]

<u>Miscellaneous interpretations</u>

There were also several other attempts to interpret quantum mechanics in

1926-7. In 1926, Lewis put forward his ideas on a time-symmetric theory based on corpuscular quanta.[218] This may be linked to Tetrode's earlier theory,[219] but appears to have been independent of it. Lewis observed that the two views on the nature of light were both proven to be correct in some sense, and that this suggested that a revision of our concepts was necessary. Arguing that the basic equations of physics were symmetric, and that unidirectional time was without justification at the microscopic level of radiation processes, he proposed to treat all elementary radiation processes as reversible and time-symmetric and suggested that

> "an atom never emits light except to another atom, and that in this process ... the atom which loses energy and the atom which gains energy play ... symmetrical parts."[220]

He hoped that this condition would give the interference effects required. Since the theory was applied only to radiation, which was distinguished explicitly from matter, the wave phenomena of matter were not accounted for, but Lewis made no mention of this problem. Though his theory has since been taken up by others and developed quite considerably, it was dropped fairly soon after its inception as a result of heavy criticism by Einstein.

Oscar Klein[221] and Louis de Broglie[222] both tried in this period to develop Kaluza's five-dimensional unified field theory[223] so as to incorporate quantum mechanics, but these attempts had no significant outcome. Madelung[224] and Korn[225] both suggested that Schrödinger's wave mechanics should be seen as a theory of hydrodynamic flow; but although the hydrodynamic equations were similar to those of wave mechanics these interpretations suffered from the same objections as did Schrödinger's, and do not seem to have been taken seriously in this period.

The fifth Solvay Congress

By the time of the Solvay Congress in October 1927, there were almost as many interpretations as there were physicists. With the exception of Jordan, all those physicists who had been most concerned with the recent developments of quantum mechanics were present, including Bohr, Heisenberg, Pauli, Born, Dirac, Einstein, Schrödinger and de Broglie; and since Jordan still maintained a methodological positivism and a neutral philosophical position, his absence does not affect our discussion. Einstein and Pauli confined themselves to criticism, but all the others mentioned expounded positive views on the problem of interpretation. Leaving aside the hydrodynamic, five-dimensional and time-symmetric theories as relatively unimportant in the present context, the following interpretations were discussed.

(1) Schrödinger repeated his interpretation of quantum mechanics as a classical theory of resonance effects, avoiding energy quantisation by dismissing energy as being a concept unsuitable for microscopic systems. The fact that his waves were complex and in multi-dimensional space does not seem to have bothered him, and he seems to have retained the hope, despite evidence to the contrary,[226] that the waves could form localised packets corresponding to particles. The earlier criticisms of his interpretation by Born, Bohr, Dirac and Heisenberg were repeated and shared by Lorentz,[227] while de Broglie argued that the use of waves in configuration space was meaningless, especially as with

no point-particles Schrödinger had no physical coordinates upon which such a space could be constructed:

> "Propagation in a configuration space of purely abstract existence is, in fact, out of the question from the physical point of view. The wave representation of our system ought to involve N waves propagating in real space instead of a single wave propagated in the configuration space."[228]

Einstein was less antagonistic but he insisted, writing to Schrödinger in 1928, that it should be the energy and not the frequency that was treated as ultimate reality.[229]

(2) De Broglie described his theory of double solution, but was unable to explain how the causal propagation of particles, on which he insisted, might be achieved. Presumably on account of his radical suggestion that two solutions should coexist, no-one seems to have thought his theory worthy of much attention, but Pauli was able to criticise it very strongly on technical grounds and so save them the trouble of taking it seriously.[230]

(3) Heisenberg and Born had overcome their differences by the time of the Congress, and they presented a joint paper.[231] Basically, Born accepted Heisenberg's interpretation, while on the causality issue they compromised. Heisenberg had said previously that the concept of causality was irrelevant in the causal context, while Born had said that causality should be abandoned, and they seem to have accepted that their disagreement was purely philosophical. They talked at the Congress of causality being abandoned,[232] but the following year Born, whose view this had been, expressed the view that it was irrelevant,[233] on the same grounds as Heisenberg had earlier used. According to the interpretation they now shared, Born's concept of the wave as a probability was retained, but so was Heisenberg's view that particles (which continued to feature strongly in his interpretation) could only exist when observed. It was agreed that any world beyond that described by the theory was irrelevant since unobservable, and that physics had reached its limits in this theory, which was described as "complete and without contradictions".[234]

De Broglie was critical of this interpretation on the grounds that it was nothing but probabilities, with no physical realities in it at all,[235] while Schrödinger observed that these 'probabilities' were not probabilities at all but interfering vectors.[236] In 1928 Schrödinger complained further[237] that it was absurd to interpret measurements according to concepts that one held at the same time to be wrong, and the same year Einstein attacked the Heisenberg-Born "religion" as "a gentle pillow".[238] Einstein had mocked the theory at the Congress as having "the pretence of being a complete theory of individual processes"[239] and had criticised the interpretation of observation as involving "a very strange action at a distance".[240] At the end of 1926, he had also reiterated his belief in causality,[241] and the same opinion was expressed at the Congress by Lorentz.[242]

(4) Dirac agreed with the Born-Heisenberg theory so far as our knowledge of the system was concerned, but he refused to jump to any ontological conclusions. So far as he was concerned, the wave represented our knowledge of a system and the particle our more precise knowledge after measurement: any further elaboration was metaphysics, and not for him.

(5) Bohr reiterated his complementarity interpretation.

(6) Einstein's mockery of the pretensions of the Heisenberg-Born theory introduced the notion of a statistical or ensemble interpretation, according to which the wave could have only a statistical significance and the theory could describe only the behaviour of ensembles. Einstein thought that as the theory stood this was all that could be claimed of it, but realised that to leave things at that was to avoid the issue of interpretation. Of the available interpretations he seems to have favoured de Broglie's, as being at least in the right direction, but he found even that to be beset with insuperable problems.[243]

Einstein also raised the problem of the reduction of the wave packet, and apart from Heisenberg's considerations this was the first time that the problem of measurement had been seriously considered.[244] A measurement in quantum mechanics corresponded to a reduction of the mathematical description of the system from a range (wave) to a single value (particle), and this naturally disturbed Einstein, who saw it as a strange form of action at a distance. But it was no problem for de Broglie, whose particles were supposed always to exist, for Bohr, who treated the whole description as inadequate, for Dirac and Jordan, to whom it was metaphysics, or even to Heisenberg and Born, who argued that since it was contained in the theory it must be alright.

In fact, despite the many difficulties to the outside observer over the problem of interpretation, the Göttingen-Copenhagen school seemed quite happy with the situation in this respect. The various visualisations of Dirac, Born, Heisenberg and Bohr were soon all included under the 'umbrella' of the Copenhagen interpretation, and this was espoused, in one form or another, by Pauli, Jordan, Kramers and the vast majority of quantum physicists — even, for some twenty years, by de Broglie. Einstein had described the Heisenberg-Born theory as a gentle pillow, and they were quite prepared to sleep on it. The problem really facing the theory was in their view not the interpretation at all, but the extension of the theory to include relativity.[245]

IV.7. SUMMARY

By the end of 1927, the classical ideology of physics had been well and truly exorcised from the mainstream development of quantum theory. The process of exorcism had been remarkably swift, but nevertheless confused and complex; I shall briefly review the main points.

It had long been suggested, especially by Bohr, that there could be no structural description of quantum phenomena in space-time, in the classical sense. This view was dramatically reinforced in 1925 by the introduction of the commutation relationships. Bohr's earlier views, and those of Pauli and Heisenberg, had been based on physical considerations; but the commutation relationships were part of a successful mathematical theory of physics, and since the only other foundation of this theory was the recently confirmed principle of energy conservation, they were clearly a necessary part of it. The old quantum theory had relied upon quantisation of continuous classical formulae and had kept the two sides of the wave-particle duality distinct, but matrix mechanics effectively included this duality, as a whole, in a single axiom: duality thus became an essential part of physical theory. The impossibility of a classical type of structural description was also emphasised by two other developments in the winter of 1925-6. The realisation that the imaginary constant 'i' was also essential to the theory precluded any attempt at a visualisation in the usual space-time framework, and the split that developed between the mathematical and physical sides of the theory led to this theory being developed quite independent of any physical visualisation.

Although the introduction of operator theory took some of the sting out of the commutation relationships, it also marked a departure, its basis again in the formalism, from classical ideology. For operator theory includes, almost by definition, a strong element of subjectivity. Its very origins are in Boole's study of the laws of thought, and it implies a non-negligible interaction betwen operator and operand, subject and object, observer and observed. This move towards a subjective world view was reflected in the growing emphasis upon the observabilty criterion, apparent through 1926. Heisenberg's introduction of this criterion in 1925 may be seen in terms of phenomenalist methodology; Born raised it to an epistemological status and Dirac, in his work on quantum statistics, introduced it to the foundation, not only of the theory's aims and interpretation, but also of its formulation. The generalised Pauli principle was derived purely form the observability criterion.

The third element of classical ideology, namely causality, was also rejected in mid-1926, by Born, but this was probably for external reasons. It was clear, however, that causality was effectively abandoned in quantum mechanics, and in Born's association of the Schrödinger wave with probability as some kind of independent existent this was connected with the abandonment of a classical structural description. These departures from classical ideology were also apparent in the methodology of quantum theory in 1926: the separation of the mathematical theory from the physical interpretation was emphasised first in Dirac's formulation of matrix mechanics, then in Born's treatment of wave mechanics, and finally in the various formulations of transformation theory. In the hands of Dirac, Hilbert and von Neumann this was formulated in two distinct parts, one being a purely mathematical theory with no reference to any physical conceptualisation, and the other a set of rules by which this theory might be linked to physical observation (and we note it was again observation and not objective reality that was treated). Born had gone so far as to claim a deduction of the physical interpretation from a formalism that was without any physical grounding, and although Jordan reversed this methodology, and founded his theory upon physical axioms, these axioms had to be statistical; his approach was not adopted anyway.

The final developments with which we are concerned were those of Uncertainty, Complementarity, and interpretations in general. There were philosophical differences between most of the physicists concerned, and I have tried (though this is difficult) to indicate these, but for the most part they did not affect the main issues. Some physicists tried to preserve the classical ideology in all its aspects. Others were prepared to sacrifice the ideal of a consistent structural description in space-time so long as such a description could be maintained in phase-space, and so long as causality and objectivity were retained. De Broglie retained the latter ideals, but introduced the radical notion of structurally incompatible but coexistent solutions. He soon gave this up, however, and joined the vast majority of quantum physicists, who accepted an interpretation within the Copenhagen 'umbrella'. The 'Copenhagen interpretation' was based on elements of the Uncertainty principle and Complementarity, and its acceptance marked the end of classical ideology in mainstream quantum theory. Its main features, from this point of view, were as follows:

(1) A consistent structural description was abandoned, as was any hope of one in the future. This could be seen in terms of either the Uncertainty principle or the philosophy of Complementarity.

(2) Causality was abandoned as irrelevant, except so far as the propagation of probabilities was concerned. This was because, as Heisenberg first explained, we could not specify a physical situation to which the law of causality could be applied. The restriction was again a final, irrevocable, one.

(3) A phenomenalist epistemology was accepted, together with the belief that the limits of our observations (in terms of Uncertainty) had been reached: this limit was accepted as a result of an essential, unremoveable, observer-observed interaction, which precluded any precise objective knowledge.

CONCLUSION

The central aim of this work has been to trace a change in physical ideology, a change that manifested itself most clearly in the quantum theory of physics. The ideology of 1900 was objective, in that a real world independent of the observer was assumed to exist, and to be the subject matter of physics; it was causal, in that strict causality was supposed to govern physical behaviour; and it was structurally consistent, in that a consistent structural description of nature in space and time was assumed to be possible. By about 1930, however, after the establishment of quantum mechanics, the ideology had become subjective: a phenomenalist epistemology was widely accepted, and an irreduceable non-negligible interaction between observer and observed was held to limit, in principle and for all time, the possibility of objective description. Causality had been abandoned, and so had all hope of a consistent structural description in space and time.

In pursuit of this aim, several problems arose. It was clear from the start that I could not hope to cover both quantum and relativity theories in a single study, and I had to restrict myself to quantum theory. It seems probable that the development of relativity theory was of only minor importance for the change in ideology, for whereas quantum theory was inextricably linked with this change relativity theory could, and did, support a variety of ideologies. It did however provide the context for much conceptual and ideological discussion, and my analysis cannot, in a sense, be thought of as complete until it has been extended to cover this discussion.

It was also clear from the start that a comparative analysis of the developments in physics with those in other branches of science, and especially in philosophy, literature and the arts, could not be brought within the scope of the thesis. This naturally limited the achievement of my aim, especially when combined with the lack of thorough studies of the individuals most concerned with the change in ideology: in many cases I was able to establish how and when an idea was formulated, but not why. In such cases my procedure was to ennumerate the possible influences, together with the arguments for and against their having acted, and to refrain from judging any one influence <u>the</u> cause of a development. Often the historian can do no more. One or more influences may have provided the background for a new idea, another may have sparked it off, and still others may have been instrumental in its formulation. Looking back on the event, all these influences may seem to offer potential 'explanations' of the idea, but the apparent sufficiency of any one in this respect cannot be held to exclude consideration of the others. Sometimes, however, an understanding and knowledge of the individual concerned may lead to one or more influences being attributed a particularly high, or low, degree of importance, and it is in this respect that the limitations of the present work may be felt.

A serious problem of another kind concerns the definition of the ideology. Different physicists may use the same word to convey different senses, or different words to convey the same sense, and this may even be true of the same physicist at different times. Again, different physicists may have different ideas as to what constitutes, and especially as to what is essential to, the prevailing ideology; the same statement may be in one context a threat to the ideology and in another a defense of it. Reason, finally, does not always square with emotion; the fact that a simple logical deduction may have been drawn does not necessarily mean that it was drawn, especially in respect of an emotive issue such as causality.

This problem may be partially solved through a deep understanding of the individuals concerned, but such an understanding was again beyond the scope of the thesis. Moreover, any such understanding is necessarily subjective.

In fact, any discussion of an ideology must to some extent remain subjective, but I hope that in the present work awareness of this limitation has lessened its importance. Thus I have not imposed any technical philosophical framework upon my discussion, and although it may be objected that I have, as a result, left many philosophical terms only imprecisely defined, I hope that I have avoided the greater danger of imposing my own conceptions upon physicists discussed. Finally in this context I should note that the three features used to characterise the ideologies (the selection of which is perhaps the most obviously subjective element of my discussion) are not intended to be mutually exclusive, or to constitute a precise definition: they are simply the features that seemed to me most strongly to characterise the contrasting ideologies.

The problems I have discussed so far are all, in a sense, unsolved: they all represent limitations upon the achievement of my main aim. There was however one problem that had to be solved for this aim to be pursued at all, and the solution of which constitutes the major part of this work. In order to discuss the change in ideology, I needed a history of quantum theory on which such a discussion could be based, and I had to write this almost from scratch. Much has been written on the history of quantum theory, but it has almost all been concerned with the technical details of the theory, and with its successful predictions. My concern was with physical conceptualisation, methodology and ideology, with the failures and paradoxes of the theory rather than its successes, and with the attitudes and ideas to which these led. My whole perspective was necessarily different from that usually adopted: the emphasis upon the various features was different, and there were important features that had hardly been mentioned, let alone analysed, by previous historians. On top of this I found that time and time again the existing accounts were inadequate, even on their own terms; they either added nothing to what could be learnt from simply reading the published work that they discussed, or imposed upon this work interpretations that were quite unconvincing in the light of deeper investigation. In the remainder of this conclusion I shall summarise extremely briefly the main fields in which I have departed from existing accounts, and the main results relating to my main theme, the change in ideology.

A new perspective on quantum history

In the new perspective developed in this work the history of quantum theory from 1900 to 1920 is very much the history of the wave-particle duality, and a large part of Chapter I is devoted to the introduction and partial acceptance of this concept in the period 1909-1914. So far as I know mine is the first thorough analysis of these developments, and of the impasse that characterised the wave-particle problem between 1915 and 1920. In comparison with these developments, those of atomic theory and the old quantum theory are relegated in importance in the new perspective. I do find the old quantum theory to be of crucial importance, but not for its technical development and success; its importance seems to lie rather in some significant methodological changes that it involved, and that have not previously been discussed, and in its role as a training ground for young physicists. My other major contribution in respect of the early period is a study of Planck's derivation, in 1900, of the black-body radiation law. Despite a mass of scholarship on the subject many puzzling questions remained unanswered, but by linking Planck's 1900 work with his work of previous years it was possible to reinterpret the former such that all the questions were simply and convincingly answered.

After the impasse of the previous years the period 1921-3 saw a quantum conceptual crisis, and this crisis, only a few aspects of which have been previously treated, is the subject of my second chapter. For a variety of reasons, linked with relativity theory, X-ray experiments and the development of Bohr's ideas, there was a sudden revival of interest in the wave-particle problem, and by 1923 most quantum physicists recognised the need for a totally new theory. The positive steps towards such a theory included the study of dual theories, discussions on energy conservation, causality and space-time descriptions, and some new ideas, arising from problems in the old quantum theory, on the structure of the atom. The most important of the dual theories was probably de Broglie's, and I include a discussion of his work that goes well beyond the existing treatments. Regarding the causality issue there already existed an important discussion by Forman, who found that there was a largescale departure, in Germany, from the causality ideal, and that this was largely in accomodation to the Weimar intellectual millieu. I however found that, despite strong external pressures and even stronger internal ones, there were only a few isolated rejections of causality. Moreover only Bohr's tendency in this direction (clearly unrelated to Forman's thesis) seemed to be of any importance for the development of quantum theory.

The next major conceptual development was the Bohr-Kramers-Slater viewpoint of 1924, and here I have been able to go well beyond existing accounts, especially in respect of Slater's contribution. The same is true in respect of the development of Heisenberg's kinematics, the ideas behind which were almost entirely due, it would appear, to Pauli. I was also able to make significant contributions in respect of the reception of de Broglie's ideas, the development of Schrödinger's wave mechanics, and the relationship between the wave mechanics and matrix mechanics.

In treating the further developments of 1926 and those of 1927 I was effectively in virgin territory, so that my treatment was almost entirely original. My main discussions concerned the developments, which I found to be intimately related, of quantum statistics, transformation theory, and the Uncertainty principle, and the development of the various interpretations of quantum mechanics, most of which were established by the Solvay Congress of 1927.

The introduction of quantum ideology

In the period to 1920, there seems to have been no significant challenge to the classical ideology. The establishment of the wave-particle duality paved the way for the conceptual crisis that was to come, and the positivist approach adopted in the old quantum theory, though still linked with a structural model of the atom, prepared the ground for the move to a phenomenalist methodology; but the ideology remained as yet untouched. The first challenge to the ideology seems to have come in the conceptual crisis of 1921-23. The wide acceptance that strict energy conservation might have to be abandoned was not in itself a departure from the ideology, but it was seen as such by its oponents. Bohr in this period abandoned a classical description in space-time, and came very close to abandoning causality. There was considerable discussion of the causality issue. In reaction to the problems facing the old quantum theory, Heisenberg, and especially Pauli, adopted a phenomenalist methodology.

Still, however, the classical ideology reigned. The phenomenalism does not seem to have been extended at this stage to epistemology, Bohr's rejection of a classical space-time description was not generally taken up, and rejection of strict causality was limited to a few isolated individuals. Bohr's ideas on causality, energy conservation and space-time description came to fruition in the Bohr-Kramers-Slater theory of 1924, which also postulated an intermediate type of existence for the "virtual" oscillators supposed to make up the atom. But although the new technique was successfully applied to a variety of situations, the physical content of the theory was almost universally rejected. With the results of Geiger and Boethe in 1925, this challenge to the classical ideology crumbled.

These results, however, coincided with a mass of evidence indicating a wave-particle duality for matter as well as light, bringing the ideal of a consistent structural description under much greater pressure than before. Meanwhile, in 1924, Pauli had concluded that an explicitly phenomenalist methodology should be adopted, that the orbital model of the atom should be abandoned, and that the atom should be seen as a set of oscillators and described in terms of observables. In 1925 Heisenberg, working from the oscillator model of the atom, developed a new kinematics in which he incorporated all of Pauli's conclusions. Heisenberg's theory was the first really important challenge to the ideal of objectivity, and the first really important departure from the ideal of a consistent structural description in space and time. It also established the quantum methodology of formalism first, interpretation later.

Schrödinger claimed that his wave mechanics of 1926 was within the classical ideology, but the fact that his waves were in multi-dimensional phase-space and his failure to eliminate the wave-particle paradox both argued against this. His theory was also, like that of Born and Wiener, an operator theory, and the very use of operators would seem to imply a non-negligible interaction between operator and operand, or observer and observed. In the development of matrix mechanics in 1925-26 the impossibility of a structural description was emphasised by a series of factors. First, the commutation relationships effectively incorporated the wave-particle duality in a single axiom acting as a logical foundation for the theory: any resolution of the duality within the theory was thus precluded. The realisation by Born, Wiener and Dirac that the imaginary constant 'i' entered essentially into the theory precluded any attempt at a structural visualisation within the usual space-time framework, and the split that developed between the mathematical and physical sides of the theory led to its being developed independent of any physical visualisation.

In his development of Schrödinger's theory in the summer of 1926, Born raised Heisenberg's phenomenalism to an epistemological status, insisting that physics could describe only phenomena, and introducing the idea that the current theory of physics (of which he was largely the author) was final. Of the physicists concerned with quantum mechanics, the most conscious of the role of the observability criterion was Dirac, and in his 1926 work on quantum statistics he derived the generalised exclusion principle, a necessary foundation of quantum theory, from the observability criterion; the new phenomenalism thus became part of the very fabric of the theory.

Another feature of Born's 1926 work was his outright and explicit rejection of causality. He called this a philosophical decision and it may well have been externally motivated, but it had become clear that causality was <u>effectively</u> abandoned in the new theory, and in Born's identification of the Schrödinger wave with a probability (as some kind of independent existent) this was associated with the abandonment of a structural description.

The final development of the new ideology was contained in the Uncertainty and Complementarity principles, and in the general acceptance of an interpretation of quantum theory based upon these. Some physicists did try to preserve the classical ideology in all its aspects, while others tried to retain causality and objectivity, sacrificing only the structural description in space-time (replaced by one in phase-space). De Broglie introduced the notion of structurally incompatible but coexistent solutions. He soon gave this up, however, and joined the majority of physicists who accepted solutions under the Copenhagen 'umbrella'. There were considerable differences of opinion within this 'umbrella", not least between Heisenberg and Bohr, on whose ideas it was based, but the main features were established by the Solvay Congress of 1927; they may be expressed in terms of an abandonment of a consistent structural description, an abandonment of the causality concept as irrelevant in its strict classical form, and an acceptance of a phenomenalist epistemology.

Between 1928 and 1933 the dominant position of the Copenhagen interpretation was confirmed and consolidated through the publication of a long series of textbooks and review papers, written by its adherents. Among the authors were Heisenberg, Dirac, Weyl, Born and Jordan, Kemble, Pauli, and von Neumann. Von Neumann's book (Mathematische Grundlagen der Quantenmechanik, Springer 1932) was particularly important, for as well as the definitive account of the formulation of quantum mechanics it gave a solid mathematical foundation to the Copenhagen interpretation. As a part of his analysis von Neumann gave a demonstration, accepted for a long time as rigorous, that an interpretation incorporating hidden variables, according to which the classical ideology might have been maintained, was impossible. He also analysed the measurement problem, or the collapse of the wave-packet, deducing that this collapse could not be avoided. His analysis seemed to imply that it could only take place in the consciousness of the observer and this conclusion, stated explicitly by London and Bauer, cemented the phenomenalist element of the Copenhagen interpretation.

APPENDIX A

THE ORIGINS OF PLANCK'S RADIATION LAW

Although there have been many attempts at an historical analysis of Planck's radiation law, several questions still remain unanswered. Why did Planck adopt Boltzmann's statistical treatment of thermodynamics, after having rejected it for so long? Why did he modify thid treatment as he did? How did he derive his combinatorial probability formula (equivalent to the Einstein-Bose statistics) ? Is there any truth in the story that he told his son "Today I have made a discovery which is as important as Newtons"? If so, what did he mean? Finally why, in his earlier derivation of Wien's radiation law, did he ignore the equipartition theorem?

To answer these questions is not difficult, but it involves a penetration of the barrier that seems to lie between the historical accounts of Planck's 1900 theory and those of his earlier work, and a complete reassessment of what he actually intended in his 1900 work.

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It is usual to attribute the 'discovery of the quantum' to Planck in 1900. From the conceptual viewpoint, however, Planck's work should be seen as part of the prehistory of quantum theory, for although he introduced the quantum constant 'h' he did not associate it with any change from the traditional concepts. He saw 'h' neither in terms of the structure of radiation (energy quanta), nor even in terms of the nature of radiative processes (action quanta), but as part of an hypothesis of natural radiation, related to the problem of thermodynamic irreversibility and to Boltzmann's hypothesis of molecular disorder.

The general historical background to Planck's radiation law is well known.[1] By 1900, he had for some years been developing a derivation of Wien's black-body radiation law.[2] The classical theory of black-body radiation had progressed smoothly as far as <u>Wien's displacement law</u> of 1894 for the radiation density per unit frequency,

[ν = frequency, T = temperature] $$\rho_\nu = \nu^3 F(\nu/T) \quad ., \quad \text{.....} [1]$$

but the theoretical derivation of an exact form for the function F had caused problems. By a spurious argument, Wien had derived the radiation formula

[α, β constants] $$\rho_\nu = \alpha \nu^3 \exp\{-\beta\nu/T\} \quad . \quad \text{.....} [2]$$

This formula, which we shall refer to simply as Wien's law, seemed to be in reasonable agreement with experiment. Planck accepted it on empirical grounds and set out to improve upon the derivation, finally achieving a solution in two parts. First, he related the black-body radiation density, at frequency ν and temperature T, to the average energy U of a harmonic oscillator of the same frequency in an isothermal cavity of the same temperature. From the classical Maxwell-Hertz equations, he deduced that

$$\rho_\nu \propto \nu^2 U(\nu, T) \quad . \quad \text{.....} [3]$$

Secondly, he 'solved' the problem of the oscillator by the simple expedient of defining the entropy S of the system quite arbitrarily as

[a, b constants] $$S = \frac{U}{a\nu} \cdot \log\{U/eb\nu\} \quad . \quad \text{.....} [4]$$

This gave the required solution immediately, on application of the standard classical definition of entropy, $$\frac{dS}{dU} = \frac{1}{T} \quad . \quad \text{.....} [5]$$

This derivation of Wien's law provides several clues to the working of Planck's mind, and so helps us to understand his later derivations of his own law. The first thing we notice is that it is hardly a 'proof', resting as it does upon an arbitrary definition. From a knowledge of Planck's character, we can, however, be confident that there was no element of deceit, so we may reasonably ask what he felt his 'derivation' had achieved.

The answer is that he had reduced the problem to one of the definition of entropy. The importance he attached to this concept stemmed from his general outlook on life, as indeed did his whole approach to science. As is shown by his own writings,[3] and by those of his friends and colleagues,[4] he had an extraordinarily systematic mind which was fired by an enormous conviction and faith — in Nature, in the "absolute", in God. Late in life he wrote of the "crusade of natural science" with its rallying cry of "On to God",[5] and a belief in the 'divine' nature of scientific laws seems always to have been his main source of inspiration. In his scientific autobiography he recalled that from an early age he had considered it "of paramount importance that the outside world is something independent from man, something absolute", and that "the quest for the laws which apply to this absolute appeared to me the most sublime scientific pursuit in life."[6] His study of science was the study of this "absolute": he had complete faith in observations of the "absolute", i.e. in experimental results, and he sought to base science upon laws of a suitably "absolute" nature. The most absolute law he could find was the second law of thermodynamics, and its subject, entropy, became his Lieblingsthema:

"Since maximum entropy identifies final equilibrium, all laws of physical and chemical equilibrium can be derived from a knowledge of entropy." [7]

We may now see how faith in experiment prompted him to seek a derivation of Wien's law, and how the mere reduction of the problem to one of the determination of entropy was in his eyes a worthwhile achievement. Moreover, having accounted for what he did do, we may also learn something from what he did not do. Having derived equation [3], he could have introduced the well known equipartition law,

[κ constant] $$U \propto \kappa T \qquad , \quad \ldots\ldots [6]$$

which would immediately have given the radiation formula

$$\rho_\nu \propto \nu^2 \kappa T \qquad , \quad \ldots\ldots [7]$$

in contradiction with Wien's law.

Assuming equation [3] to be valid, Planck should have seen that either Wien's law or the equipartition law had to be wrong; but he passed this by without comment. Why?

First, we must consider the possibility that Planck had never come across the equipartition law.[8] This may be dismissed, however, for the law was not only well known but also frequently disputed. Moreover, Planck was definitely familiar, as we shall see, with the Maxwell-Boltzmann kinetic theory, from which the law was derived;[9] he seems to have had some difficulty with the mathematics of this theory,[10] but he must have been aware of such an important physical result.

Assuming that Planck did know of the equipartition law, he could hardly have rejected it as inapplicable in the particular situation, for if applicable to anything it would be so to a set of simple oscillators. His procedure therefore appears to be tantamount to a rejection of the equipartition law itself; there was indeed some dispute about its validity. But if he rejected the law, why then did he not comment on the fact that it led to a clearly impossible answer, equation [7] giving infinite radiation density as ν increases (later termed the ultraviolet catastrophe)? Planck was not usually prone to hiding his opinions.

We cannot give a definitive explanation of this situation, but we can elucidate it by considering Planck's attitude to the kinetic theory in general.[11] As early as 1882, we find Planck comparing the kinetic theory of physics with an approach based upon absolute entropy and the second law of thermodynamics. He concluded then that "The second law of thermodynamics, logically developed, is incompatible with the assumption of finite atoms",[12] and he expressed the opinion that a continuous concept of matter would prevail. Bearing in mind that his beloved Second Law could only have a statistical significance within the kinetic theory, this is indeed what we should expect. In accordance with his ideals and expectations, he continued tu use the Second Law as a basis for his physics, in preference to the kinetic theory, but he by no means ignored the latter. In 1889-91, he co-edited Clausius' Die kinetische Theorie der Gase,[13] and in 1891 he refered to the theory in a lecture stressing the success of the Second Law in the realm of physical chemistry:

> "Everyone who studies the work of the two investigators who probably have penetrated most deeply into the analysis of molecular motions, namely Maxwell and Boltzmann, will be unable to avoid the impression that the admirable expenditure of physical ingenuity and mathematical dexterity necessary to master the problems of the day are out of proportion to the results achieved." [14]

The same year, at a meeting of German scientists in Halle, he attacked both the kinetic theory and Boltzmann, and defended his own approach, which he continued to pursue..

So far, Planck's attitude had remained constant, but towards the end of the century a change began to take place. In 1896, Zermelo objected to the kinetic theory on the grounds that, as a mechanical theory, it could describe only reversible processes, so that any state must necessarily recur, in contradiction with an absolute statement of the Second Law. Boltzmann replied that the Second Law was statistical, not absolute, but he admitted that the kinetic theory required a hypothesis of molecular disorder for all states, if the Second Law was to be satisfied.[15] In 1897, Planck began a series of papers[16] on irreversible radiation processes that took up the debate with Boltzmann. His derivation of Wien's law, in 1899, was contained in the last of these papers.

By 1897, Planck had already retreated somewhat from his earlier position. He accepted that his own approach was frankly empirical and a temporary expedient, and he was prepared to concede that the kinetic theory "penetrates most deeply into the nature of the processes considered, and, were it possible to carry it out exactly, would be characterised as the most perfect" approach.[17] But as long as it remained a statistical theory, it could not be "exact", and he felt that "obstacles, at present unsurmountable, ... , seem to stand in the way of its further progress", the obstacles being "not only in the highly complex mathematical treatment of the assumed hypotheses, but above all principally in the ... difficulties in the mechanical interpretation of the fundamental thermodynamic principles."[18] He thus felt that his own approach was the most fruitful and satisfactory available. He expected it to be superceded by a new and more fundamental theory, but thought that this would be based on an electromagnetic, and not a mechanical, interpretation; this distinction formed the basis of his argument with Boltzmann.

In his first paper on irreversibility, Planck raised Zermelo's objection, and claimed that it would not necessarily apply to an electromagnetic theory, such as he tried to develop. Boltzmann replied that an electromagnetic theory would need just the same sort of assumptions as a mechanical theory, suggested that Planck's attempted theory was in fact reversible, and dismissed the recurrence objection as applying only to a finite number of molecules, whereas the Second Law could only apply to an infinite number (for which the recurrence period would be infinite). In the ensuing papers, Planck struggled to get round Boltzmann's first two points, but without success. In July 1898[19] he was forced to introduce the concept of "natural radiation", the processes of which were irreversible, and the following year he realised that this concept was characterised by a randomness or indeterminacy[20] analogous to Boltzmann's concept of molecular disorder.[21] Moreover, although he still prefered his own to Boltzmann's interpretation, he realised that, in their present states, the two theories were more or less equivalent, in that they rested on equally arbitrary hypotheses. This was the context of his derivation of Wien's law: he may well have noted that the kinetic theory, through the equipartition law, gave an impossible radiation formula, but he no longer wished to criticise this theory. In a complex situation with which he had not yet come to terms, he had every reason to remain silent.

In 1900, Rayleigh[22] wrote a short paper emphasizing that equation [7] was a necessary consequence of the equipartition law. In October of that year, Rubens and Kurlbaum[23] found that for very low frequencies it actually agreed with experiment, and communicated this result privately to Planck. Planck was then faced with the situation that, although Wien's law had by now received strong experimental verification for high frequencies, the 'Rayleigh-Jeans' law,

$$\rho_\nu = \frac{8\pi}{c^3}\nu^2 kT \qquad , \ldots\ldots [7a]$$

which we shall refer to simply as Rayleigh's law,[24] seemed to be valid for low frequencies. As we have noted, Planck was always deferential to experimental results, and he immediately set to work to find a new law covering both situations. He presented his results[25] to the German Physical Society at the same meeting as did Rubens and Kurlbaum theirs.

Returning, characteristically, to entropy, Planck reduced the two limiting radiation laws to their respective equations for its second derivative, and compromised. His own derivation of Wien's law gave $\frac{d^2S}{dU^2} \propto \frac{1}{U}$, and the equipartition law gave $\frac{d^2S}{dU^2} \propto \frac{1}{U^2}$. He settled for the compromise $\frac{d^2S}{dU^2} \propto \frac{1}{U(U+A)}$, with A constant, combined it with Wien's displacement law and with his own formula [3], and arrived at a formula for the radiation density with precisely the desired properties, namely

$$[\beta \text{ constant}] \qquad \rho_\nu \propto \nu^3/(\exp\{\beta\nu/T\}-1) \qquad . \ldots\ldots [8]$$

Planck's choice of compromise was not the only one possible: the most general choice would have been $\frac{d^2S}{dU^2} = \frac{BU+C}{U(U+A)}$, which, for different values of the constants, gives not only the Rayleigh, Wien and Planck laws but also that associated with Fermi-Dirac statistics. Planck's choice was purely empirical, and he recognised this, but the result seemed satisfactory (and was of course much verified subsequently). He therefore strove furiously to produce a theoretical derivation, and within a few weeks he had succeeded. His approach to the problem was to adopt a combinatorial technique similar to, and apparently inspired by, Boltzmann's statistical methods. In his fampus paper,[26] he stated quite happily that the constant energy of a stationary (equilibrium) state resonator could only be an average, over time or over a large number of similar resonators. His approach to physics was apparently to be "absolute" no more. We shall consider below the nature of this apparent conversion, but we shall first outline the derivation.

As always, Planck started with entropy, and, since this depended upon the distribution of energy over a set of resonators, he considered the possible distributions, following Boltzmann in treating the problem as one of probabilities. Taking N resonators of frequency ν , he considered the probability W that they have average energy U, the total energy NU being divided for convenience into P elements of size ε , so as to make the number of possibilities finite. N and P were assumed very large, ε very small. He then made the assumption that

$$W = \frac{(N+P-1)!}{(N-1)!\,P!} \qquad \ldots\ldots [9]$$

He explained, quite correctly, that this entailed the assumption of equal probabilities for equal complexions, a complexion being defined by the assignment of P_1 energy elements to resonator number 1, P_2 to number 2, etc.; but a really clear visualisation of the assumed formula was quite lacking. The fact that it did not matter which energy element was where, so long as the totals agreed, was not even mentioned, let alone compared with alternatives or given significance. This complete lack of discussion is very important, for Planck's assumption was in fact completely new to physics and in contradiction with the assumptions usually made,[27] yet he supplied no physical explanation for it.

His next assumption was that the entropy was given by $S = k \log W$, this being Boltzmann's formula without the usual additive constant, which Planck sacrificed in the interest of absoluteness. The omission of the constant did not, however, affect the argument. He then defined the equilibrium state in the usual way by the formula $\frac{\partial S}{\partial (NU)} = \frac{1}{T}$, which led, via Stirling's approximation for W, to

$$U \propto \varepsilon \Big/ \left(\exp\left\{\frac{\varepsilon}{kT}\right\} - 1 \right) \qquad \ldots\ldots [10]$$

Planck next used his own equation [3] to convert to a formula for ρ_ν, invoking Rayleigh's law in the limit of low frequencies to give the overall constant factor. He deduced from Wien's displacement law, [1], that $\varepsilon \propto \nu$; the constant of proportionality could not be zero on empirical grounds (this gives Rayleigh's law), so he called it 'h', giving the well known radiation formula, <u>Planck's law</u>,

$$\rho_\nu = \frac{8\pi\nu^3 h}{c^3} \Big/ \left(\exp\left\{\frac{h\nu}{kT}\right\} - 1 \right) \qquad \ldots\ldots [11]$$

Planck adopted Boltzmann's method, but he modified it too. He did not in fact mention explicitly that ε could not be zero, nor that according to Boltzmann's method it should be zero. The formula for W was also inconsistent with Boltzmann's assumptions, though again this was not mentioned, and there were other inconsistencies in the argument. So why did Planck adopt Boltzmann's method, why did he modify it as he did, and from where did he get his formula for W? To answer these questions, we must return to the clues gleaned from our study of his derivation of Wien's law; but first we shall examine the myth that Planck somehow 'invented' the quantum consciously.

This quantum myth stems from the oft-repeated anecdote that while preparing the above derivation Planck told one of his sons: "Today I have made a discovery which is as important as Newton's."[28] Arguments have been advanced both for and against the truth of this anecdote, but little attention has been paid to the more important problem of what, if it is true, Planck meant. What was his discovery? Reading Planck's paper, there can be little doubt that he genuinely introduced ε as a mathematical convenience, much as Boltzmann had, reducing the uncountably infinite number of possibilities associated with a continuous energy spectrum to something that could be handled mathematically. The introduction of 'h' rather than letting ε tend to zero appears to have been forced on him by the need to reproduce the empirical results. Certainly, he maintained afterwards that his introduction of 'h' was purely formal, and he recalled that he "tried immediately to weld the elementary quantum of action 'h' into the framework of the classical theory",[29] when he realised that its introduction was necessary. What then did he discover?

Reading his later writings, we get the impression that Planck positively disliked quanta — but this is not the impression given by the 1900 paper. Later, nothing would have pleased him more than a continuous classical form for energy, but, although the quantum $\varepsilon = h\nu$ was introduced in the paper only as a mathematical convenience, he accepted without comment the finiteness forced upon it by experiment and was even quite enthousiastic about the constant h. The reason for this, we suggest, is that when Planck wrote this paper he was not concerned with the structure of radiation at all. He was not writing about quanta of energy, or even of action, but, as he stated explicitly in the paper,[30] about the hypothesis of natural radiation that he had introduced as a result of the debate with Boltzmann. Planck's natural radiation hypothesis may be defined as that hypothesis necessary to derive the empirical laws of radiation, and the main aim of his paper was to determine this hypothesis. Thus, what appears to be an assumption — that all distributions of energy among resonators (defined by the energy levels of the resonators) are equally probable, the distributions being 'defined' by the constant h — was in fact a conclusion. Only later did he consider the implications for the structure of radiation in terms of classical physics, and he then examined the necessity of h and tried to weld it into the framework of the classical theory. At this stage, h was an absolute natural constant defining natural radiation, and as such it was truly a great discovery, even on a par with Newton's. To put it another way, Boltzmann's statistical mechanics was wrong after all, since it gave Rayleigh's law. Planck's statistical mechanics, based on this new definition of natural radiation, would be right!

This is my interpretation of Planck's discovery. The main alternative is that his discovery was the realisation that his radiation formula allowed an empirical calculation of h, and, more importantly, of k, thus providing values for a host of other constants dependent upon k. He was indeed very proud of this achievement, giving it great prominence in his paper, and later resenting the fact that k should have been named after Boltzmann rather than himself. Whichever interpretation may be 'true', Planck had ample grounds for self-congratulation, other than those with which he is traditionally credited.

Having disposed of the myth, we may now return to our analysis of Planck's derivation. He claimed later that he adopted the particular method he did because " no other path appeared open to me"[31] and " a theoretical interpretation had to be found at any cost, no matter how high."[32] This is hardly a sufficient explanation, but it is an important element in one. By 1900, Planck had already spent three years struggling with Wien's law, and must have pretty well exhausted the possible approaches within his original non-statistical framework.

The added complications of his own law may well have been the last straw. His formula was empirically proven, so it must have a theoretical basis somewhere, and whereas the end was, so to speak, God-given, the means, due to mere humans, were adaptable. So he was forced to adapt.

The rapidity of Planck's abandonment of Wien's law can be sufficiently explained by his respect for experimental results, but from our earlier study we can see that he may actually have welcomed these results. The fact that he had been forced into a purely empirical derivation of Wien's law cannot have left him happy, and the subject had been inextricably involved with the arguments with Boltzmann, which must have left him quite confused. By the time he plunged into a theoretical derivation of his own law, he had had a little time to consolidate these arguments, and he now had a completely fresh problem in which to put into practice any new ideas that he had evolved. His first problem must have been not to determine a probability formula, but to determine an entropy formula. Not only would this have been consistent with his general approach, and with his evaluation of the equilibrium state in terms of entropy (as opposed to Boltzmann's use of probability), but it would also have tied in with his statement in the paper that

> "The entropy of the system in a given state is proportional to the logarithm of the probability of that state..... [this is] just a definition of the probability of the state."[33]

As he pointed out, this was not the case with the kinetic theory, where it was the probability that defined the entropy.

Planck therefore did not adopt the philosophy of the kinetic theory. His starting point was not the probability assumption, but the entropy definition, which he knew in this case had to lead to the formula for the second differential, $\frac{\partial^2 S}{\partial u^2} = \frac{B}{u(u+A)}$. Thus the entropy itself had to be of the form $S \sim B(u+A)\log(u+A) - Bu\log u$. This form had to be derived, and the only alternative expression from which to take such a derivation was one in terms of probabilities. But Planck could now take this path with a clear conscience. He had emphasized the essential difference between his approach and that of the kinetic theory, and he had learnt from the debate with Boltzmann that, apart from this distinction, there was nothing wrong with the technique of the kinetic theory that was not also wrong with that of his own theory. Having realised that a hypothesis of natural radiation was essential to his own theory, he felt free to express this hypothesis in terms of probabilities, at least as a preliminary step.

Putting $S = k\log W$, he then needed a formula for W. Our last question must be why he chose the one that he did, and here we may recall from Planck's earlier discussions of the kinetic theory that he seemed to find it mathematically difficult. The interpretation of physical situations in terms of mathematical probabilities was not apparently his strong point. He showed no sign of having derived W from physical considerations, and it seems unlikely that he did so. On the other hand it is not an easy formula to work back to. There is, however, one explanation that seems to fit perfectly: although the formula was strange and unknown to physicists at that time, it was perfectly familiar to mathematicians, as 'how to put P balls into N urns, the balls being identical'. We may envisage that once again Planck adopted an empirical approach, going either to a mathematician or to a standard text, and simply trying out the probability formulae that he found. He may have selected the one he did on the naive basis that the energy elements were of course identical (though physically this is a strange and unsatisfying assumption, as we now know), or he may have tried several, choosing the one that worked. He in fact wrote of the formula that

> "In the last resort, its proof can only be given empirically. It can also be understood as a more detailed definition of the hypothesis of natural radiation which I have introduced." [34]

It was Planck's answer to Boltzmann's molecular disorder, and it worked, whereas the latter did not.

— * —

Planck's work had nothing to do with either the nature of radiative processes or the structure of radiation, and the second point is quite apparent when we examine his derivation of the radiation law in terms of structural hypotheses. Splitting the derivation into two parts, we may tabulate the assumptions:

In deriving [10], $U \propto \epsilon \Big/ \left(\exp\left\{\frac{\epsilon}{kT}\right\} - 1\right)$:

$S = k \log W$	, derived by Boltzmann on the assumption that energy is composed of <u>independent particles</u>, with their energy <u>tending to zero</u>.
$W = \dfrac{(N+P-1)!}{(N-1)!\,P!}$	, a combinatorial formula which assumes, as we shall see, energy transfer in <u>non-independent particles</u>, whose energy is later assumed to be <u>non-zero</u>.

In deriving [11], $\rho_\nu = \dfrac{8\pi h \nu^3}{c^3} \Big/ \left(\exp\left\{\frac{h\nu}{kT}\right\} - 1\right)$:

$\rho_\nu \propto \nu^2 U$	, derived from the Maxwell-Hertz <u>wave</u> equations.
constant $8\pi/c^3$	, derived from the classical <u>wave</u> theory in the limit $\nu \to 0$.
$\rho_\nu = \nu^3 F(\nu/T)$	, Wien's displacement law, which can in fact be derived independent of the structure of light.
$\epsilon = h\nu$, $h \neq 0$	, <u>Non-zero particles</u> of energy, forced empirically.

Both the fact that h is non-zero and the choice of the constant factor are essentially empirical, and the former completely contradicts the use of the wave theory. The form for W is also empirical, and renders the law $S = k \log W$ completely a priori, in view of the assumptions needed to derive this law.

Thus, as soon as Planck's derivation is interpreted in terms of structure, it appears as a mass of contradictions. His law in fact originated as a compromise between that of Wien, which we now know (though he did not) results from the assumption of light particles, and that of Rayleigh, which is a consequence of the classical wave theory of light. The wave-particle duality of the structure of light (often used to characterise quantum theory) was therefore implicit in it. But Planck, clearly was not only unaware of this duality (recognised by Einstein in about 1909) but also blissfully ignorant of the fact that the structure of light in terms of waves and particles was an issue at all.

APPENDIX B

J.J.THOMSON AND THE STRUCTURE OF THE AETHER[1]

When X-rays were discovered in 1895 by Röntgen,[2] their classification within the generally accepted dichotomy of matter as particles and light as a wave effect in the aether proved to be difficult. Basically, as Stokes said, "Everything tends to show that these Röntgen rays are something which, like rays of light, are propagated in the ether."[3] But in many ways they differed considerably from light. They did not show significant reflection, refraction, or diffraction, but did show a clear ionizing effect, suggesting a corpuscular nature. In 1900, γ-rays were discovered,[4] and these shared the basic X-ray similarity to light, whilst at the same time being exceptionally penetrating and very strongly ionizing; they tended to appear, moreover, in the company of α- and β-rays, whose corpuscular nature was quickly established.

Röntgen himself suggested that X-rays might be associated with longitudinal vibrations in the aether,[5] in contrast with the transverse vibrations of light, but as well as facing mathematical difficulties this clashed with the results of Galitzin and Karnojitzky,[6] which appeared to show polarisation of the rays. In 1896, the suggestion was therefore dismissed by Stokes[7] and independently by Wiechart[8] in favour of an aether-pulse hypothesis. Stokes suggested that X-rays, emitted from a Crookes tube, resulted from the collision of cathode rays, which he believed to be corpuscular, with the end of the tube:

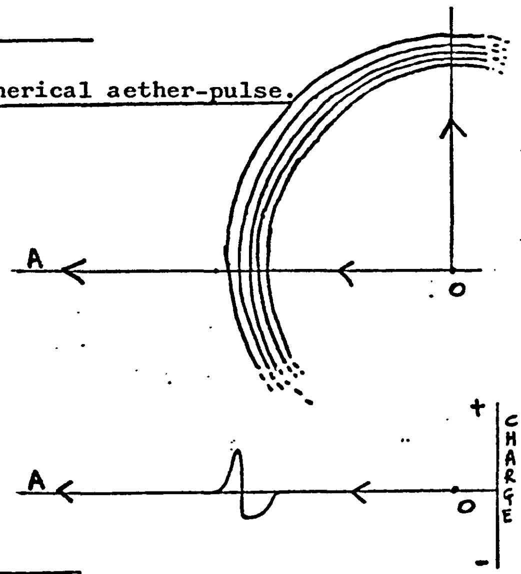
> "If these charged molecules strike the target we may think it exceedingly probable that by virtue of their charge they produce some sort of disturbance in the ether." [9]

This disturbance would have the nature of a "pulse", the simplest form of which "would be one consisting of two halves in which the disturbances were in opposite directions", as illustrated in figure 1. Whereas light was supposed to be a regular, continuous and periodic wave form the spherical X-ray pulse would be irregular, isolated and independent, the difference accounting for its different properties.

Figure 1. Stokes' spherical aether-pulse.

(i) Section showing propagation from point source.

(ii) Section showing example of irregular disturbance.



In 1903, J.J.Thomson[10] adopted this spherical-pulse hypothesis, and modified it to tie in with some idiosyncratic ideas of his own. To understand these ideas and the quantum concept to which they led, we must go back to the work of Faraday. We have already noted that the aether concept was generally accepted in the 1890's; fifty years earlier, however, the electromagnetic theory was only just emerging, and the later conceptualisations were not yet established. Faraday's ideas were the main source of these later concepts, but in many ways they were radically different from them. J.J. Thomson explained that

> "Faraday was deeply influenced by the axiom, or if you prefer it, dogma that matter cannot act where it is not. Faraday, who possessed, I believe, almost unrivalled mathematical insight, had had no training in analysis, so that the convenience of the idea of action at a distance for purposes of calculation had no chance of mitigating the repugnance he felt to the idea of forces acting far away from their base and with no physical connection with their origin. He therefore cast about for some way of picturing to himself the actions in the electric field which would get rid of the idea of action at a distance, and replace it by one which would bring into prominence some continuous connection between the bodies exerting the forces. He was able to do this by the conception of lines of force." [11]

In the mainstream of physics, these lines of force were soon treated as mere visualisations, without any structural significance. Faraday, however, saw them in a stronger role, as he indicated in 1846:

"The view which I am so bold as to put forth considers, therefore, radiation as a high species of vibration in the lines of force which are known to connect particles and also masses of matter together. It endeavours to dismiss the aether, but not the vibration." [12]

In two papers of 1852, he discussed the physical nature of the lines and concluded in favour of their having a physical (structural) existence.[13] Most physicists rejected this conceptualisation in favour of a continuous aether, but not J.J.Thomson. He developed Faraday's concept of the line of force in his book Recent researches in electricity and magnetism[14] of 1893, and in his Silliman lectures[15] of 1903.

Faraday had postulated magnetic lines, stretching from the positive to the negative pole of a magnet, and electric lines stretching from positively to negatively charged bodies. In line with the general attitude of his time, Thomson regarded magnetism as a secondary effect, so he abandoned the magnetic lines and worked with only the electric ones, introducing a second set of these, from negatively to positively charged bodies, to account for magnetic fields in the absence of electric force. To quantify the lines, he converted them into tubes, and he used these tubes, together with an aether, to explain both radiation and matter, the latter in terms of the mass of aether carried along by the tubes linking electrified particles in the atom. With this conceptual basis for his work, Thomson was working along completely different lines from the other major physicists of his time, lines that already gave the theory of light (constrained to propagation along the tubes) "some of the characteristics of the Newtonian emission theory."

Thomson's theory of light seems to have been developed by 1893, but only in 1903 did he examine its implications and actively propound its virtues. The problem that seems to have led him to this was that of explaining the ionizing property of X-rays. It had been found that X-rays ionized some, but not all, of the molecules of a gas that they passed through, the ionization being independent of the temperature of the gas. This independence suggested that the ionization could not be a function of the energy of a molecule, and Thomson was led to ask why, if the radiation had no structure, some molecules should be ionised but not others. His conclusion was that the radiation did have a structure, and that this was related to the Faraday tubes, as he explained in 1903:

"This view of light as due to the tremors in tightly stretched Faraday tubes raises a question which I have not seen noticed. The Faraday tubes stretching through the ether cannot be regarded as entirely filling it. They are rather to be looked upon as discrete threads embedded in a continuous ether, giving to the latter a fibrous structure; but if this is the case, then on the view we have taken of a wave of light the wave itself must have a structure, and the front of the wave, instead of being, as it were, uniformly illuminated, will be represented by a series of bright specks on a dark background, the bright specks corresponding to the places where the Faraday tubes cut the wave front." [16]

In Thomson's conceptualisation, light (which included, so far as he was concerned, visible light, X-rays and γ-rays) was restricted to discrete tubes; in the case of an X-ray, a γ-ray, or a single wavelength of visible light, the light was further restricted to the intersections of the tubes with individual pulses of roughly the Stokes-Wiechart type, and was therefore localised in all directions. As Thomson wrote in 1907,

"The energy is as it were done up into bundles and the energy in any particular bundle does not change as the bundle travels along the line of force." [17]

The energy content of the bundles was not quantified, but Thomson's concept of light may nevertheless rank as one of the first quantum concepts.

APPENDIX C

PLANCK'S 1911 DERIVATION OF HIS RADIATION LAW

Basing his derivation upon continuous absorption and discrete emission of the total energy content of the resonator, Planck proposed that every time the energy of a resonator, frequency ν, reached a multiple of $h\nu$, this energy should have a probability η of being emitted; alternatively, and with probability $(1-\eta)$, nothing might happen. He did not specify the mechanism behind this behaviour, but he insisted that it was causally determined, though

"The factors which determine emission causally seem to be so deeply hidden that for the time being their laws can only be statistically ascertained."[1]

If P_n was the probability that a resonator had energy in the range $(n\varepsilon, n\varepsilon+\varepsilon)$, the average energy for such a resonator was clearly $(n+\frac{1}{2})\varepsilon$, and the average energy over all possible ranges $U = \sum_n P_n(n+\frac{1}{2})\varepsilon$.

The probability P_n, however, was equal to $(1-\eta)^n\eta$, so that

$$U = \left(\frac{1}{\eta}-\frac{1}{2}\right)\varepsilon .$$

Planck assumed the entropy to be given by $S = k\log W$ and proceeded as he had done in 1900, only since he was now concerned with the distribution of resonators among energy levels, rather than that of energy quanta among resonators, his probability formula for W took the form $W = N!/\prod(m_i)!$, where N was the total number of resonators of a characteristic frequency, and m_i the number in the i^{th} energy level.[2] Thus $W = N!/\prod_n (NP_n)!$, and so $S_N \propto \left\{\frac{1}{\eta}\log\left(\frac{1}{\eta}\right)+\left(\frac{1}{\eta}-1\right)\log\left(\frac{1}{\eta}-1\right)\right\}$.

In equilibrium, $\frac{dS_N}{dU} = \frac{dS_N}{d\eta}\cdot\frac{d\eta}{dU} = \frac{1}{T}$, giving $U = \frac{\varepsilon}{2}\left\{\frac{e^{\varepsilon/kT}+1}{e^{\varepsilon/kT}-1}\right\}$.

At this stage, Planck abandoned his classical formula[3] relating the resonator and radiation energies, but he could do no more than replace it with a completely ad hoc formula, $\rho_\nu \propto \left(\frac{1-\eta}{\eta}\right)$. In terms of U and with the constant factor determined from the Rayleigh limit, $\rho_\nu = \frac{8\pi h\nu^3}{c^3}\left\{U-\varepsilon/2\right\}$, which gave the radiation formula as required.

The difference of the $(-\frac{\varepsilon}{2})$ term between the old and new formulae, $\rho_\nu \propto U$ and $\rho_\nu \propto (U-\varepsilon/2)$, connecting the resonator and radiation energies was necessary to extend the agreement with Rayleigh's law in the red limit to the first order in ε. The abandonment of the old formula removed one of the worst inconsistencies of the 1900 derivation, but, all things considered, the new derivation was as unsatisfactory as the old one. The emission mechanism, the formula for W and the formula just discussed were all essentially ad hoc assumptions, none of them having any individual empirical justification, let alone any theoretical basis.

The publication of the 1911 derivation coincided with the first general interest in the quantum problem, and the the derivation was thus included in textbooks and widely disseminated; but its inadequacies were obvious even to Planck himself, and in 1914 he tried to replace it with a new derivation in which both absorption and emission were assumed continuous, the quantum effects resulting from molecular collisions.[4]

APPENDIX D

THE EHRENFEST AND POINCARE PROOFS OF THE NECESSITY OF QUANTA

Ehrenfest, following Debye and Jeans, considered the black-body problem directly in terms of radiation, or vibrations in the aether. In 1902, Rayleigh had computed the number of mutually independent electromagnetic proper oscillations, in the frequency range $(\nu, \nu+d\nu)$, in a reflecting cavity of volume l^3 as

$$N(\nu)\,d\nu = \frac{8\pi l^3 \nu^3}{c^3}\,d\nu \qquad , \ldots\ldots [12]$$

and had shown that under an adiabatic contraction of the cavity, $l \to l'$, the the frequency and associated energy transformed so that

$$\nu \to \nu', \quad E \to E' \quad ; \quad \nu' l' = \nu l \quad , \quad \frac{E'}{\nu'} = \frac{E}{\nu} \qquad . \ldots\ldots [13]$$

The study of adiabatic processes seems to have been Ehrenfest's favourite approach to a problem, and he adopted it in this case, drawing up six requirements to be satisfied by the black-body radiation. The first three were that

(i) There should be no entropy change in a reversible adiabatic process, ... [14]

(ii) Wien's displacement law should be satisfied (this being independent of the nature of the radiation), $\rho\,d\nu = \alpha\nu^3 f(\beta\nu/T)$, ... [15]
[notation as app. A]

and (iii) In the low frequency ("red") limit, Rayleigh's law should hold,

$\rho \to \frac{8\pi k}{c^3}\nu^2 T$. Writing $\sigma = \frac{\beta\nu}{T}$, this meant that

[f as in [15]] $\quad f(\sigma).\sigma \to 1$ as $\sigma \to 0$ . ..... [16]

The remaining requirements were increasingly strong conditions on the high frequency ("ultraviolet") behaviour, the first merely to avoid the ultraviolet catastrophe, the second to agree in the limit with Wien's law, and the third to agree there with Planck's law. The respective requirements were that

(iv) $\quad f(\sigma).\sigma^4 \to 0$ as $\sigma \to \infty$, ... [17]

(v) $\quad f(\sigma).\sigma^N \to 0$ as $\sigma \to \infty \quad \forall N$, ... [18]

and (vi) $\exists\, L$ s.t. $f(\sigma)\, e^{L\sigma} \to M$ as $\sigma \to \infty$ for some $0<M<\infty$ . ..... [19]

Having established these conditions, Ehrenfest sought the most probable distribution of energy amongst the proper oscillations in the cavity, following Boltzmann's method but allowing an arbitrary weight function in place of Boltzmann's assumption of equal a priori weights for equal volumes of phase space. Defining $\gamma(\nu,E)d\nu$ as the probability of an individual proper oscillation, frequency ν , having energy in the range $(E, E+dE)$, he calculated the joint probability that, of $N(\nu)d\nu$ oscillations of frequency ν , a_1 should have energy E_1 , etc.: $W = \left\{ \prod_i [\gamma(\nu,E_i)\,dE_i]^{a_i} \right\} . [N(\nu)\,d\nu]! \Big/ \prod_i [a_i!]$

Assuming that $S = k \log W$, he obtained the most probable distribution in the usual way, by maximisation subject to the energy constraints

$$\int_0^\infty dE.\, a(\nu, E) = N(\nu) \qquad \ldots\ldots [20]$$

[$\mathcal{E}$ total energy] and $\int_0^\infty d\nu \int_0^\infty dE.\, E\, a(\nu, E) = \mathcal{E}$, ... [21]

$a(\nu, E)$ being the continuous equivalent of the a_i. Equation [20] gave

$$a(\nu, E) = N(\nu)\, \gamma(\nu, E)\, e^{-\mu E} \Big/ \int_0^\infty dE.\, \gamma(\nu, E)\, e^{-\mu E} \qquad , \ldots\ldots [22]$$

for some μ, while [21] imposed the additional conditions that

(i) μ should depend only upon $\mathcal{E}$, ... [23]

(ii) any two choices of weight function, related as $\gamma_2 = Q(\nu)\gamma_1$ for some Q, should lead to the same most probable state, ... [24]

and (iii) γ should always be finite, both pointwise and when summed over a finite energy range. ..... [25]

Next, Ehrenfest considered an adiabatic process $L \to L'$, such that $L = mL'$. From [13], $\nu' = m\nu$ and $E' = mE$, so that $a(\nu, E)\, d\nu\, dE = a'(\nu', E')\, d\nu' dE'$, giving $a' = a/m^2$, $N(\nu') = N(\nu)/m^2$. Application of the entropy requirement [14] gave a variational problem, $\log W' - \log W$ or $\int_0^\infty d\nu \int_0^\infty dE.\, a(\nu, E).\log\left\{\frac{m.\gamma(m\nu, mE)}{\gamma(\nu, E)}\right\} = 0$, with $a(\nu, E)$ subject to [20], and this resulted in $\gamma(\nu, E) = Q(\nu) G(E/\nu)$ for some Q, G.

Returning to the analysis of the most probable distribution, he calculated the radiation density per unit volume, $\rho = \frac{1}{L^3}\int dE.\, E a$, and reexpressed it in terms of G:

$$\rho = \frac{1}{L^3}\int dE.\, E a = \frac{N(\nu)}{L^3}\int_0^\infty dE.\, E.\, e^{-\mu E}\gamma(\nu, E) \Big/ \int_0^\infty dE.\, e^{-\mu E}\gamma(\nu, E) \qquad \text{, from [22],}$$

$[q = \frac{E}{\nu}]$

$$= \frac{8\pi\nu^3}{c^3}\int_0^\infty dq.\, q.\, e^{-\mu\nu q} G(q) \Big/ \int_0^\infty dq\, e^{-\mu\nu q} G(q) \qquad \text{, from [12, 15],}$$

$$= 8\pi\nu^3 f(\nu\mu) \qquad \text{, where}$$

$[C = \frac{\alpha c^3}{8\pi}, \sigma = \mu\nu = \beta\nu/T]$

$$C f(\sigma) = \frac{\int_0^\infty dq\, q\, e^{-\sigma q} G(q)}{\int_0^\infty dq\, e^{-\sigma q} G(q)} \qquad \ldots\ldots [26]$$

He then generalised this to allow for a discrete spectrum (some q_r having singular weights G_r) in addition to the continuous one:

$$C f(\sigma) = \frac{\sum q_r e^{-\sigma q_r} G_r + \int_0^\infty dq\, q\, e^{-\sigma q} G(q)}{\sum e^{-\sigma q_r} G_r + \int_0^\infty dq\, e^{-\sigma q} G(q)} .$$

Having established this result, Ehrenfest could analyse the implications of different choices of G_r and $G(q)$ for the function $f(\sigma)$, and compare the results with his requirements for the high and low frequency limits, [16 - 19], drawing from the comparison conclusions on the possible choices of weight function. In the red limit [16] in which $\sigma \to 0$, $G(q)$ had to $\to 0$ as $q \to \infty$, or at least remain constant. In the ultraviolet limit, itwas clear that $G(q)$ must $\to 0$, as $q \to 0$, as an infinite power of q, for [18] to be satisfied (or faster than the second power for [17]). Moreover, the point $q=0$, $E=0$ had to have a non-zero singular weight function G_0. For satisfaction of [19], the last requirement was clearly unchanged, but the stronger condition was imposed on $G(q)$ that it must be actually zero for $0<q<L$, while the point $q_1 = L$ also had to have a non-zero singular weight function G_1; the former condition arose from M being finite, and the latter from it being non-zero.

Thus, from an analysis restricted essentially to the empirical behaviour in the high and low frequency limits, and without even drawing on Planck's law except in the former limit, Ehrenfest deduced that the weight function, postulated by Einstein as zero apart from finite point values for the energies 0, $h\nu$, $2h\nu$, etc., was necessarily of such a discrete form, commencing by being zero between finite point values for energy zero and some energy L.[2]

Poincaré's approach to the problem was to consider the Planck resonators and to proceed classically as far as possible, making extensive use of Fourier analysis. This gave him, in his own notation,

$$U = -\frac{\phi'(\alpha)}{\phi(\alpha)} \quad , \quad \text{where} \quad \phi(\alpha) = \int_0^\infty w(\eta) e^{-\alpha\eta} d\eta \quad , \quad \ldots\ldots [27]$$

which is in fact the resonator equivalent of equation [26] in our presentation of Ehrenfest's treatment: it gives

$$U = \int_0^\infty w(\eta).\eta.e^{-\alpha\eta} d\eta \Big/ \int_0^\infty w(\eta).e^{-\alpha\eta} d\eta \quad ,$$

and we can identify η with Ehrenfest's νq, α with his σ/ν, and w with his G. Classically, w is a constant, as Einstein had observed in 1907, and Rayleigh's law is recovered. To obtain Planck's law, Poincaré followed Einstein in giving w a discrete form, but he did this by replacing the Fourier integral for $\phi(\alpha)$ in [27] with the Fourier sum, $\phi = \sum_{m=0}^{\infty} w(m\varepsilon) e^{-m\varepsilon\alpha}$.

Thus the weight function was kept constant, but the values over which it was defined were changed. The result was $\phi = (1-e^{-\alpha\varepsilon})^{-1}$, leading to

$U = \varepsilon \big/ \{e^{\alpha\varepsilon} - 1\}$, in agreement with Planck.

Incorporating the ground state energy, $\frac{\varepsilon}{2} = \frac{h\nu}{2}$, which Planck had introduced in 1911, Poincaré argued that Planck's formula for U was equivalent to

$$\phi = e^{-\alpha\varepsilon/2} + e^{-3\alpha\varepsilon/2} + e^{-5\alpha\varepsilon/2} + \ldots$$

so that w was necessarily zero except for odd multiples of $\varepsilon/2$. He insisted, moreover, that he had made no physical assumptions, and that his equations applied at all times to all resonators. Thus no resonator could at any time adopt energy values other than the odd multiples of $\varepsilon/2$. Finally, he proved that any distribution leading to finite total radiation must involve some discontinuity.

APPENDIX E

QUANTUM PHENOMENA AND STRUCTURAL SPECULATION

The hallmark of the 'structural' physicists was speculation, and the greatest speculator of them all was J.J.Thomson. Although he had been one of the originators of a light-quantum concept, his concept had been classical in the sense that it involved a disturbance in the aether rather than an independent existent, and as such it had to be accounted for in terms of atomic behaviour. In 1910,[1] he put forward an ingenious suggestion of how this might be achieved, interpreting the quanta as resonance phenomena resulting from the interaction between an incoming light wave and the motion of electrons round electric doublets in a pre-Rutherford atom. Jeans[2] criticised this on the unarguable grounds that it did not give the required result, and in 1913 Thomson[3] tried again, postulating an atom within which were electrons experiencing "a radial repulsive force varying inversely as the cube of the distance from the centre, diffused throughout the whole of the atom" and also "a radial attractive force, varying inversely as the square of the distance from the centre, confined to a limited number of radiant tubes in the atom."[4] The idea behind this curious concept was that at some point within each tube the two forces would balance, allowing an electron to remain in equilibrium, while if the electron could be shifted sideways out of the tube by a resonance of incoming light, the repulsion would eject it from the atom with a total kinetic energy equal to a finite constant related to h.

Jeans[5] performed the same service for the new model as he had done for the old, but, the same year, Thomson's light-quantum concept was itself developed for the first time since its inception, by McLaren.[6] There was a strong British tradition from the nineteenth century of the study of magnetism, and McLaren believed firmly that this should be treated as a primary rather than as a secondary phenomenon, and that [7]

> "The magneton ought to hold an equal place with the electron in our account of the nature of matter, neither is to be explained in terms of the other."

McLaren's magneton had the structure of a ring electron, but the concept fell within Thomson's general theory of matter and radiation, and McLaren viewed matter, composed of electrons and magnetons, as a structure in the aether in the form of "internal boundaries of the electromagnetic field", the magneton boundary being ring shaped, and its properties being explained in terms of Faraday tubes:

> "The tubes of electric induction which terminate on its surface give it an electric charge, the magnetic tubes linked through its aperture make it a permanent magnet."

He was able to prove[8] that the angular momentum of the magneton was proportional both to the number of tubes of electric induction and to the number of tubes of magnetic induction.

Unfortunately, McLaren was killed during the war, and did not himself take the theory any further, but in 1916 Bernoulli[9] extended his result to the case in which the ring electron was replaced by a classical electron moving in a closed circular orbit, and in 1921 Allen[10] put this work on a more rigorous basis and extended it to elliptical orbits. Allen also investigated the consequence of attributing to the magneton a natural unit of angular momentum, defined by Planck's constant h, and found that the product of the numbers of electric and magnetic tubes was proportional to nh. Defining a unit electric tube by the value e, he deduced that [11]

> "The simplest explanation of the results is based on the assumption that we are dealing with discrete magnetic tubes, the unit tube being defined by (h/e)."

Allen related these tubes to the theory of absorption and emission of radiation by associating them with the absorption and emission frequencies, such that emission, for example, consisted of the dissociation of one tube from the system; in 1925, following a suggestion by E.T.Whittaker, he[12] considered the rotation of magnetic tubes around the axis of a magnet and came to a "speculative" conclusion that one could in fact visualise electrons and positively charged bodies as magnetic tubes in rotation. This brought light and matter back to the same basic constitution, a triumph for British speculation. The final sophistication of the Faraday tube concept was to connect it with the special theory of relativity: this was suggested in 1921 by both Whittaker[13] and Allen[14] and developed by Whittaker, the resulting four-dimensional quantised magnetic Faraday tubes being christened "calamoids".[15]

Related to the calamoid concept was another of Thomson's hypotheses,[16] the electric ring quantum of 1924. The idea here was that the mutual energy of a bound electron and positive charge (in the atom) might be viewed as being located not in the particles themselves but in the Faraday tubes of electric induction joining them. During the motion of the electron the tubes might conceivably be thrown into a loop, which might conceivably detach itself from the particles and escape as a closed ring, quickly becoming circular, and moving with the velocity of light in the direction of its axis. For absorption the process would be reversed.

The significance of Thomson's electric ring quantum concept was that it involved a peculiarly British conception, namely magnetic current (passing in this case through the aperture), and this was present also in Whittaker's atomic model of 1922.[17] This was considered so important that it was supported at its presentation by subsidiary papers of approval by Professors Allen, Houston and Peddie,[18] three of the most eminent physicists in the 'structural' tradition. Moreover, the two major continental physicists with the closest ties with Britain, Bohr and Lorentz, both commented upon the model, making it one of very few structural speculations to reach the continent. Bohr mentioned it in a 1923 summary paper, writing that it "may point out the direction in which a complete, comprehensive picture of the processes is to be sought in the future",[19] while Lorentz considered[20] it in his role of grand old gentleman of physics, translator and generaliser of any new ideas that came his way.

In common with many of the speculative physicists, Whittaker believed in the existence of a classical mechanism behind the quantum theory, and he was more ingenious than most in suggesting such a mechanism. He considered an electron incident upon an atom, without enough energy to ionise it, but with energy of the correct order of magnitude for some of it, $U = h\nu$, to be absorbed by the atom. In this case, as he correctly observed, "The experimental results indicate that the electron, as it approaches, experiences a repulsion which is sufficient to turn it back altogether if it's kinetic energy is less than $h\nu$." [21]

Such a repulsion, he claimed, could only come from an electric or magnetic force, and it must be the former in this case, since only motion through an electric field could affect the kinetic energy, on which "the fate of the electron depends". So, he deduced,

"An electron approaching the atom experiences, in the vicinity of the atom, a field of electric force." [22]

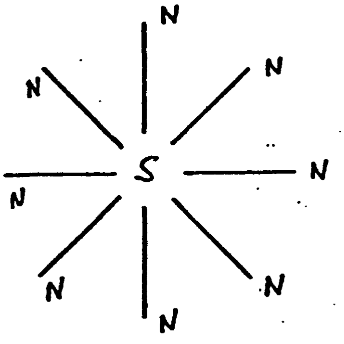
Since the atoms did not respond to a normal electric field, he argued that they could clearly not sustain permanent force fields, and that, hence, "the electric field about an atom ... is created by the approach of the electron." [23]

To provide a mechanical explanation for this behaviour, Whittaker suggested a model based upon one Ewing[24] had expounded the same year for diamagnetism, in which

"The electron, as it approaches the atom, induces within the atom a 'magnetic current', that is, the magnetic analogue of an electric current: or at any rate induces something which behaves like a magnetic current." [25]

He imagined a wheel of bar magnets with like poles to the centre, as indicated in figure 2. An electron approaching along the axis would, he argued, set the whole wheel turning, thus producing the magnetic current required. If the velocity of the electron were below a critical value, the collision would be elastic, but if above, the electron would pass through the atom, losing a fixed amount of energy on the way. The energy imparted to the angular motion of the wheel would, in the elastic case, be returned to the electron on its way out, but would otherwise be given up for good. Further, taking the fixed amount of energy given up to be U, Whittaker argued that the energy absorbed by the atom was taken into a closed magnetic current, and that was equivalent to an electric shell, equivalent to a charged condenser, equivalent when discharging to a Hertzian oscillator.[26] By fixing the capacity and inductance of the condenser in terms of h, it would be easy to arrange that $U=h\nu$ as in the quantum theory.

Figure 2.



Whittaker's mechanism was generalised by Baker,[27] and also by Lorentz.[28] Lorentz was attracted by "the possibility of a sharp criterion by means of which it can be decided whether an encounter is effective or otherwise", but he was forced to conclude that

"The hypothesis of 'magnetism' existing independently of electric currents is quite essential to Whittaker's model. I need not speak at length of the reasons for which such an assumption is not to be readily entertained." [29]

A similar objection was put forward by Eldridge, who observed that the mechanism as it stood would not work because of the contribution of the central pole, which Whittaker had omitted from his considerations. What the model required if it were to work, he pointed out, was a unipolar magnet, "and a unipolar magnet is a conception abhorrent to physics."[30] Lorentz actually looked at the consequences of modifying the classical field equations to include a magnetic current density, and found that such a system would at least be consistent on the surface, but he could not overcome his opposition to the conception of magnetism involved. This opposition, shared by physicists in general, proved crucial in respect of both Whittaker's atomic model and Thomson's electric ring quantum concept, and with the rejection of these conceptions, followed quickly by the development of the new quantum theory, the tradition that they represented came more or less to an end. There had been other structural speculations as well as those considered, but the most important of these, the static models of the atom suggested by Langmuir,[31] Thomson,[32] and Parsons,[33] were associated by Allen[34] with Whittaker's model, and seem to have died with it.

When compared with the developments in the mainstream of quantum theory, these structural speculations may seem to us almost farcical, but to dismiss them as such would be grossly unfair, for the difference between the two approaches was purely methodological. Thomson and Whittaker did not believe that their models represented physical reality, but rather saw these models as visualisations, proposed with a view to learning more about the behaviour they represented. The approach may not have been fruitful in the quantum context, and there are strong grounds that it is generally inapplicable to the physics of the twentieth century, but in adopting it they were following a long and successful tradition. As we have noted, Thomson's ideas were developed from those of Faraday, and, in a paper on the physical nature of the magnetic lines of force,[35] Faraday had defended the structural approach, which was that he himself had adopted: structural speculations "should ever be held as doubtful, and liable to error and to change",[36] but

> "It is not to be supposed for a moment that speculations of this kind are useless, or necessarily hurtful, in natural philosophy...... they lead on, by deduction and correction, to the discovery of new phaenomena, and so cause an increase and advance of real phys cal truth, which, unlike the hypothesis that led to it, becomes fundamental knowledge not subject to change." 37

This was the justification of Thomson's speculations, too, and he often stressed the fact that his models were meant only as visualisations, while Whittaker, taking a similar view, was not at all perturbed by Eldridge's criticism of his bar magnet mechanism, claiming that it was only the resulting conception of magnetic current that really mattered.[38] The conception was itself generally rejected, largely because all the available evidence seemed to dictate against the existence of magnetic monopoles, but it also had strong roots in tradition, a magnetic current density having been suggested in 1885 by Heaviside.[39] When looking at these unsuccessful speculations we should also bear in mind the state of physics today (conceptually little advanced from the twenties, but technically immeasurably so), in which magnetic monopoles are proposed without screams of "abhorrent", and in which Faraday tubes too find their counterpart, in the much discussed "strings".

APPENDIX F

EINSTEIN'S 1916 DERIVATION OF PLANCK'S LAW

Einstein assumed the Maxwell-Boltzmann canonical distribution for the states of the atom, as he had done in 1907, but this time he allowed general (but discrete) energy levels, E_n, and a general weight function, p_n, so that $W_n = p_n \exp\{-E_n/kT\}$. Thus he no longer assumed that the weighting of the states was equal, and this allowed a free choice of quantum statistics. Using the transition probabilities derived from analogy with the classical case (see main text), he required that there should be equilibrium between each pair (m,n) of states, so that

$$W_n B_n^m \rho = W_m \{ A_m^n + B_m^n \rho \} ,$$

or $$p_n \exp\{-E_n/kT\} . B_n^m \rho = p_m \exp\{-E_m/kT\} \cdot (A_m^n + B_m^n \rho) .$$

To derive Planck's law, he made the further assumption that ρ should tend to infinity with temperature, T. This gave $p_n B_n^m = p_m B_m^n$, and, from comparison with Wien's displacement law[1], $E_m - E_n = h\nu$ and $(A_m^n/B_m^n) \propto \nu^3$, so that Planck's law followed, the constant factor being taken as usual from the Rayleigh law in the red limit[2].

In this derivation, the radiation was considered in connection with its source in the atom, but since it was the radiation energy and not the atomic energy that was analysed the formula connecting the two, for which there was still no satisfactory derivation, was avoided. Most intriguingly, Einstein appeared on the surface to have made no assumption corresponding to Planck's probability formula (or to the non-independence of quanta), since the weight function p_n was not defined. What he actually did was to replace this assumption, which, though fundamental, was not physically intuitable, with assumptions concerning observable properties bringing out the fundamental peculiarities of the quanta. Classically, there was nothing more natural than the assumption of stimulated emission, but this was a property for the comprehension of which the classical wave concept of light was essential, and when transferred by Einstein to a theory of light-quanta it suddenly aquired great significance.[3] It was perfectly natural for a wave to stimulate a secondary wave of the same frequency, but there was no obvious reason why the same should be true of particles: one would have to assume that the incoming quanta were somehow connected with quanta in the atom of the same frequency, or in other words that quanta of the same frequency were not independent. If we in fact omit the stimulated emission from Einstein's calculations, and allow ρ to remain finite (as it must in this case to avoid infinite emission in the absence of a field), we can easily derive Wien's law.[4] The assumption of stimulated emission ensured a wavelike behaviour, and the requirement that ρ should tend to infinity determined the extent of the stimulated emission in terms of the free emission, and ensured that the wave behaviour entered into the considerations completely. Einstein assumed a particle structure of light, but his other assumptions ensured that, in their interactions with one another, the particles acted like waves.

Unfortunately, Einstein himself did not bring out the nature of his assumptions, and , like earlier derivations, the new one was a mixture of wave and particle assumptions, without it being clear which was which. In 1925 Eddington[5] showed that the canonical distribution assumed by Einstein could in fact be deduced from Wien's displacement law and the Bohr frequency relations ($E_1 - E_2 = h\nu$). He also noted the importance of the asymptotic behaviour of ρ, and the fact that this had not been previously noted is indicative of the lack of attention that Einstein's assumptions received.

APPENDIX G

DE BROGLIE'S DERIVATIONS OF WIEN'S LAW AND OF PLANCK'S LAW

Wien's law (1922)

De Broglie derived Wien's radiation law from relativistic particle mechanics. Considering n particles in a unit volume, he adopted the standard canonical distribution for the number, dn, in an element $dx\,dp$ of phase space,

$$dn = C\,e^{-E/kT}\,dx\,dp \quad ,$$

and deduced the number dn_E of energy E, $\quad dn_E = C'e^{-E/kT}\,p^2 dp \quad ,$

where E, p, are the relativistic kinetic energy and momentum, and C, C', are constants: $\quad E = m_0c^2\left\{\frac{1}{\sqrt{1-v^2/c^2}} - 1\right\}$, $\quad p = \frac{m_0 v}{\sqrt{1-v^2/c^2}}$.

Substituting for p, v, $\quad dn_E = C'e^{-E/kT}\,m_0c^2\left\{\frac{E}{m_0c^2}+1\right\}\sqrt{\frac{E}{m_0c^2}\left\{\frac{E}{m_0c^2}+2\right\}}\,dE$,

and in the material limit, $E \ll m_0c^2$, this gave Maxwell's formula, while in the radiation limit, $E \gg m_0c^2$, it gave $\quad dn_E = c'\,e^{-E/kT}\,E^2 dE$.

From this, he obtained the average energy of a light-quantum,

$$dU_E = \frac{n\,e^{-E/kT}}{2k^3T^3}\,E^3\,dE \quad ,$$

and, by integration the total energy, $U = 3nkT$. Applying the usual entropy requirement for equilibrium, n and T being treated as variables, he got $n = Ak^3T^3$, leading to Wien's law, namely

[A constant] $\quad dU_E = \frac{A}{2}\,e^{-E/kT}\,E^3\,dE = \frac{A}{2}\,h^4\nu^3\,e^{-h\nu/kT}\,d\nu$.

Planck's law (1923)

There were only two important derivations of Planck's law within the period covered by this chapter, but they were both very important indeed. The first was de Broglie's derivation from his theory of matter waves.[1]

To derive Planck's law, de Broglie imposed the equilibrium condition that the wave pattern resulting from the light-quanta should be stationary, i.e. a standing wave. He adopted the standard formula for the number of waves in a unit volume, $n_\nu d\nu = \frac{4\pi \nu^2 d\nu}{U v^2}$ $[U = c\beta, \beta = 1/\sqrt{1 - v^2/c^2}]$, and substituting for ν, U, v, he found

$$n_\nu d\nu = \frac{4\pi}{h^3} m_0 c^2 \left(1 + \frac{E}{m_0 c^2}\right) \sqrt{\frac{E}{m_0 c^2}\left\{2 + \frac{E}{m_0 c^2}\right\}} \cdot dE ,$$

as in his earlier derivation, which he proceeded to follow. As before, he had to introduce a 'molecular' term, $\sum e^{-nE/kT}$ in place of the factor $e^{-E/kT}$, but this time he had an explanation, namely that

"Each phase wave can carry with it one, two, or more atoms"[2]

of light or gross matter. The required non-independence of light-quanta arose from the fact that two quanta could spatially overlap, in the sense that their phase waves could coincide, and he expressed this clearly in his Thèse:[3]

> "If two or more atoms have exactly superposing phase waves so that one can say in consequence that they are transported by the same wave, their movements cannot be considered as entirely independent and the atoms can no longer be treated as distinct unities in the calculation of probabilities."

An added triumph of the derivation was that despite the matter waves the gross matter limit still gave Maxwell's law, for the higher terms in the molecular expansion disappeared in this limit.

In a sense, De Broglie's derivation was the closest anyone had come to a structural derivation of Planck's law. His assumptions were strange, and were not generally acceptable, but they were clearly stated and, apart from the problem of conceiving of multi-dimensional phase waves, physically intuitable. They were, at least, more intuitable than those of Bose's derivation of 1924, which quickly became the 'standard' derivation, for as Dirac commented in 1925,[4]

> "It is a disadvantage of the present theory [Bose's] that the cells play so important a part in it. One assumes that the whole of phase space is divided into a number of compartments, and each atom or light-quantum as the case may be is definitely in one compartment. One can get over this 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 of waves associated with a given region in phase space is equal to the number of cells into which that region of phase space was divided in the previous theory. This results in the two theories being mathematically equivalent."

In general, however, de Broglie's derivation was soon forgotten, while Bose's behavioural treatment, adopted by Einstein and applied by him to matter as well as to light, became the standard derivation of Planck's law, and has in fact remained so ever since.

APPENDIX II

NOTES ON CAUSALITY AND QUANTUM THEORY

FORMAN'S THESIS

In 1971, Paul Forman published a paper entitled "Weimar Culture, Causality, and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment" (FORMAN [1971]). This was a pioneer work in its field, and has generally, and rightly, been hailed as a great achievement, being the first break from the 'equations plus dates' methodology in the history of twentieth century physics. Unfortunately, there seems to me to be a serious discontinuity between Forman's evidence and his conclusions, so far as the latter are presented in the paper. A weak thesis, relating to the paper's subtitle, can be defended: adaptation to the environment was one factor in the pronouncements, and possibly also the ideas, of some German physicists and mathematicians. But Forman appears to claim much more than this; I shall first outline his thesis as I understand it, and then discuss it.*

Forman's thesis is presented in two parts. In the first place he finds that

> "in the years after the end of the First World War but before the development of an acausal quantum mechanics, under the influence of 'currents of thought', large numbers of German physicists, for reasons only incidentally related to developments in their own discipline, distanced themselves from, or explicitly repudiated, causality in physics.

> "Thus the most important of Jammer's theses — that extrinsic influences led physicists to ardently hope for, actively search for, and willingly embrace an acausal quantum mechanics — is here demonstrated for, but only for, the German cultural sphere." [p.3]

Secondly, he undertakes a "causal analysis" of the phenomenon, asking why extrinsic influences should play a part in German quantum physics after 1918, when "anticausal sentiments among a variety of late nineenth-century philosophers — French, Danish, and American" had little or no effect earlier in the century. He finds his answer in accomodation:

> "I show that in the aftermath of Germany's defeat the dominant intellectual tendency in the Weimar academic world was a neo-romantic, existentialist 'philosophy of life', revelling in crises and characterised by antagonism toward analytic rationality generally and toward the exact sciences and their technical applications particularly. Implicitly or explicitly, the scientist was the whipping boy of the incessant exhortations to spiritual renewal, while the concept — or the mere word — "causality" symbolised all that was odious in the scientific enterprise." [p.4]

> "There was in fact a strong tendency among German physicists and mathematicians to reshape their own ideology toward congruence with the values and mood of that environment — a repudiation of positivist conceptions of the nature of science, and, in some cases, of the very possibility and value of the scientific enterprise." [p.7]

> "I am convinced, and ... endeavour to demonstrate that the movement to dispense with causality in physics, which sprang up so suddenly and blossomed so luxuriantly in Germany after 1918, was primarily an effort by German physicists to adapt the content of their science to the values of their intellectual environment." [p.7]

In particular, Forman finds a very strong influence in Spengler's Decline of the West:

> "Most German mathematicians and physicists largely participated in, or accomodated their persona to, a generally Spenglerian point of view." [p.55]

* All references in square brackets are to pages in FORMAN [1971]

Of this view, Forman says that

> "Over and over again Spengler equates causality, conceptual analysis, and physics, and flays them across the stage of world history." [p.33]
>
> "Spengler's indictment of physics=causality is all the weightier because he pretends to be a connoisseur of the physical sciences and modern technology." [p.34]

The Weimar milieu and the Decline of the West

The first criticism I must make of Forman is that he caricatures the milieu. The tendencies he describes were present, but neither so dominant nor so extreme as he makes out. He emphasises the importance of the defeat in the war, which heightens the drama but distorts the reality: two of his prime historical sources, Ringer and especially Gay, stress that the origins of the milieu were pre-war, and that this was indeed so is clear from a moments thought on the history of art, say, or music. Spengler wrote his book while Germany still expected to win the war.

Spengler's book is also caricatured by Forman: it was not anti-causality, or anti-physics, although it was probably interpreted as such. Spengler compared our declining civilisation with earlier ones, and used the comparison to make predictions; these included a move away from original science, but also a move away from original art and philosophy, towards historical subjects, technology, and routine reproduction. The move was from causality to destiny, but both were characteristic of our present civilisation, and hence of the next few hundred years: science within our civilisation was causal and would remain so, as long as it remained at all. From Spengler's asserted equivalence of the values of different scientific criteria for different civilisations could be inferred anti-causality, but only on the same basis that an assertion of the equal validity of the major religions will be often as anti-Christian by a Christian, anti-Jewish by a Jew, etc.. The element of anti-causality must therefore be placed in the reception of Spengler's work rather than in the work itself, and we should note that the most "anti-causal" quotes used by Forman were all from the first edition of the book (1918), deleted from the second edition (1923) — hardly consistent with an eager reception!

Concerning the milieu in general, the historians quoted by Forman largely excluded science from their theses, and the same is true for the contemporary observers quoted: Forman even admits that Troeltsch confined himself explicitly to the humanities, but assumes that the same views can be taken over to science. Scientists were naturally affected by the milieu (which did have a very strong element of Lebensphilosophie), but not as Forman makes out. Thus Born's longing to participate in a 'whole' was typical, as claimed, [p.47] of the milieu, but in the very same passage Born described Goethe's attitude as a "fruitless rebellion against the greater power of science" — not very accomodating. I have noted in the main text that Einstein did not accomodate in respect of the causality issue, Forman's claim that he did resting on a clear misinterpretation, and Forman also seems to have misinterpreted Einstein on Spengler: "Spengler hasn't spared me either" was not a reaction to Spengler's "digging" at him, but merely a statement that the ideas in Spengler's book had been occupying his attention. [p.71]

Again, Sommerfeld's number mysticism [p.50] cannot be blamed on the environment, and from what I have said it is clear that von Mises' argument in 1920, [p.49] that technology was out and speculative natural science in, was not Spenglerian. When von Mises did adopt a Spenglerian viewpoint it was the genuine, not the inferred one. [p.51]

If scientists speaking publicly replaced the utilitarian motive with the culture motive as a justification for science, this was not necessarily a positive adaptation, for most of them, including such staunch determinists as Einstein and Planck, had always held the latter view. In this respect and in general, the milieu was relevant, but not a positive factor. Irrationality became respectable, but rationality was not rejected, and the same was true of causality. There was opposition to scientists, but more because they were Jewish than because they were scientists, and to that there was no adaptation.

Having argued that Forman's picture of the milieu is a gross exaggeration, however, I must agree that there is an element of truth in it. The milieu was potentially significant, and did contain a strongly anti-causal element. There was a crisis in physics, and there was debate about the causality issue. Accomodation to the milieu can be seen in the public lectures (though not the private writings) of some physicists and mathematicians. It is only when he makes assertions about the community at large, or about the quantum physicists, that I totally disagree with him: I shall restrict my analysis here to the latter point.

German quantum physicists and causality

Forman discusses many "converts to acausality", but most of them seem to be irrelevant to his strong thesis. Exner, [p.74] he admits, was influenced by nineteenth century philosophical ideas. Even in the passages from Nernst that he quotes, it is clear that far from trying to "sink the law of causality by hook or by crook" Nernst is most anxious to retain it, as a possibility at least, despite the internal evidence against it, and despite the influence (admittedly active here) of the milieu. [p.85] Senftleben [p.98], as I have argued in the main text, did not adopt anti-causality, and there is nothing to connect his work with the milieu. Von Mises [p.80] had nothing to do with quantum theory, and little to do with physics at all. Schrödinger [p.87] was influenced, on his own admission (see main text), primarily by Exner, and reconverted to causality when he escaped this influence. Reichenbach's [p.88] anti-causality was too late for Forman's thesis, and almost certainly based on academic philosophical considerations: he was a philosopher of science rather than a scientist, anyway. Weyl's interest in quantum theory commenced only in 1925, while his opposition to causality [p.76] was again based on academic philosophical considerations and dated from well before the Weimar era.

In short, the only physicist who fits well into Forman's thesis is Schottky, who seems to have advocated anti-causality under the influence of the milieu, but who is hardly a significant character in the history of quantum theory.[p.82]

Non-German physicists and causality

Outside Germany, there is no greater tendency toward anti-causality than inside, but there is no less either. Darwin considered free will "in the last resort", while Bohr rejected causality (for reasons reviewed in the main text) outright. In fact the first significant appearance of anti-causality in quantum theory was in the Bohr-Kramers-Slater paper of 1924, written by a Dane, a Dutchman (who's views on the subject are not in fact clear) and an American (who in fact believed in causality).

Reactions to Bohr-Kramers-Slater

Forman suggests that the reactions to the BKS paper support his thesis,[p.98] but I find it very difficult to see how. As I have argued in the main text, those physicits who adopted the BKS theory did so almost unanimously without accepting the implications for causality and energy conservation. The only exception to this was Schrödinger, whose reasons were, as noted, independent of the milieu. Opposition to the physical implications of BKS was however widespread, led by Einstein, Pauli, and Sommerfeld (from the German sphere), and by Compton, van Vleck, and Ehrenfest (from outside).

Conclusion

As I have indicated in the main text, I find the main impetus toward anticausality in quantum theory to have been internal, and closely linked with the absence of a space-time description. This is a surprising conclusion, in that the agreement of internal considerations with prior external developments seems to call for some explanation: why should the physics of the 1920s reflect the philosophy, literature, art etc. of an earlier period, if not influenced by it? But it is nevertheless a conclusion in which I have considerable confidence. The introduction of anti-causality to main-stream quantum theory, first by Bohr and later by Born, may have been motivated by external factors, in Bohr's case philosophical and in Born's as yet unknown, but the acceptance of the fact that causality could no longer be upheld seems to be an internal one: as Heisenberg explained in 1927, the real problem was that physics found itself no longer capable of defining a present state on which a causal prediction of the future could be based, and causality thus became irrelevant.

APPENDIX J

SOME DERIVATIONS OF PLANCK'S RADIATION LAW

Einstein (1907)

Einstein recognised the contradiction between Planck's discrete energy hypothesis and his use of a purely classical formula (appendix A, eqn.[3]) to link the resonator and radiation energies, but he could not resolve this problem and so gave only a derivation of resonator formula (eqn. [10]). For this, he started with Boltzmann's canonical probability distribution,

[C; R constants; N oscillarors of energy E, momentum p.]
$$dW = Ce^{-NE/RT}dp \, .$$

He then let $\int_{dE} dp = w(E)\,dE$ so that $dW = Ce^{-NE/RT}w(E)\,dE$.

In the classical theory, he argued that $w(E)$ must be constant, since $dW = Ce^{-NE/RT}dx\,d\xi$ with $\int dx\,d\xi = \text{const.}\,dE$, and this gave $dW = C'e^{-NE/RT}dE$ which led to energy equipartition and thus to Rayleigh's law, since $u = \int EC'e^{-NE/RT}dE \Big/ \int C'e^{-NE/RT}dE = RT/N$.

To obtain Planck's law, Einstein adopted the assumption
$$\int_0^\alpha w\,dE = \int_{\varepsilon}^{\varepsilon+\alpha} w\,dE = \int_{2\varepsilon}^{2\varepsilon+\alpha} w\,dE = \dots = A\text{, constant},$$

for some ε and very small positive α. The probability of a resonator having energy infinitely close to $n\varepsilon$ was then given by
$P(n\varepsilon) \propto e^{-n\varepsilon N/RT} = e^{-n\varepsilon/kT}$, leading to average energy in agreement with equation [10], $$u = \sum (n\varepsilon)P(n\varepsilon) \Big/ \sum P(n\varepsilon) = \frac{\varepsilon}{(e^{\varepsilon/kT}-1)}.$$

In some ways, this was little better than Planck's original derivation of 1900. From the conceptual point of view, the most important innovation was to replace Planck's probability formula with the genuinely physical assumption that a resonator, frequency ν, could only have energy in amounts $n_i\varepsilon$ (empirically, $\varepsilon = h\nu$), where the n_i were integers, and that it could have all such energies with a priori equal probability. This assumption was not fully satisfactory, for it begged the question how such a state of affairs was to be achieved in terms of the behaviour of the radiation; it was, however, relatively clear, and given that some sort of quantisation was necessary it seemed eminently reasonable. Moreover, the nature of the divergence from the classical theory was made quite clear, whereas it had not been by Planck. That Einstein's form of counting was equivalent to Planck's was not immediately clear, but this point does not seem to have been taken up be anyone, at least in print. The connection is fairly simple, but may not have seemed so at the time, for there was no physical portrayal of Planck's probability formula. This was provided by Ehrenfest and Kammerlingh-Onnes in 1914.

The apparent lack of concern over the connection seems to reflect a general lack of interest in Einstein's derivation. Later derivations (of 1909-10) were based on Planck's version of the probability formula rather than on Einstein's more intuitable one.

One other point to notice in connection with Einstein's derivation is that it avoided the use of entropy, a desirable simplification for those not sharing Planck's views on this concept. Due to the direct connection, $S = k \log W$, between entropy and probability, it was in a sense rather perverse (albeit quite understandable) of Planck to insist on expressing entropy in terms of probability and then maximising the former, rather than just maximising the latter.

Lorentz (1910)

Lorentz also avoided the use of entropy, and simply maximised the joint probability distribution of two systems, characterised by energy elements $\varepsilon_1 (\sim h\nu_1)$ and $\varepsilon_2 (\sim h\nu_2)$, using Planck's probability formula for the individual systems. Maximizing $\frac{(n_1+p_1-1)!}{(n_1-1)!\, p_1!} \cdot \frac{(n_2+p_2-1)!}{(n_2-1)!\, p_2!}$ for $p_1 \varepsilon_1 + p_2 \varepsilon_2 = E$ gave immediately $\frac{1}{\varepsilon_1} \log \left\{ \frac{n_1}{p_1} + 1 \right\} = \frac{1}{\varepsilon_2} \log \left\{ \frac{n_2}{p_2} + 1 \right\}$ leading to energy proportional to $\varepsilon / (e^{c\varepsilon} - 1)$, equivalent to Planck's equation [10]. Lorentz still relied on the classical part of Planck's derivation to convert to equation [11], but a comparison of the above with Planck's derivation of [10] suggests that Lorentz's version would at least prove less daunting to physicists looking at the problem for the first time.

Larmor (1909)

Seeking to avoid the quantum hypothesis, Larmor attempted to generalise Planck's derivation by replacing the assumption of discrete energy elements $\varepsilon = h\nu$ by the assumption that the ratio of the energy element to the extent of a standard unit cell should be an absolute physical quantity. Planck's resonators of frequency ν were treated as unit cells with extent in proportion to ν, available for occulation by energy elements proportional to ε, the constant of proportionality being the same in both cases. The point of this modification was to avoid the quantisation of the energy element itself. The result $\varepsilon = h\nu$ was seen as a deduction from the more general $a\varepsilon = ah\nu$, with a a constant, and Larmor thought it most likely that the constant would in fact tend to zero, and with it the energy element. Jeans apparently shared this view.

Larmor's derivation was an attempt to evade the discreteness of energy, in tune with Jean's reaction to Planck's law in 1905. Physically, it was not particularly satisfying, involving something like an infinity of mini-resonators of infinitesimally small extent, each emitting and absorbing infinitesimally small quantities of radiation, even over a finite period. But to others wishing to evade the energy quanta, it must have offered some hope, and even as late as 1915, when the necessity of quanta was well established, Larmor's approach was referred to with approval by Livens.* Larmor recognised that discrete energy elements were being seriously advocated, and thought that this should, and possibly could, be resisted, but he realised that it could not merely be dismissed.

*LIVENS, G [1915] PM 29 p.388, "The law of partition of energy and Newtonian mechanics".

Bose (1924)

Bose treated radiation directly, without considering the resonators, and assumed that it was composed of quanta in the form of zero rest mass particles. He then considered the volume of phase space available to those quanta in the energy range $(h\nu, h\nu + h\,d\nu)$, which, if V were the total volume, was given by

$$\int dx\, dp = 4\pi p^2 dp V = 4\pi \left(\frac{h\nu}{c}\right)^2 \frac{h}{c} d\nu . V .$$

[p = momentum]

Considering this as divided into elementary unit cells of measure h^3, the number of such cells corresponding to the given energy range was $\frac{4\pi}{c^3} \nu^2 d\nu . V$, or, since the polarisation of the light had to be accounted for, $A = \frac{8\pi V \nu^2 d\nu}{c^3}$. Bose then considered in how many ways N quanta could be distributed over the A cells, and found this to be $A!/p_0!\,p_1! \ldots$, where p_i was the number of cells containing i quanta. Identifying $\prod_s \left(A^s!/p_0^s!\,p_1^s!\right)$, where the superscripts represented possible frequencies, with the state probability W, $S = k \log W$, he required that there should be equilibrium subject to the total energy restriction $E = \sum N_s h\nu_s$, where $N_s = \sum_r r p_r^s$. This led, for the total energy of the radiation, to $E = \sum_s \left[8\pi \frac{h\nu_s^3}{c^3} \left\{\exp\left(\frac{h\nu}{kT}\right) - 1\right\}^{-1} d\nu_s\right]$, equivalent to Planck's law.

The first stage of Bose's derivation, giving the overall constant factor without recourse to classical theory, was all that was really new, and it was a simple development of Planck's 1911 hypothesis of elementary units of equal probability. But the derivation was important for what it omitted rather than for what it contained. It was assumed that light was composed of quanta defined by h, which also defined the unit cells in phase space, and that these quanta obeyed Planck's probability formula, or the Bose-Einstein statistics; that was all. In behavioural terms, Bose's was easily the simplest derivation of Planck's law.

APPENDIX K

NOTES ON QUANTUM STATISTICS

The connection between Bose-Einstein and Fermi-Dirac statistics

Heisenberg's confusion on this issue prompts us to ask in what way the two sets of statistics can be seen as similar. They both involve an element of wave-particle duality of course, but we can go beyond this. In the first place, in respect of Planck's choice of formula for the second derivative of entropy, the most general choice he could have made would have been not $B/u(u+A)$ (as a compromise between $1/u$ and $1/u^2$) but $(Cu+D)/u(u+A)$; this includes both Bose-Einstein and Fermi-Dirac statistics as special cases. In the language of de Broglie, according to which light-quanta were interpreted as 'moecules' of light, Planck's law being derived from Wien's as

$$e^{-h\nu/kT} \rightarrow e^{-h\nu/kT} + e^{-2h\nu/kT} + e^{-3h\nu/kT} + \cdots ,$$

the fermion equivalent can be similarly expressed as

$$e^{-h\nu/kT} \rightarrow e^{-h\nu/kT} - e^{-h\nu/kT} + e^{-3h\nu/kT} - \cdots .$$

Finally, we may note that the boson condition can in fact be expressed as an exclusion principle, but in time not space: the requirement that over a period of time only one of a set of indistinguishable states can be realised leads to the Bose-Einstein statistics.

Brillouin's analysis

A similar question to that raised above was asked as early as 1927, by L.Brillouin.(BRILLOUIN,L. [1927] Annales de Physique 7 p.315; trans in BROGLIE & BRILLOUIN [1928] as "A comparison of different statistical methods applied to quantum problems"). Brillouin assumed that the particles of a particular type were always identical, but did not get promising results so he changed his assumption and postulated that they were all distinguishable, if not in themselves then by their history. He then investigated wh.t additional assumptions were needed to derive the various statistics. He assumed a total of N particles, of which n_1 had energy E_1 , etc., and G compartments, g_1 corresponding to E_1 etc., in which the particles might be found. Then if each compartment had a capacity of one when empty, and if each particle occupied a volume 'a', he found that

$$\log W = \frac{1}{a}\left[\sum_i g_i \log g_i - G \log G\right] + N \log N + \left(\frac{G}{a} - N\right)\log(G - aN) + \sum_i\left[-n_i \log n_i + \left(n_i - \frac{g_i}{a}\right)\log(g_i - a n_i)\right].$$

Classical particle statistics followed from the assumption $a=0$, Fermi-Dirac statistics from $a=1$, and Bose-Einstein statistics from $a=-1$. The two former cases were easily recognisable (fermions obeying the exclusion principle), but the analysis gave a new condition for bosons: that they should have a mutual attraction, the probability of a state being adopted increasing as the number already in that state. This seems to have been the first expression of the boson characteristic of a tendency towards the same state.

NOTES TO THE INTRODUCTION

1. This is of course an oversimplification of Newton's position, but it is intentional. My aim here is to introduce the range of methodologies and structural visualisations to be found within classical physics, and generally to set the scene for the development of quantum theory. In the interest of brevity and clarity I shall continue to take liberties of this kind throughout the introduction: in particular, Faraday and Lorentz held views far more complex than those that will be attributed to them.

2. They do not share the same ontological status.

3. Newton discusses this in <u>Optics</u> II-3, prop.XII, suggesting that the particles be visualised as being accompanied by a vibration in the medium.

4. It is an interesting feature of this approach, rarely commented upon, that one calculates the effect on a particle if it were there, on the express assumption that it is not.

5. Not of course that they were then known as field theories.

6. What these measurements actually confirmed was that Newton's theory of refraction gave the wrong answer, whereas Hygens' did not. The extension to Newton's theory of light as a whole, which does not lead necessarily to the refractive theory, was quite unjustified albeit understandable.

6a. This is another intentional simplification. In fact the Cartesian fluid theories were extremely persistent, while quantitatively successful theories may be said to commence with Coulomb and Cavendish in the last quarter of the eighteenth century. This simplification is continued through the following two paragraphs, where I wish to indicate the range of possible approaches rather than the precise details of those favoured.

7. Whatever the metaphysics of the optical aether, the theory used to describe it was of Cartesian origin.

8. Faraday dfended this approach explicitly in his paper "On the physical character of the lines of magnetic force", in his <u>Experimental Researches</u> vol.3, p.407; originally in <u>PM</u>, 1852.

9. Mach seems in fact to have made many contributions to science, but none of any <u>fundamental</u> significance. The same may perhaps be said of Kant.

10. He did of course influence Einstein considerably, and'Mach's principle' was a contribution to science. But it was effectively a contribution by Einstein who, as a scientist, was very selective as to what aspects of Mach's philosophy he adopted.

11. Tychism was based on the absence of any possibility in a deterministic physics for growth; Contingency argued that the infinitessimal steps in a causal chain were unobservable, and causality thus inadmissable. These philosophies, due to Peirce and Renouvier respectively, are discussed at greater length in Chapter III.

12. He wondered whether, in another age, science would have been so settled on determinism, or whether it might have embraced contingency: see essay reprinted in Campbell (L) & Garnett (W) 's <u>Life</u>, p.357.

13. See MCCORMMACH [1970a].

14. To realise the immensity of this question, we have only to think of the tendency against causality and objectivity in creative work other than physics: in philosophy, theology and psychology, Kierkegaard, Husserl, Bergson, James, Barth, Bultmann, Buber, the intuitionists, the popular Lebesphilosophie of the Weimar era, etc.; in art, the expressionist and surrealist movements, and the general tendency towards subjectivity; in music, Schoenberg, Strauss, etc.; and in literature, again, a general tendency towards subjectivity. In general, this cultural movement seems to have preceded the scientific one, but its impact can only be taken account of in the limitations of the present study where it can actually be pinpointed.

NOTES TO CHAPTER I

1. See Appendix A for references.
2. See Appendix B for references.
3. My discussion of the background to Einstein's light-quantum hypothesis is partly based on McCORMMACH [1970], KLEIN [1963a,1968] and GOLDBERG [1969].
4. McCORMMACH [1970].
5. EINSTEIN,A. [1901] AP 4 p.513; [1902] AP 8 p.798, 9 p.417;[1903]11 p.170.
6. EINSTEIN,A. [1904] AP 14 p.354.
7. EINSTEIN,A. [1905] AP 17 p.132; trans. AARONS,A.B. & PEPPARD,M.B. [1965] AJP 33 p.367, and TER HAAR [1967] as "On a heuristic viewpoint concerning the production and transformation of light".
8. EINSTEIN,A. [1905] AP 17 p.891; trans. LORENTZ,H.A. et al. [1923] Methuen, The principle of relativity, and KILMISTER,C.W. [1970] Pergamon, Special Theory of Relativity, as "On the electrodynamics of moving bodies".
9. EINSTEIN,A. [1905] AP 17 p.549; trans. [1926] Investigations on the theory of the Brownian movement, as "On the movement of small particles suspended in a stationary liquid, required by the molecular-kinetic theory of heat".
10. EINSTEIN,A. [1905] AP 18 p.639; trans. in refs. 72, as "Does the inertia of a body depend upon its energy content?"
11. Ref.71, Ter Haar p.102.
12. The other effects were the ionization of gases by ultraviolet light, similar to that by X-rays that led Thomson to his quantum hypothesis, and Stokes' law for the change of frequency of light interacting with matter.
13. WIEN,W. [1901] AP 4 p.422, "Zur Theorie der Strahlung schwarzer Körper".
14. BURBURY [1902] PM 3 p.225, "On irreversible processes and Planck's theory in relation thereto".
15. EHRENFEST,P. [1905] WB 114 p.1301, "Uber der physikalischen Voraussetzerungen der Planck'schen Theorie der irreversiblen Strahlungsvorgänge".
16. Ibid. p.1313:
    "Die Hypothese ..., die in der gegenwärtigen Form offenbar nur formal gemeint ist...."
17. LARMOR,J. [1902] Enc. Brit. 11th edn. 32 p.120, "Radiation".
18. LARMOR,J. [1902] BAAS p.546.
19. LORENTZ,H.A. [1903] PASA 5 p.666, "On the emission and absorption by metals of rays of heat of great wavelengths".
20. He found it "certainly a most remarkable conclusion" that the laws should agree in the low frequency limit, considering that one started from the hypothesis of continuous and other from that of discrete energy. The fact that a physicist of Lorentz's calibre should have failed to see that the low frequency limit corresponded to the continuous energy limit of Planck's theroy is strongly indicative of of the lack of physical clarity of Planck's paper; it emphasises in particular that the assumption $\varepsilon=h\nu$ was not made at all clear.
21. JEANS,J.H. [1905] PRS A76 p.545, "On the laws of radiation"; PM 10 p.91, "On the partition of energy between matter and aether"; N 72 p.293, "A comparison between two theories of radiation".

    Also in 1905, Rayleigh looked at Planck's work, but stated that he had not "succeeded in following Planck's reasoning."; RAYLEIGH,J.W. [1905] N 72 p.54.
22. See for example JEANS,J.H. [1901] PTRS 196 p.397; [1902] PM 4 p.585; [1903] PM 5 p.597; 6 pp.279,720; [1904] PM 7 p.467; 8 pp.692,700.
23. JEANS,J.H. [1904] C.U.P., The dynamical theory of gases.
24. RAYLEIGH, J.W. [1905] N 71 p.559.
25. Ref. 21.
26. See the review of this book in N 71, April 27 1905.
27. LENARD,P. [1902] AP 8 p.149, "Uber die lichtelektische Wirkung". See also [1899] WB 108 p.1649, [1900] AP 2 p.359.
28. Ibid. [1902].
29. WIEN,W. [1907] GN p.598, "Uber eine Berechnung der Wellenlänge der Röntgenstrahlen aus dem Planckschen Energie-Element"; AP 22 p.180, "Uber die absolute, von positiven Ionen ausgestrahlte Energie und die Entropie der Spektrallinien".

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30. App. B, ref.17.

31. CAMPBELL,N.R. [1910] PCPS 15 p.310, "Discontinuities in light emission."

32. TAYLOR,G.I. [1910] PCPS 15 p.114, "Interference fringes with feeble light!

33. EINSTEIN,A.[1907] AP 22 p.180, "Die Plancksche Theorie der Strahlung und die Theorie der spezifischen Wärme".

34. See appendix J. The law was applied to the specific heats problem in its resonator form.

35. NERNST,W. & LINDEMANN,F.A. [1911] ZE 17 p.817, "Spezifische Wärme und Quantentheorie".

36. LANGEVIN,P. & BROGLIE,M.de (ed.) [1912] Gauthiers-Villars, La théorie du rayonnement et les quanta: Rapports et discussions de le réunion tenue à Bruxelles, du 30 octobre au 3 novembre 1911 sous les auspices de M. E.Solvay.

37. LORENTZ,H.A. [1908] NC 16 p.5, "La partage de l'énergie entre la matière pondérable et l'ether".

38. LORENTZ,H.A. [1910] PZ 11 p.349, "Die Hypothese der Lichtquanten".

39. LORENTZ,H.A. [1910] PZ 11 p.1234, "Alte und neue Fragen der Physik, 6 Vertrag." See appendix J.

40. LARMOR,J. [1909] PRS A83 p.82, "The statistical and thermodynamical relations of radiant heat".

41. See appendix J.

42. JEANS,J.H. [1909] PM 17 p.229, "Temperature radiation and the partition of energy in continuous media."

43. JEANS,J. [1910] PM 20 p.943, "Non-Newtonian mechanical systems and Planck's theory of radiation".

I shall discuss Jeans' work in more detail in the next section. The quantisation of the aether did not strictly imply a localisation of the radiant energy quanta, but it did imply discrete and hence localised (though not necessarily instantaneous) absorption.

44. EINSTEIN,A. [1909] PZ 10 p.185, "Zum gegen wörtigen Stand des Strahlungsproblems".

45. EINSTEIN,A. [1909] PZ 10 p.817, "Uber die Entwicklung unserer Anschauungen uber das Wesen und die Konstitution der Strahlung". This includes both Einstein's conference paper and the ensuing discussion.

46. Ibid., discussion.

47. Ibid., discussion.

48. Ref.45, p.286:

> "Die Interferenz scheinungen würden wohl nicht so schwierig einzureihen sein als man sich vorstellt, und zwar aus folgendem Grunde: man darft nicht annehmen, dass die Strahlungen bestehen aus Quanten, die nicht in Wechselwirkung stehen; dass würde unmöglich sein fur die Erklärung der Interferenzsceinungen. Ich denke mir ein Quantum als ein Singularität umgeben von einem grossen Vektorfeld. Durch eine grosse Zahl von Quanten lässt sich ein Vektorfeld zusammensetzen, das sich wenig von einem solchen unterscheidet, wie wir es bei Strahlungen annehmen."

49. Ibid.

50. Ibid.

51. Einstein to Lorentz, 23-5-09, quoted in translation by STEUWER [1975].

52. Ref.45, p.825:

> "Das scheint mir ein Schritt, der in meiner Auffassung noch nicht als notwendig geboten ist."

53. Ref. 39, p.1250:

> "Redner möchte den heuristischen Wert dieser Hypothese zwar nicht bestreiten, aber die alte Theorie so lang als möglich verteidigen."

54. STARK,J. [1909] PZ 10 p.585, "Uber Röntgenstrahlen und die atomische Konstitution der Strahlung"; p.902, "Zur experimentallen Entscheidung zwischen Ätherwellen und Lichtquantenhypothese, I Röntgenstrahlung".

55. See STEUWER [1975] for discussion.

56. BARKLA,C.G. [1904] PM 7 p.453, "Energy of secondary Röntgen radiation".

57. BARKLA,C.G. [1905] PTRS 204 p.467, [1906] PRS A77 p.247, PM 11 p.812.

58. BRAGG,W.H. [1907] PM 14 p.429, "On the properties and nature of various electric radiations".

59. Ref.30.

60. E.g. BARKLA,C.G. [1907] N 76 p.661, [1908] N 77 p.319, PM 15 p.288; BRAGG,W.H. [1908] N 77 pp.270,560. The debate continued in N 78, and sporadically thereafter.

61. COOKSEY,C.D. [1908] N 77 p.509, "The nature of γ- and X-rays". KLEEMAN,R.D. [1908] PM 15 p.638, "On the different kinds of γ-rays of radium, and the secondary γ-rays which these produce".

62. See THOMSON,J.J. [1908] PCPS 14 p.540, "The nature of the γ-rays".

63. Refs.58,60.

64. Ref. 45., discussion.

65. SOMMERFELD,A. [1911] S.ts.Mü.41 p.1, "Uber die Struktur der γ-strahlen".

66. Bragg to Sommerfeld, 7-2-11, 17-5-11, 7-7-11, SHQP.

67. This debate is discussed at greater length by STEUWER [1975], whose translations of Sommerfeld's comments I have used.

68. SOMMERFELD,A. [1909] PZ 10 p.969, "Uber die Verteilung der Intensität bei der Emission von Röntgenstrahlung".

69. DEBYE,P. [1910] AP 33 p.1427, "Der Wahrscheinlichkeitsbegriff in der Theorie der Strahlung".

70. HAAS,A.E. [1910] WB 119 p.119, "Uber die elektrodynamik Bedeutung des Planck'schen Strahlungsgesetzes und über einer neue Bestimmung des elektrischen Elementarquantums und der Dimensionen des Wasserstoffatoms"; PZ 11 p.557; JdR 7 p.261.

71. See HERMANN [1969] for a discussion of this.

72. Refs. 36, 38.

73. Ref. 36.

74. Ibid.

75. Ibid., p.428:
"La conception que l'énergie rayonnante est constituée par des quanta localisés de grandeur hν conduit à cette fluctuation; mais cette manière de voir semble tout à fait inconciliable avec les phénomènes de diffraction et d'interférence."

76. Ibid., p.429:
"On doit se résoudre à abandonner la loi de la conservation de l'énergie sous sa forme actuelle, par example en lui attribuant seulement une validité d'ordre statistique, comme on le fait déja pour le second principe de la thermodynamique."

77. Ibid. (footnote to above):
"Dans ce cas je ne vois pas qu'on puisse introduire l'hypothèse d'une accumulation et l'on ne peut choisir qu'entre la structure du rayonnement et la négation d'une validité absolue de la loi de la conservation de l'énergie."

78. Ibid., p.443:
"Il nous faut introduire d'une manière quelconque une hypothèse comme celle des quanta à côté des indispensables équations de Maxwell."

79. Ibid., p.436:
"En premier lieu nous serons d'accord pour conserver le principe de l'énergie."

80. Ibid., p.70:
"Il ne semble donc pas douteux qu'aucune extension de la théorie de Maxwell-Boltzmann dans le sens qui vient d'être indiqué ne pourra rendre compte des phénomènes de rayonnement. ... Mais il est peu probable que la théorie classique de Boltzmann et de Maxwell, combinée avec une hypothèse quelconque sur le mécanisme de radiation dans lequelle les équations canoniques seraient conservées, puisse jamais conduire à des formules représentant aussi bien les faits que celles de Planck, Nernst, et Einstein."

81. Rayleigh wrote a letter to the Congress (he could not be present) in support of Jeans' view. Reproduced Ibid.

82. Ref. 43.

83. This criticism was also made by Langevin, but he also objected to Einstein's theory on the grounds that it ignore the matter responsible for the creation of light.

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84. Ref. 36, p.451:

"Ce qui m'a frappé dans les discussions que nous venons d'entendre, c'est de voir une même théorie s'appuyer tantôt sur les principes de l'ancienne mécanique et tantôt sur les nouvelles hypothèses qui en sont la négation; on ne doit pas oublier qu'il n'est pas de proposition qu'on ne puisse aisement démontrer, pour peu que l'on fasse entrer dans la démonstration deux premises contradictoires."

85. Ibid.

86. POINCARÉ,H. [1912] "L'hypothèse des quanta." in [1913] Flammarion, Paris Dernières pensées. His formal discussions of the same year are treated below.

87. Ibid., p.186.

88. In order to clear up any misapprehensions, we must stress that Poncaré was not contemplating any change in the status of physics, or in its underlying philosophy. Jammer [1966] writes, on p.171, that

"It is clear that Poincaré's question whether differential equations are still the appropriate instrument for the mathematical formulation of physical laws was but a mathematician's way of expressing his doubts in the validity of the causal principle.",

but this is nonsense. Poincaré was aware that determinism was an assumption, and he had in the past discussed it as such, but he never considered abandoning it, and he certainly did not take discontinuity, even of time, to imply its renunciation. In fact he found this discontinuity "not contrary to [Planck's] thought: he would never have attributed anticausal ideas to Planck. Whilst advocating discontinuity, Poincaré was fully aware that this was only a provisional model, suitable for the particular stage in which physics found itself, and that in terms of basic physical reality one would never be able to distinguish between conti uity and discontinuity:

"Will discontinuity reign over the physical universe and is its triumph definitive? or will we rather recognise that this discontinuity is onle apparent and conceals a series of continuous processes. ... To seek to give an opinion on these questions at this stage would be a waste of ink."

("La discontinuité va-t-elle régner sur l'univers physique et son triomphe est-il definitif? ou bien reconnaîtrait-on que cette discontinuité n'est qu'apparente et dissimile une série de processus continus. ... Chercher dès aujourd'hui à donner un avis sur ces questions, ce serait perdre son encre.", from Ref. 42, p.192.)

There is no reason to think that Poincaré had changed his mind on the subject of causality since he discuseed it extensively in 1904, in La Valeur de Science (Flammarion, Paris). His view then was that where the laws of chance did play an apparent role it was only in aggregating microscopic causal results (circa p.110). With respect to this discussion Jammer draws the conclusion that Poincaré was prepared to consider the role of chance: but Jammer does not realise that Poincaré defined chance as an "assemblage of complex causes".

89. He had of course commented upon Einstein's Salzburg paper.

90. Ref. 36 ; PLANCK,M. [1911] VDPG 13 p.138, "Eine neue Strahlungshypothese."; [1912] AP 37 p.642, "Über die begründung des Gesetzes der schwarzen Strahlung."

91. He had by this time accepted the molecular hypothesis for matter, but unwillingly.

92. Ref. 36 , p.101:

"Il va sans dire que telles hypothèses sont inconciliables avec les équations de Maxwell. ... Quand on songe à la confirmation expérimentale complète qu'a reçue l'électrodynamique de Maxwell par les phénomènes des interférence les plus délicats, .. , on éprouve quelque répugnance à en ruiner de prime abord les fondaments."

93. Ibid., p.109-10:

"comme dans tous les cas où l'énergie d'un oscillateur isolé est supposée varier de manière discontinue, il est impossible de comprendre d'où vient l'énergie absorbée par un oscillateur lorsque, ... , son énergie augmente brusquement de 0 à $h\nu$. ... le phénomène d'absorption du rayonnement libre est essentiellement continu."

94. Ibid., p.99:

"L'hypothèse des quantités élémentaires d'action"

95. See below.

96. Ibid., p.118:
"En réalité ... la probabilité est nulle partout, sauf en certain points isolés; elle est la même en ces divers points isolés est il y a un seul point dans chaque domaine."

97. Ibid., p.113:
"Avant tout, il est nécessaire d'insister sur ce fait que l'hypothèse des quanta n'est pas à proprement parler une hypothèse d'énergie, mais pourrait plutôt s'appeler une hypothèse d'action."
Among other things, Planck hoped, by restricting quantisation to action, to make the theory compatible with a form of classical physics based on a generalisation of the principle of least action.

98. Ibid., p.313; SOMMERFELD,A. [1911] PZ 12 p.1057, "Das Plancksche Wirkungsquantum und seine allgemeine Bedeutung für die Molekularphysik."

99. Ibid. (Ch.I, ref. 66) p.314:
"Les propriétés générales de toutes les molécules ou atomes qui déterminesnt les phénomènes de rayonnement ne consistent pas dans l'intervention d'éléments particuliers d'énergie, mais en ceci, que la manière dont se produisent les échanges d'énergie dans un temps plus ou moins long est dominée par une loi universelle."

100. Ibid., p.316:
"Dans tout phénomène moéculaire pur, l'atome prend ou perd une quantité d'action déterminée de manière universelle et de grandeur ....."

101. SOMMERFELD,A. & DEBYE,P. [1913] AP 41 p.873, "Theorie des lichtelektriscen Effektes vom Standpunkt des Wirkungsquantums."

102. See appendix C..., for details of Planck's hypotheses.

103. As did Languevin, Planck, and, after defending it as best he could, Lorentz.

104. Ref. 36 , p.124:
"Quant à moi, je préfère une hypothèse générale sur h à des modèles particuliers d'atomes."

105. Ibid., p.367ff.

106. Ibid., p.129.

107. See STEUWER [1975] for discussion.

108. EHRENFEST,P. [1911] AP 36 p.91, "Welche Züge der Lichtquantenhypothese spielen in der Theorie der Wärmestrahlung eine wesentliche Rolle?"

109. POINCARÉ,H. [1912] JP 2 p.5, "Sur la théorie des quanta."; CR 153 p.1103.

110. Ref. 43, p.943-4.

111. Ibid.

112. Ibid. This assumption has been challenged recently by the supporters of a neo-classical radiation theory, and with some justification. Certainly it can only be taken as an approximation.

113. Ibid. p.953.

114. Ibid.)114a No likeness to rat psychology is intended! But "phenomenalist" in this context carries too many connotations.

115. Ref. 108 p.113:
"Man kann zeigen: Die Annahme (β) führt nicht zur Planckschen Strahlungsformel, sondern zu eine einfach unendlichen Schar von anderen Strahlungsformeln, wobei dann die Heraushebung einer bestimmten Formel dieser Schar durch eine Zusetz-forderung geschieht."

116. See appendix D.

117. Ref. 36 , p.453:
"L'hypothèse des quanta parait être la seule qui conduise à la loi expérimentale du rayonnement, si l'on admet la formule habituellement adoptée pour la relation entr l'énergie des résonnateurs et celle de L'éther, et si l'on suppose que des échanges d'énergie puissent se faire entre les résonnateurs par le choc mécanique des atoms ou des électrons."

118. See ref. 112.

119. BATEMAN,H. [1913] PM 26 p.579, "Corpuscular radiation."

120. Ibid., p.579.

121. Ibid.

122. Ibid.

123. Bateman did construct, in terms of a "gun" and "bullets", a picture of field with apparently chargeless singularities, but it could not be related to any physically recognisable situation.

124. CAMPBELL,N.R. [1913] C.U.P. Modern electrical theory , 2nd edn.

125. Ibid., p.303.

126. Newton is remembered as an advocate of corpuscular light, but his corpuscles were in fits:

> "Every ray of light in its passage through any refracting Surface is put into a certain transient Constitution or State, which in the progress of the Ray returns at equal Intervals, and disposes the Ray at every return to be easily transmitted through the next refracting Surface, and between the returns to be easily reflected by it. ...
>
> The returns of the disposition of any Ray to be reflected I will call its Fits of easy Reflexion, and those of its disposition to be transmitted its Fits of easy Transmission." (Opticks, 1704 edn., 1730 printing, Bk.II,pt.III,prop.XII.)

"What kind of action or disposition this is ...", wrote Newton, "I do not here enquire"; but this did not stop him suggesting that one could visualise the ray as being accompanied by some kind of vibration in the medium. Concerning the structure of light, we must conclude that Newton held it to be composed of particles, but with wave properties as well. This is close to Campbell's conception, and to that later adopted in quantum mechanics.

127. Reported in N 90 p.423, "Atomic heat of solids", [1912].

128. British Association for the Advancement of Science, [1913] Transactions A p.378.

129. Ibid.

130. Ibid., p.376.

131. Ibid., p.381.

132. Ibid., p.380.

133. JEANS, J.H. [1914] 'The Electrician' print.&pub. co., Report on radiation and the quantum theory.

134. Ibid.

135. RICHARDSON,O.W. [1916] C.U.P. The electron theory of matter , 2nd edn.

136. Ibid., p.507.

137. Ref. 124, p.253.

138. LAUE,M. [1912] Sitz. München p.303, "Interferenzerscheinungen bei Röntgenstrahlen."; p.363, "Ein quantitativ Prüfung der Theorie die Interferenzerscheinungen bei Röntgenstrahlen."; trans in BACON,G.E. [1966] Pergamon, X-ray and neutron diffraction, p.89. This is discussed by FORMAN [1969]

139. RICHARDSON,O.W. & COMPTON,K.T. [1912] PM 24 p.575, "The photoelectric effect."

140. BRAGG,W.H. [1913] PRS A88 p.436, "The reflection of X-rays by crystals."

141. BRAGG,w.h. [1912] N 90 p.360, "X-rays and crystals."

142. Ref. 140.

143. Ref. 128, p.380.

144. MILLIKAN, R.A. [1913] S 37 p.119, "Atomic theories of radiation.", p.133. This was before his famous series of experiments on the effect.

145. NATANSON,L. [1912] PZ 12 p.659, "Uber die statistische Theorie der Strahlung.";

146. KRUTKOW,G. [1914] PZ 15 p.133, "Aus der Annahme unabhängigerlichtquanten folgt die Wiensche Strahlungsformel."; see also p.363 and WOLFKE,M: pp.308,463 for argument as to validity of Krukow's derivation.

147. EHRENFEST,P. & KAMERLINGH-ONNES,H. [1915] PM 29 p.297, "Simplified deduction of the formula for the theory of combinations which Planck uses as a basis of his radiation theory."; AP 46 p.1021; [1914] VAA 23 p.789; PASA 17 p.870.

148. The resonators, or containers, were distinguished by their position in the ordered array.

149. PLANCK,M. [1914] BB p.918, "Eine veränderte Formulierung der Quanten-hypothese."
The attempt was unsuccessful, as were those of DUANE,W. [1913] PR 7 p.143 and GIBSON,G.E. [1912] VDPG 16 p.104; the attempt by Sommerfeld and Debye to base a neo-classical derivation of the law upon equipartition for the joint matter-aether system also failed. Other unconvincing attempts to derive Planck's law came from:
WEINSTEIN,M.B. [1916] AP 49 p.363; BAUER,E. [1911] CR 153 p.1466; GREEN,J.B. [1916] PM 32 p.229; BENEDICKS,C. [1913] CR 156 p.1526; see also WEREIDE,T. [1916] PZ 17 p.104; AP 49 p.915.

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150. MARX,E. & LICHTENEKER,K. [1913] AP 41 p.124, "Experimentalle Untersuchung des Einflusses der Unterteilung der Belichtungszeit auf die Elektronenabgabe in Elster und Geitelschen Kaliumhydrürzellen bei sehr schwacher Lichtenergie."

151. WILSON,W. [1916] PM 31 p.156, "The quantum of action."

152. WILSON,W. [1915] PM 29 p.795, "The quantum of radiation and line spectra."

153. ISHIWARA,J. [1915] Tokyo Sugaki-Buturigakkwai Kuzi 2nd ser. 8 no.4 p.106.

154. Ref.152, p.796.

155. Ref.151.

156. GOLDBERG [1970].

157. BOHR,N. [1913] PM 26 pp.1,476,502, "On the constitution of atoms and molecules."; rep. in TER HAAR [1967].

158. NICHOLSON,J.W. [1912] MNRAS 72 p.49, "The spectrum of Nebulium."; pp.139, 677,729, "The constitution of the solar corona."
See McCORMMACH [1960].

158a. External factors may also have played a part in Bohr's innovation, for there is a passage in William JAMES [1890] Principles of psychology that describes thought processes in a way strikingly similar to Bohr's concept of the atom. See p.244 of the principles, and MOORE [1966].

159. SOMMERFELD,A. [1915] MB p.425, "Zur Theorie der Balmerschen Serie."; p.459, "Die Feinstruktur der Wasserstoff- und Wasserstoffähnlichen Linien."; [1916] AP 51 pp.1,125, "Zur Quantentheorie der Spektrallinien."

160. SOMMERFELD,A. [1916] PZ 17 p.491, "Zur Theorie des Zeemans-Effekts der Wasserstofflinien, mit einem Anhang uber den Stark Effekt."

DEBYE,P. [1916] GN p.142, "Quantenhypothese und Zeeman-Effekt."; PZ 17 p.507.

161. SCHWARZCHILD,K. [1916] BB p.548, "Zur Quantenhypothese."
EPSTEIN,P.S. [1916] PZ 17 p.148, AP 50 p.489, "Zur Theorie des Starkeffekts."; AP 51 p.168, "Zur Quantentheorie."

162. EHRENFEST,P. [1913] VGVWNAA 22 p.886, "Een mechanische theorema van Boltzmann en zijne betrekking tot de quanta theorie."; [1914] PASA 16 p.591, "A mechanical theorem of Boltzmann and its relation to the theory of energ quanta." ; [1916] AP 51 p.327, "Adiabatische Invarianten und Quantentheorie."; [1917] PM 33 p.500, "Adiabatic invariants and the theory of quanta.", rep. in WAERDEN [1967].

163. Ref. 161.

164. Ref.162 (WAERDEN),p.80.

165. BOHR,N. [1918-22] KDVSSN ser.8,IV,1 p.1, "On the quantum theory of line-spectra"; pt.I rep. WAERDEN [1967]; trans. [1923] Vieweg , Uber die Quantentheorie der Linienspektren."

The terminology was introduced in BOHR,N. [1920] ZP 2 p.423, "Uber die Linienspektren der Elemente.", trans. in [1922] C.U.P., The theory of spectra and atomic constitution.

166. Ibid . (WAERDEN) , p.109.

167. EINSTEIN,A. [1916] MPGZ 18; [1917] PZ 18 p.121, "Zur Quantentheorie der Strahlung" ; trans in WAERDEN [1967].

168. Ibid., (WAERDEN) p.75.

169. Ibid., p.66.

170. Ibid., p.76.

171. Ibid., p.78.

172. MILLIKAN,R.A. [1916] PR 7 p.355, "A direct photoelectric determination of Planck's h."; see also PZ 17 p.217, "Quantenbeziehungen beim photelektrischen Effekt."

173. MILLIKAN,R.A. [1917] Chicago, The electron, p.230.

174. BARKLA,C.G. [1916] PRS A92 p.504, "On X-rays and the theory of radiation."

175. SCHOTT,G.A. [1918] PM 36 p.243, "On Bohr's hypothesis of stationary states of motion and the radiation from an accelerated electron."

176. BICHOWSKY,F.R.v. [1918] PR 11 p.58.

177. FLAUMM,L. [1918] PZ 19 pp.116,166.

178. HOUSTON,W.V. [1919] N 103 p.145, "Ionisation and radiation."

179. Ref. , p.235.

180. Ibid., p.238.

181. Ibid., p.229-30.

182. HEILBRON & KUHN [1970].

183. Heisenberg interview, SHQP.

184. See JAMMER [1966], HEILBRON [1967], FORMAN [1970].

NOTES TO CHAPTER II

1. I shall in fact draw upon secondary sources for the technical development of the old quantum theory, but not for its conceptual implications. I shall also have occasion to discuss secondary sources with which I disagree profoundly, most notably FORMAN [1971], but I shall confine my detailed criticisms to notes and appendices.
2. WEYL,H. [1918] SPAW 26 p.465.
3. PAULI,W. [1919] PZ 20 p.457.
4. Einstein to Born, 27.1.20, BEC.
5. See below.
6. WEYL,H. [1921] AP 65 p.541.
7. EDDINGTON,A.S. [1921] PM 42 p.800; PRS A99 p.104.
8. BUCHERER,A.H. [1922] AP 68 pp.1,546, "Gravitation und Quantentheorie."

   See also SCHRÖDINGER,E. [1922] ZP 12 p.13, "Uber eine Bemerkenswerte Eigenschaft der Quantenbahnen eines einzelnen Elektrons."
   WEREIDE, T. [1923] PR 21 p.391, "The general principle of relativity applied to the Rutherford-Bohr atom-model."
9. EMDEN,R. [1921] PZ 22 p.513, "Uber Lichtquanten."
10. SCHRÖDINGER,E. [1922] PZ 23 p.301, "Dopplerprinzip und Bohrsche Frequenzbedingung."
11. Institut International de Physique Solvay [1921] Gauthier-Villars, Paris: Atomes et électrons : Rapports et discussions du troisième Conseil de Physique tenu à Bruxelles du 1 au 6 Avril 1921.
12. BROGLIE,L.de. [1921] CR 173 p.1160, "Sur la dégradation du quantum dans les transformations successives des radiations de haute fréquence."
13. Ref. 11; MILLIKAN,R.A. [1921] PR 18 p.236, "The distinction between intrinsic and spurious contact e.m.f.s and the question of the absorption of radiation by metals in quanta."
14. MILLIKAN,R.A. [1924] Chicago The electron, 2nd. edn.
15. Ref 13 (PR) p.244.
16. KLEIN,C. & ROSSELAND,S. [1921] ZP 4 p.46, "Uber Zusammenstosse zwischen Atomen und freien Elektronen."
17. Ch. I,ref. 165.
18. BOHR,N. [1920] ZP 2 p.423, "Uber die Serienspektren der Elemente."; trans. in [1922] C.U.P., The theory of spectra and atomic constitution
19. Bohr could not actually be present at the Congress; his paper was read by Ehrenfest.
20. Ref. 11; The original English draft is in BOHR Collected works v.3 p.361, "On the application of the quantum theory to atomic problems."
21. App. E, ref. 19, p.35.
22. Bohr to Darwin, July 1919 SHQP.
23. Darwin to Bohr, 20.7.19 SHQP
24. DARWIN,C.G. [1923] N 111 p.771, "The wave theory and the quantum theory."
25. SILBERSTEIN,L. [1920] Adam Hilge, London, Report on the quantum theory of spectra.
26. Ref. 24.
27. COMPTON,A.H. [1922] BNRC 4 pt.2, 20, "Secondary radiations produced by X-rays, and some of their applications to physical problems."; [1923] PR 21 p.715, "Wave-length measurements of scattered X-rays."; p.483, "A quantum theory of the scattering of X-rays by light elements."
28. STEUWER [1975] .
29. COMPTON,A.H. [1924] JFI 198, p.61, "The scattering of X-rays." :p.69
30. COMPTON,A.H. [1924] PR 23, p.440, "The recoil of electrons from scattered X-rays.": p.449. Also in the original paper, ref.27 (PR), he wrote that

    "The experimental support of the theory indicates very convincingly that a radiation quantum carries with it a directed momentum as well as energy."
31. Alternative theories were sought by Thomson, Jeans, Barkla, C.T.R.Wilson, Darwin, Bauer, Forsterling and Halpern. See STEUWER [1975].
32. COMPTON,A.H. [1923] PM 45 p.1121, "The total reflexion of X-rays."
33. Ibid., p.1130.

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34. See STEUWER [1975] and JAMMER [1966]. Duane's objection was to Compton's results rather than to his theory, and he himself helped to extend the applicability of the light-quantum concept to crystal diffraction: DUANE, W. [1923] PNAS 9 p.158, "The transfer in quanta of radiation momentum to matter; COMPTON,A.H. [1923] PNAS 9 p.359, "The quantum integral and diffraction by a crystal." By January 1924, everybody was apparently in agreement that Compton's results were correct: Kemble to Bohr, 4.1.24, SHQP.

35. Sommerfeld to Bohr, 18.5.18 SHQP:
"Der Wellenvorgang liegt allein im Äther, der den Maxwell'schen Gleichungen gehorcht und quantentheoretisch wie ein linear Oscillator wirkt, mit unbestimmter Eigenfrequenz ν. Das Atom liefert zu dem Wellenvorgang nur eine bestimmte Menge Energie und Impulsmoment als Material für den Wellenvorgang."

36. Sommerfeld to Bohr, 21.1.23 SHQP, trans. STEUWER.

37. Sommerfeld to Compton 9.10.23 SHQP, trans. STEUWER.
Compton wrote (ref.29) that
"In a recent letter to me Sommerfeld has expressed the opinion that the discovery of the change of wave-length of radiation, due to scattering, sounds the death-knell for the wave theory of radiation."
It is not clear whether he noted, or intended, the irony.

38. HEISENBERG,W. [1921] ZP 8 p.273, "Zur Quantentheorie der Linienstruktur und der anomalen Zeemaneffekte.": p.281:
"Damit stellen wir uns in bemussten Gegensatz zur klassische Ausstrahlung."

39. Heisenberg to Landé, 28.11.21 SHQP.

40. Sommerfeld to Einstein 11.1.22 ESB:
"Ich kann nur die Technik der Quanten fordern, Sie müssen Ihre Philosophie machen. Innerlich glaube ich auch nicht mehr an die Krugelwelle (In der annomalen Zeemaneffekten steht ubrigens auch eine Portion aufgeben der Undulationstheorie). Setzen Sie ihr nur ordentlich zu!"

41. DEBYE,P. [1923] PZ 24 p.165, "Zerstreuung von Rontgenstrahlen und Quantentheorie."; trans. in DEBYE Collected papers.

42. PAULI,W. [1923] ZP 18 p.272, "Uber das thermische Gleichgewicht zwischen Strahlung und freien Elektronen."; see also EINSTEIN,A. & EHRENFEST,P. [1923] ZP 19 p.301, "Zur Quantentheorie des Strahlungsgleichgewichts."

43. PLANCK,M. [1921] ActM 38 p.387, "Henri Poincaré und die Quantentheorie." This was refuted by FOWLER,R.H. [1921] PRS A99 p.462, "A simple extension of Fourier's integral theorem and some physical applications, in particular to the theory of quanta.".

44. KRAMERS,H.A. & HOLST,H. [1922] Knopf, NY, The atom and the Bohr theory of its strucure. As well as being a good account of the Bohr theory, this book is also excellent on the historical background to it; Holst essentially wrote the historical part, and Kramers the contemporary one.

45. ADAMS,E.P. [1923] BNRCW November "The quantum theory."

46. JORDAN, P. [1924] ZP 30 p.297, "Zur Theorie der Quantenstrahlung."

47. EINSTEIN,A. [1921] BB p.882, "Uber ein den Elementarprozess der Lichtemission betreffendes Experiment." ; Einstein to Ehrenfest, 11.1.22. SHQP.

48. Ehrenfest to Einstein, 17.1.22, 19.1.22, 26.1.22; Einstein to Ehrenfest 22.1.22, 30.1.22. Einstein's last letter admitted defeat, as did EINSTEIN,A. [1922] BB p.18, "Zur Theorie der Lichtfortpflanzung in dispergierenden Medien." See KLEIN [1970].

49. Interview SHQP [1962-3] We should note that, after Heisenberg had made this remark, the interviewer T.Kuhn took over and reconstructed the state of affairs himself, allowing only the occasional "yes" from Heisenberg. The reconstruction does seem to be accurate however.

50. BRAGG,W.H. [1921] N 107 p.79, "Electrons."

51. Ref. 9, p.513:
"Der Erscheinungen der Optik können bekanntlich nicht mehr auf eine Grundanschauung zurückgeführt werden; die einst allmächtige Undulationstheorie versagt auf weiten Gebieten, die durch Annahme von Lichtquanten einfach und restlos behandelt werden können. Trotzdem werden im Banne der Undulationstheorie die Lichtquanten gleichsam nur als Verlegenheitshypothese betrachtet, bestimmt früher oder

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wieder zu verschwinden. Der Zweispalt, der sich durch der Gegensatz beider Anschauungen ergibt, kann meistens vermieden werden, wenn man sich jewals konsequent auf den Boden dejenigen Theorie stellt, durch welche das betreffende Erscheinungsgebiet behandelt werden kann."

52. Ref. 20, p.374.

53. Einstein to Born, 4.6.19 BEC.

54. EINSTEIN,A. [1924] Berlinner Tagblatt. I, "Das Komptonsche Experiment."

55. LORENTZ,H.A. [1923] "L'ancienne et la nouvell Mécanique." (10.12.23), in LORENTZ Collected papers v.7 p.274: p.285:
"Toute cela est d'une grande beauté et d'une extrême importance, mais malheuresment nous ne le comprenons pas."

56. SOMMERFELD,A. [1924] Vieweg, Atombau und Spektrallinien 4th edn.; 5th edn. trans [1934] Methuen, Atomic structure and spectral lines.

57. Born to Einstein, 21.10.21 BEC.

58. The dual pictures in general have not as yet been studied. De Broglie's work has been the subject of several studies, including GERBER [1969] and KUBLI [1970], but none of these have treated satisfactorily the development of de Broglie's conceptions.

59. Ref. 4. This view is repeated in Einstein to Born, 3.3.20 BEC.

60. For Faraday's conceptions see ch.II, ref.125.

61. LORENTZ,H.A. [1923] Lecture given in California, published in [1927] Ginn, [1967] Dover, Problems of modern physics: p.156.

62. SCHOTTKY,W. [1921] dN 9 pp.492,506, "Das Kausalproblem der Quantentheorie als eine Grundfrage der modernen Naturforschung überhaupt - Versuch einer gemeinverständlichen Darstellung."

63. According to Schottky, ibid.

64. Ref. 12.

65. Louis de Broglie was in fact 29 in 1921, but he had spent six years in military service.

66. Interview SHQP [1963] De Broglie was also an admirer of Bergson, but I can find no influence of this upon his work.

67. The 'family business' since the eighteenth century had been a mixture of politics, espionage and arms dealing.

68. By "de Broglie", we shall henceforth mean Louis.

69. Ch.I, ref. 7.

70. Ref. 12.

71. BROGLIE,L.de. [1922] JP 3 p.422, "Rayonnement noir et quanta de lumière" trans. in BROGLIE,L.de. & BRILLOUIN,L. [1928] Blackie, Selected papers on wave mechanics as "Black radiation and light quanta."

72. BROGLIE,L.de. [1923] CR 177 p.507, "Ondes et quanta" (10.9.23); p.548, "Quanta de lumière" (24.9.23); p.630, "Les quanta, la théorie cinétique des gaz et le principe de Fermat."(8.10.23); [1924] PM 47 p.446 (Feb.), "A tentative theory of light quanta." (rec. 1.10.23).

73. BROGLIE,L.de. [1922] CR 175 p.811, "Sur les interférences et la théorie des quanta de lumière."

74. Ibid.:
"Il foudra sans doute faire un compromis entre l'ancienne théorie et la nouvelle en introduisant dans celle-ci la notion de periodicité."

75. Ref. 72.

76. BROGLIE,L.de. [1924] rep.[1963] Masson, Paris, Thèse - Recherches sur la théorie des quanta.

77. Ibid., p.22:
"Il y a là une difficulté qui m'a longtemps intrigué."

78. Ibid., p.76:
"Cette idée jointe à celle de correspondance nous conduit à penser que la probabilité des réactions entres atomes de matière et atomes de lumière est en chaque point liée à la résultante (ou plutôt à la valeur moyenne de celle-ci) d'un des vecteurs caratérisant l'onde de phase; là où cette résultante est nulle la lumière est indécelable; il y a interférence. On conçoit donc qu'un atome de lumière traversant une région ou les ondes de phase

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interfèrent pourra être absorbé par la matière en certains points et en d'autres ne le pourra pas."

79. Ref. 72 (PM), p.452. He changed to Einstein's guiding wave interpretation of interference in BROGLIE,L.de. [1924] CR 179 p.1309, "Sur la dynamique du quantum de lumière et les interférences."

80. Ref. 72 (PM), p.452.

81. Ibid., p.452.

82. Ibid., p.449.

83. Ibid., p.451.

84. Ref. 76, p.9 :
"L'histoire des théories optiques montre que la pensée scientifique a longtemps hésité entre une conception dynamique et une conception ondulatoire de la lumière; ces deux représentations sont donc sans doute moins en opposition qu'on ne l'avait supposé et la développement de la théorie des quanta semble confirmer cette conclusion."

85. HAMILTON,W.R. [1931] rep. [1967] C.U.P. Mathematical Papers v.3 , p.874:
"It is my hope and purpose to remodel the whole of Dynamics, in the most extensive sense of the word [i.e. including optics], by the idea of my characteristic function or central law of relation. ... I am not now offering to the Royal Society so great a work as that would need to be, in which dynamics and optics should be treated as corollaries of one common principle. I content myself as yet with offering one science to Ireland, and another to England, and holding out, along wiht each, a hope of their future union, in some unimagined consummation." Hamilton to his Uncle James, 12.3.1834.

86. Ref. 76, p.69:
"Notre dynamique (y compris sa forme Einsteinienne) est restée en retard sur l'optique: elle en est encore au stade de L'Optique Géométrique. S'il nous paraît aujourd'hui assez probable que toute onde comporte des concentrations d'énergie, par contre la dynamique du point matériel dissimile sans doute une propagation d'ondes et le vrai sens du principe de moindre action est d'exprime une concordance de phase."

87. Ref. 79 (CR):
"Toute la théorie ne deviendra vraiment claire que si l'un parvient à définer la structure de L'onde lumineux et la nature de la singularité constituée par le quantum dont le mouvement devrait pouvoir être prévue en se plaçant uniquement au point de vue ondulatoire."

88. Ref. 66.

89. Bohr's ideas have been analysed by several writers, most notably MEYER-ABICH [1965], but none are clear on this topic. The causality issue has been discussed by JAMMER [1966] and FORMAN [1971] but I disagree profoundly with both these authors. The remaining ideas treated in this section have not been satisfactorily studied.

90. Darwin, manuscript, SHQP.

91. Ref. 23. Unfortunately the structure of the correspondence is not clear Darwin's note opened "Your letter gave me courage to write my general views on quanta", but the only letter extant from Bohr to Darwin at this time seems to be in reply to the note. However, it is clear from the material available that Darwin was familiar with Bohr's views before writing to him.

92. Ref. 90.

93. Ibid.

94. DARWIN,C.G. [1922] N 110 p.841, "A quantum theory of optical dispersion".

95. Ref. 22.

96. Ref. 20, p.374.

97. BOHR Collected Works v.3. p.397, "Application of the quantum theory to atomic problems in general." : p.413.

98. App. E, ref. 19, p.40.

99. Ehrenfest to Einstein, 17.1.22 SHQP, trans. KLEIN. [1970]

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100. BOHR Collected works v.3. p.569, "Problems of the atomic theory." The dating of this manuscript is not clear. It could be 1924, but the absence of any reference to the Bohr-Kramers-Slater paper suggests late 1923. p.572.

101. Ref. 49.

102. Ref. 40.

103. SOMMERFELD,A. [1922] Vieweg, Atombau und Spektrallinien ,2nd edn. (Jan. 22); trans Methuen [1923], quote from p.253.

104. EINSTEIN,A. & EHRENFEST,P. [1922] ZP 11 p.31, "Quantentheoretische Bemerkung zum Experiment von Stern und Gerlach."

105. BORN,M. & HEISENBERG, W. [1923] ZP 16 p.229, "Die elektronenbahnen im angeregten Heliumatom."

106. Ch.I., ref. 136.

107. Ref. 44, p.157.

108. App.E., ref. 19., p.35.

109. Ibid., p.1.

110. Ref. 100, p.571.

111. FORMAN [1971].

112. As in his response in 1924 to the Bohr-Kramers-Slater theory, see below. See also ref. 113.

113. Einstein to Mrs. Born, 1.9.19 BEC.

114. Born, interview SHQP [1962], and typed recollections SHQP [1948]. The 1948 recollections are the only ones of even marginal historical value, and even they must be treated carefully. The interview is rambling and inaccurate.

115. SENFTLEBEN,H.A. [1923] ZP 22 p.127, "Zur Grundlagen der Quantentheorie." This is Hans Albrecht Senftleben, not to be confused with Hermann Senftleben, an experimental physicist. Unfortunately they have been confused in the SHQP index and ever since; thus FORMAN [1971], discussing H.A., referred to a letter catalogued and indexed in SHQP under Hermann, but crossreferenced variously as H.A. and H.R.. In WAERDEN [1967], H.A.'s paper is correctly indexed but a paper by Hermann is indexed under H.A..

116. Ch.I., ref. 88.

117. See FORMAN [1971]

118. Ch.I , ref. 88.

119. Ibid.

120. PETZOLDT,J. [1918] VDPG 20 p.189; [1919] VDPG 21 p.495; [1920] ZP 1 p.467, argued against causality. HOLST,H. [1920] KDVS 2,no.11 p.1; ZP 1 p.32, argued in its favour. The problem was one that arose naturally with the abandonment of absolute time.

121. A fuller discussion of this aspect is included in appendix H.

122. Einstein to Born, 27.1.20 BEC, discussed causality:

> "Can the quantum absorption and emission of light ever be understood in the sense of the complete causality requirement, or would a statistical residue remain? ... But I would be very unhappy to renounce complete causality."

Forman took this to mean that Einstein would happily renounce a part of causality, but it is clear from the context that he meant that he would not give up any part of the strict causality requirement.

123. Lorentz allowed the republication in 1921 of his paper to the second Solvay Congress, which included a defence of determinism: LORENTZ,H.A. [1921] Revue de l'Université de Bruxelles 26 p.445, "La révision scientifique."

124. TETRODE,H. [1922] ZP 10 p.317, "Uber der Wirkungszusammenhang der Welt. Eine Erweiterung der klassischen Dynamik." This theory was not developed at the time, but it was a forerunner of the absorber theory of radiation: LEWIS,G.N. [1925] PNAS 11 p.182, "A new principle of equilibrium" ; p.423, "The distribution of energy in thermal radiation and the law of entire equilibrium; WHEELER,J.A. & FEYNMAN,R.P. [1945] RMP 17 p.157, "Interaction with the absorber as the mechanism of radiation." Tetrode's theory can also be seen as an attempt to derive quantum behaviour by a redundancy in determination, such as Einstein sought.

125. Ref. 62.

126. SCHRÖDINGER [1922] Inaugural lecture at Zurich, published [1929] dN 17 p.9; the debt to Exner is explicit and emphasised.

127. Ref. 115.

128. BOHR,N & KRAMERS,H.A. & SLATER,J.C. [1924] PM 47 p.785, "The quantum theory of radiation."(rec. Jan. 24); ZP 24 p.69, "Uber der Quantentheorie der Strahlung."; PM paper rep. in WAERDEN [1967]

129. BOHR,N. [1922] 7th Guthrie lecture (24.3.22), published in [1923] PPSL 35 p.275.

130. AppE, ref. 19, p.20. Bohr continued:
"According to this method of treatment, we do not seek a cause for the occurrence of radiative processes, but we simply assume that they are governed by the laws of probability."
This is getting very close indeed to anticausality, but the first phrase is important.

131. Ref. 128 (WAERDEN) p.162.

132. By JAMMER [1966] p.114.

133. Ref. 17 (WAERDEN) p.100.

134. AppE, ref. 19, p.21.

135. Ref. 100, p.571.

136. Elements of this topic have been studied by SERWER [1977] and CASSIDY [1976], as well as by FORMAN [1968], but Forman and Cassidy have restricted themselves completely, and Serwer largely, to the failure of the old quantum theory in a very narrow context, and none of them have considered the general conceptual problem as in this work. This work was substantially completed before I could obtain copies of Serwer's (as a preprint) or Cassidy's (as a microfilm dissertation) papers, and the argument owes nothing to them, but both have been very useful in providing extracts from the SHQP archive that I had myself not picked up, and that add force to my arguments.

137. Pauli to Bohr, 31.12.24. SHQP.

138. As early as 1921, Heisenberg wrote of the anomalous Zeeman effect that
"It is still gradually becoming the general conviction that one must give up much of the hitherto mechanics and physics if one wants to approach the Zeeman effect in another way.", Heisenberg to Landé, 26.10.21 SHQP, trans. CASSIDY.

139. LANGMUIR,I. [1921] PR 17 p.339, "The structure of the Helium atom."

140. EPSTEIN,P.S. [1922] PR 19 p.578, "Problems of the quantum theory in the light of the theory of perturbations."

141. VLECK,J.H.van. [1923] PM 44 p.842, "The normal Helium atom and its relation to the quantum theory." He concluded that some "radical modification" of the theory was necessary.

142. KRAMERS,H.A. [1923] ZP 13 p.312, "Uber das Modell des Heliumatoms." (rec. 31.12.22).

143. Especially through Sommerfeld, who was in America for the winter and spring of 1922-3.

144. Ref. 49.

145. PAULI,W. [1925] dN 13 p.487, revue of Born's Vorlesungen über Atommechanik :
"Die aufgewandte Mühe nicht den erreichten Resultaten entspricht, zumal diese hauptsächlich negativ sind."

146. He had worked briefly on the applications of Planck's law: see BORN,M. & COURANT,R. [1913] PZ 14 p.731.

147. FRANCK,J. & HERTZ,G. [1914] VDPG 16 p.457, "Uber Zusammenstösse zwischen Electronenund den Molekülen des Quecksilberdampfes und die Ionisierungsspannung desselben."; [1916] PZ 17 p.409, "Uber Kinetik von Elektronen und Ionen in Gasen."; [1919] PZ 20 p.132, "Die Bestätigung der Bohrschen Atomtheorie im optischen Spektrum durch Untersuchungen der unelastischen Zusammenstösse langsamer Elektronen mit Gasmolekulen."

148. Interview SHQP [1962] with J.Franck.

149. BORN,M. & PAULI,W. [1922] ZP 10 p.137, "Uber die Quantelung gestörten mechanischer Systeme." Born had already done some preliminary work on the problem with Brody, earlier in 1921: BORN,M. & BRODY,E. [1921] ZP 6 p.140, "Uber die Schwingungen eines mechanischer System mit endlicher Amplitude und ihre Quantelung."

150. Ref. 57.

151. BORN,M. & HEISENBERG,W. [1923] ZP 14 p.44, "Uber Phasenbeziehungen bei den Bohrschen Modellen von Atomen und Molekeln."

152. STERN,O. & GERLACH,W. [1922] ZP 8 p.110, "Der experimentelle Nachweis des magnetischen Moments des Silberatoms"; ZP 9 p.349, "Der experimentelle Nachweis der Richtungsquantelung im Magnetfeld."

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153. EINSTEIN,A & EHRENFEST,P. [1922] ZP 11 p.31, "Quantentheoretische Bemerkungen zum Experiment von Stern und Gerlach."

154. BORN,M. & HEISENBERG,W. [1923] ZP 16 p.229, "Die Elektronenbahnen im angeregten Heliumatom."

155. Ibid., p.229:
"Wir haben uns nun die Aufgabe gestellt, alle möglichen Bahntypen des angeregten Heliumatoms systematisch aufzusuchen, die quantentheoretisch zulässingen Lösungen auszusondern und die Energiewerte zu berechenen, um festzustellen, ob nicht Bahn vorhanden sind, die die empirische Terme richtig liefern. Das Ergebnis unserer Rechnung ist negativ: man gelangt durch konsequente Anwendung der bekannten Quantenregeln zu keiner Erklärung des Heliumspektrums."

156. SMEKAL,A. [1925] PB 6 p.1258.

157. Ref. 49.

158. PAULI,W. [1922] AP 68 p.177, "Über das Modell des Wasserstoffmolekülions."

159. See FORMAN [1968] and JAMMER [1966] for references.

160. Again see Forman and Jammer (ibid.), but some explanation is necessary, as both the primary and secondary literature are confused. The problem was two-fold: to determine the stationary states (by quantum numbers) and to determine the possible transitions (by selection rules). Bohr and Sommerfeld originally used two quantum numbers, n and k, with $n \geqslant k \geqslant 0$, selections by $k=\pm 1$. Later, j was introduced (SOMMERFELD,A. [1920] AP 63 p.221) such that $j=\pm 1, 0$ but with $j=0 \rightarrow j=0$ not allowed (LANDE,A. [1921] PZ 22 p.417). In the core model, k was linked with the orbital angular momentum of the valence electron (of an alkali) and j with the total angular momentum, the difference being attributed to the angular momentum of the core, r. For the Zeeman effect, the component of total angular momentum in the direction of the applied field was

introduced: m; $m=\pm 1, 0$, $-j \leqslant m \leqslant j$. To obtain the observed results it was found (LANDE,A. [1921] ZP 5 p.231) that some numbers had to be given half-integer values, against the fundamental principle of the theory, and to obtain the energy of the atom in the magnetic field, i.e. the g-factor, the core had to be counted twice in its contribution. (Also, terms such as j^2 had to be replaced by $j(j+1)$.) Linked with this was the anomalous Paschen-Back effect: the ratio of the angular momentum of the core to its magnetic moment was found to be half the classically computed value.

Forman and Jammer discuss the development that arose, but the extent of the problem can be easily seen just by considering the selection rules as used today: transitions are governed by parity conservation and j conservation, with $0 \leqslant l \leqslant n-1$; $-l \leqslant m \leqslant l$; $-s \leqslant \sigma \leqslant s$; for a single electron, $s=\frac{1}{2}$, $|l-s| \leqslant j \leqslant |l+s|$; for two or more electrons, $|l_1 - l_2| \leqslant l \leqslant |l_1 + l_2|$ and similarly for the spins s. In the early 1920s however, they had no concept of spin, no concept of parity: the problems were thus enormous.

Finally, to further help sort out some of the confusion, I give a comparison table of notations, necessary if one is to know which quantum numbers are being talked about.

Standard notn.	n	l	j	s	m	σ	
Other common notns.	n	$k-\frac{1}{2}$	n_1	r	m_l	m_s	m_j
Landé's notn.	n	$K-\frac{1}{2}$	$J-\frac{1}{2}$	R			m
Pauli's notn.	n	$k_1-\frac{1}{2}$	$k_2-\frac{1}{2}$		m_1		

161. Bohr to Landé, 15.5.22 SHQP :
"Mein Gesichtspunkt is der, dass die ganze Quantisierungsart (halbzählige Quantenzahlen u.s.w.) nicht mit dem Grundprinzipien der Quantentheorie vereinbar scheint."
He continued:
"Der stärkste Einwand gegen die Heisenbergsche Arbeit liegt für mich vielleicht darin, dass die Abweichungen von der klassischen Theorie, die seinen Rechnungen unterliegen, kaum erwähnt und gar nicht präzisiert werden, obwohl es ja eine fundamentaler Punkt ist."

162. Bohr to Landé, 3.3.23 SHQP :
"Es war, wie Sie gesehen haben, ein Verzweiflungsversuch, den ganzen Quantenzahlen treu zu bleiben, indem wir hofften, eben in den Paradoxen einen Fingerzeig zu sehen für die Wege, auf denen man die Lösung des annomalen Zeemaneffektes suchen dürfte."

163. Heisenberg to Landé, 3.3.23 SHQP :
"Ich selbst bin jetzt auch ebenso wie Herr Prof. Sommerfeld, fast überzeugt davon, dass die Halbenquantenzahlen, im Gegensatz zu Bohrs Ansicht, richtig sind."

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164. Ibid.:
"Sommerfeld schreibt mir aus Amerika, dass ein amerikanischer Mathematiker van Vleck das Bohrsche-Modell gerechnet u. exakt den experimentelle falschen Wert von 22V für die Ionisierungsspannung gefunden hat. Der Bohrsche-Modell muss also falsch sein."

165. Ibid.

166. HUND,F. [1924] ZP 22 p.405 "Rydbergkorrektionen und Radien der Atomrümpfe." (rec. 25.2.24)

167. BORN,M. & HEISENBERG,W. [1924] ZP 23 p.388, "Uber den Einfluss der Deformierbarkeit der Ionen auf optische und chemische Konstanten I." Unfortunately, Sommerfeld had found by October 1924 that the half-integer model didn't work either: Ref 56, p.206 (German):
"Indessen hat eine vorlaüfige Stabilitätsrechnung auch dieses Modell als instabil ergeben."

168. Pauli to Bohr, 21.2.24 SHQP:
"Die Atomphysiker in Deutschland verfallen jetzt in zwei Klassen. Die einen rechnen ein bestimmtes Problem zuerst mit halbzahligen Werten der Quantenzahlen durch und wenn es dann mit der Erfahrung nicht stimmt, rechnen sie es dann noch mit ganzen Quantenzahlen. Die anderen rechnen zuerst mit ganzen Zahlen und wenn es nicht stimmt, dann rechnen sie eben mit halben. Beiden Klassen von Atomphysikern haben aber die Eigenschaft gemeinsam, dass aus ihren Theorien a priori keinerlei Argumente zu gewissen sind, bei welchen Quantenzahlen u. bei welchen Atomen man mit halbzahligen Werten der Quantenzahlen u. bei welchen man mit ganzzahligen Werten zu rechnen hat. Dies können sie vielmehr bloss a postereori durch Vergleich mit der Ehrfahrung entscheiden. Ich selbst kann an dieser Sorte von theoretischer Physik keinerlei Geschmack gewinnen und ziehe ich mich von ihr zu meiner Wärmeleitung im festen Körper zurück."

169. See FORMAN [1968].

170. LANDE,A. [1923] PZ 24 p.441, "Schwierigkeiten in der Quantentheorie des Atombaues, besonders magnetischer Art." See FORMAN [1968]

171. BOHR,N. [1920] ZP 2 p.423, "Uber die Serienspektren der Elemente."; trans in [1922] C.U.P., The theory of spectra and atomic constitution , as "On the series spectra of elements."

172. LANDE,A. [1922] ZP 11 p.353, "Zur Theorie der annomalen Zeeman- und Magnetomechanischen Effektes: p.361.

173. BOHR,N. [1923] manuscript in BOHR Collected wks.v.3,p.502; trans. p.552, from which all quotations taken.

174. Ibid., p.558.

175. Although the manuscript was never published, it was discussed with Pauli, Kramers, Heisenberg, and maybe others; its contents would almost certainly have been familiar to most if not all those physicists who were involved in the origins of quantum mechanics.

176. App. E, ref. 19 .

177. Pauli to Bohr, 14.7.23 SHQP, trans in BOHR Collected works v.3.

178. Heisenberg to Bohr, 22.12.23 SHQP.

179. Ibid. and HEISENBERG,W. [1924] ZP 26 p.291, "Uber eine Abänderung der der formalen Regeln der Quantentheorie beim Problem der annomalen Zeemaneffekte." (rec. 13.6.24) . See Ch. III for discussion.

180. Heisenberg to Bohr, 3.2.24 SHQP:
"Ich sehe eine Hoffnung, die halben Quantenzahlen aus den Formalisierung $\int HdJ$ zu erhalten."

181. Pauli to Landé, 14.12.23 SHQP:
"Ihre Ansicht uber Heisenbergs neue Theorie teile ich in keiner weise. Ich halte diese sogar für unschön. Denn trotz radikaler Annahmen liefert sie keine Aufklärung der halben Quantenzahlen und des Versagens des Larmor-Theorems (im besondere der magnetisch Anomalie). Ich hatte nicht viele von der ganzen Sache."

182. PASCHEN,F. [1923] PZ 24 p.401, "Die spektroscopische Erforschung des Atombaus." wrote that "The present contradictions must be augmented by further incomprehensible problems."

183. Ref. 180. Landé saw the need for a new, as yet unknown, internal mechanics of the atom.

184. Pauli to Sommerfeld, 6.6.23 SHQP:
"Und vor allem, dass sie Wegen des Versagens der klassischen Mechanik auch inden stationären Zustanden selbst bei Systemen mit mehr als einem Elektron überhaupt keine ausreichende Grundlage für die quantitative Berechnung der Spektren solcher Systeme liefert.

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"An diesem Versgen kann kaum mehr geweifelt werden und es scheint mir eines der wichtigsten Ergebnisse des letzten Jahres zu sein, das die Schwierigkeiten des Mehrkörperproblems bei den Atomen physikalischen und nicht mathematischen Art sind. (Wenn z.b. bei Born u. Heisenberg die He Terme falsch herauskommen, so liegt dies gewiss nicht daran, dass die Näherung nicht ausreichend ist."

185. Heisenberg to Pauli, 6.3.22. SHQP:
"Schicke ich Ihnen hiermit meinen Zeemanbraten mit Quantensosse ohne damit die Absicht zu verbinden, Sie von meiner Ansicht zu überzeugen!"

186. Heisenberg to Bohr, 2.2.23. SHQP.

187. Heisenberg to Pauli, 19.2.23. SHQP:
"entweder neue Quantenbedingungen, oder Abänderungsvorschläge für die Mechanik."

188. As also did Lorentz, who wrote (ref.55) that "there is no doubt that a mechanics of quanta, a mechanics of discontinuities, still has to be made."

189. BORN,M. [1923] dN 11 p.537, "Quantentheorie und Storungstheorie.":p.542:
"Is wird immer wahrscheinlich ... das ganze Systeme der Begriffe der Physik von Grund aus umgebaut werden muss."

190. Ref. 49 and HEISENBERG [1971]

191. Heisenberg to Pauli, 19.11.21. SHQP, trans. CASSIDY. [1976]

192. Heisenberg to Landé, 26.10.21. SHQP

193. Heisenberg to Landé, 26.10.21, 29.10.21. SHQP.

194. Quoted in HEISENBERG [1960]

195. Ibid.

196. Ibid.

197. SOMMERFELD,A & HEISENBERG,W. [1922] ZP 11 p.131, "Die Intensität der Mehrfachlinien und ihre Zeemankomponenten.": p.132:
"Auch über den modellmässigen Ursprung der Atombahnen brauchen wir uns keine genaueren Vorstellung zu bilden; es genügt für des Folgende die vorangehende allgemeine kinematische Beschreibung."

198. SOMMERFELD,A. & HEISENBERG,W. [1922] ZP 10 p.393, "Eine Bemerkung über relativistische Röntgendubletts und linienscharfe.": p.398:
"In der Tagt liegt das nicht in der Absicht des Korrespondenzprinzips welches ja auf jedes modellmässige Verständnis verzichtet."

199. Heisenberg to Pauli, 19.2.23. SHQP, trans. CASSIDY. [1976]

200. Heisenberg to Pauli, 6.3.23. SHQP, trans. CASSIDY. [1976]

201. Pauli to Landé, 5.6.23. SHQP, trans. CASSIDY. [1976]

202. Pauli to Landé, 23.5.23. SHQP:
"Wie Sie sehen werden, habe ich mich auf das rein phänomenologische beschränkt und alle modellmässigen Betrachtungen weggelassen."

203. Pauli to Landé, 17.8.23. SHQP:
"Denn ich bin überzeugt, dass es fur den anomalen Zeemaneffekt kein bedingt periodisches Modell gibt u. etwas prinzipielles Neues machen muss."

204. PAULI,W. [1923] ZP 16 p.155, "Uber die Gesetzmässigkeiten anomalen Zeemaneffektes.":p.155:
"Andererseits stehen einer modellmåssigen Deutung der emperisch Gesetzmässigkeiten zurzeit noch sehr grosse Schwierigkeiten entgegen."

205. Ibid., p.155-6:
"Bei diese Sachlage ist es vielleicht nicht unangebracht, zu versuchen auch abgesehen von Modellbetrachtungen einfache Formale Eigenschaften der Werte der Kombinationsterme beim anomalen Zeemaneffekt festzustellen."

206. Heisenberg to Pauli, 9.10.23. SHQP trans. CASSIDY. [1976]

207 Pauli to Bohr, 21.2.24. SHQP:
"Die wichtigste Frage scheint mir die, inwieweit überhaupt von bestimmten Bahnen der Elektronen in den stationären Zuständen gesprochen werden darf. Ich glaube, dass diese keines Wegs als selbstverständlich vorausgestetzt darf, besonders im Hinblick auf Ihre Betrachtungen über die Bilanz der Statistischen Gewicht bei der Kopplung. Heisenberg hat nach meiner Ansicht gerade in Punkt das Richtige getroffen, dass er die Möglichkeit, von bestimmten Bahnen zu sprechen, bezweifelt. Zweifel dieser Art hat mir Kramers früher wie als vernünftig zugegeben. Ich muss trotzdem darauf bestehen denn die Sache scheint mir zu wichtig."

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208. DEBYE,P. [1915] MB p.1, "Die Konstitution des Wasserstoff-Moleküls." SOMMERFELD,A. [1917] AP 53 p.497, "Die Drudesche Dispersionstheorie vom Standpunkte des Bohrschen Modelles und die Konstitution von H_2,O_2,und N_2."
DAVISSON,C. [1916] PR 8 p.20, "The dispersion of Hydrogen and Helium in Bohr's theory."

209. Ref. 17 (WAERDEN) p.98.

210. LADENBURG,R. [1921] ZP 4 p.451, "Die Quantentheoretische Zahl der Dispersionselektronen."; trans. in WAERDEN [1967].

211. LADENBURG,R. & REICHE,F. [1923] dN 11 p.584, "Absorption, Zerstreuung und Dispersion in der Bohrsche Atomtheorie.": p.597:

> "Jedoch glauben wir auf Grund der beobachten Erscheinungen das Endresultat der Einwirkung einer Strahlung von der Schwingungszahl ν auf Atome als nicht wesentlich verschieden von dem Effekt auschen zu müssen, den eine solche Welle auf klassische Oszillatoren ausübt es entstehen sekundäre Wellenzüge von der gleichen Schwingungszahl ν in Phase mit der aufallenden Welle. Soger die Stärke der zerstreuten Strahlung scheint häufig mit der von einem Oszillator entsandten angenähert uber einzustimmen; darauf beruht es offenbar, das die rein klassische Rechnung in vielen Fällen auch quantitativ die Beobachtungen annähernd wiederzugeben imstande ist. Aber nicht Immer! Haufig ist auch ein Versagen der klassischen Theorie, in quantitative Beziehungen wenigstens bemerkt worden."

212. Ref. 97, p.414.

213. Bohr to Darwin, 21.10.22. SHQP.

214. App.E, ref. 19, p.38.

215. Ref.194:

> "Ich denke oft daran, dass vielleicht nicht nur bei der Dispersion, wo es sich am die Einwirklung einer einfach harmonischen periodischen ausseren Kraft handelt, sondern auch bei der Wechselwirkung der Elektronen im Atom, sich die einzelnen Elektronenbahnen mehr wie ein System von Oszillatoren verhalten, deren Frequenzen nicht mit denen der Bewegung, sondern mit denen der Ubergänge ubereinstimmen. (Etwas ähnliches hat schon Epstein geragt)"

This may have been linked with some of Born's work of the same period. Born studied high frequency electron-electron coupling in the atom, and ran into considerable difficulties: BORN,M. [1923] dN 11 p.537, "Quantentheorie und Störungsrechnung"; recd. 6.7.23.

216. Pauli to Bohr, 16.7.23. SHQP, trans. in BOHR Collected works, v.3.

217. KRAMERS,H.A. [1923] PM 46 p.836, "On the theory of X-ray absorption and the continuous X-ray spectrum."

218. Ibid., p.861.

219. Ibid., p.843.

220. Ibid., p.852.

221. Ref. 217.

222. App.E, ref. 19, p.42:

> "As frequently emphasised, [the correspondence and adiabatic] principles, although they are formulated by help of classical conceptions, are to be regarded purely as laws of the quantum theory which give us, notwithstanding the formal nature of the quantum theory, a hope in the future of a consistent theory, which at the same time reproduces the characteristic features of the quantum theory, important for its applicability, and, nevertheless, can be regarded as a rational generalisation of classical electrodynamics.

NOTES TO CHAPTER III

1. Ch.II, ref.128: all references will be to the reprint in WAERDEN [1967].
2. Ibid., p.159.
3. App. E, ref.19.
4. Ref. 1, p.161.
5. Ibid., p.162.
6. Ibid., p.164.
7. Ibid., p.164-5.
8. He may already have reached it if the 1923/4 manuscript predates BKS — see Ch. II, ref. 100.
9. Bohr always stressed the "formal nature" of his theories, and it is not clear how much significance can be attributed to the phrase.
10. Interview with Slater [1963] SHQP.
11. Slater to Kramers 8.12.23. SHQP.
12. Ibid.
13. Bohr's 1923 survey might well have been a subject for discussion at Cambridge, where Fowler communicated it to the Cambridge Philosophical Society.
14. Slater to van der Waerden 4.11.64: WAERDEN [1967]
15. Fowler to Bohr 14.1.24. SHQP seems to tie in :
    "I am glad you are hopeful of Slater. I thought he was on sound lines and I encouraged him as much as I could, which wasn't very much of course."

    Kemble to Bohr 4.1.23. (catalogued as 1924) suggested that
    "Perhaps Dr. Slater's recent scheme for a combination of the wave theory and the corpuscular theory may solve our difficulties."
16. This is unfortunate, for initial recollections of one's own influences are notoriously unreliable.
17. This is assumed by JAMMER [1966].
18. Ch. II, ref.72, PM.
19. SLATER,J.C. [1924] N 113 p.307, "Radiation and atoms".
20. Refs. 10, 14.
21. Ref. 19.
22. COMPTON, A.H. & SIMON,A.W. [1925] PR 26 p.289, "Directed quanta of scattered rays";
    BOTHE,W. & GEIGER,H. [1925] dN 13 p.440, "Uber das Wesen des Comtoneffekts; ein experimenteller Beitrag zur Theorie der Strahlung".
23. SLATER,J.C. [1925] N 116 p.278, "The nature of radiation".
24. This feeling was amplified by Slater's dislike of Bohr's vagueness. To Bohr this was a combination of caution and subtlety, but to Slater it was "handwaving", and he was particularly annoyed by Bohr's favourite saying that "you cannot really explain it in the framework of space and time". Due to the antipathy between Slater and Bohr, Slater left Copenhagen well before he had been due to: see ref. 10.
25. Slater to van Vleck 27.7.24. SHQP.
26. Interview with Heisenberg [1963] SHQP; see also HEISENBERG [1971]. Aristotle might not have agreed.
27. Heisenberg to Bohr 8.1.25. SHQP: see below.
28. Ref. 1, p.172.
29. See DEMPSTER [1923] AJ 57 p.193, and ref. 1, p.170.
30. He likened the problem to a more everyday one: when electromagnetic rays are emitted from an antenna, the duration of each transition is only a small fraction of the period of oscillation of the electric current causing the emission. So how, he asked, without the virtual oscillators could the emitted rays 'know' what this period was?
31. Ref. 1, p.173:
    "The scattering of the radiation by the electrons is, on our view, considered as a continuous phenomenon to which each of the illuminated electrons contributes through the emission of secondary

wavelets. Thereby the incident virtual radiation gives rise to a reaction from each electron, similar to that to be expected on the classical theory from an electron moving with a velocity coinciding with that of the ... imaginary source [which would classically emit the scattered radiation] and performing forced oscillations under the influence of the radiation field. That in this case the virtual oscillator moves with a velocity different from that of the illuminated electrons themselves is certainly a feature strikingly unfamiliar to the classical conceptions. In view of the fundamental departures from the classical space-time description, involved in the very idea of virtual oscillators, it seems at the present state of science hardly justifiable to reject a formal interpretation as that under consideration as inadequate."

I have quoted this at such length because it seems to me to show quite strikingly the problems with which Bohr was faced. The explanation is so unconvincing as to be almost counter-productive.

32. BORN,M. [1924] ZP 26 p.379, "Uber Quantenmechanik", recd. 13.6.24; trans. in WAERDEN [1967] : quote from p.181.of translation, to which all references will be made.

33. Ibid., p.189.

34. Ladenburg to Kramers 31.5.24. My interpretation is confirmed by Ladenburg to Kramers 8.6.24, where Einstein's reaction to the BKS theory is described as:

"Seine Meinung was entschrieden nicht ungünstig."

This is difficult to understand, as we shall see that Einstein rejected the theory completely, but can perhaps be explained by a combination of factors: Einstein not wishing to hurt Ladenburg's feelings; Ladenburg not wishing to hurt Kramers' feelings; and Ladenburg taking the theory independent of the physical considerations with which Einstein was most (though not solely) concerned. It may of course have been a slip of the pen, a possibility that is not eliminated by the (sparse) context. Both letters in SHQP.

35. FOWLER,R.H. [1925] Manuscript in SHQP.
BECKER,R. [1924] ZP 27 p.173, "Uber Absorption und Dispersion in Bohr's Quantentheorie."

36. Schrödinger to Bohr 24.5.24. SHQP.

37. SCHRÖDINGER,E. [1924] dN 12 p.720, "Bohr's neue Strahlungshypothese und der Energiesatz".

38. Heisenberg to Pauli 8.6.24. SHQP. (catalogued as 1925)

39. Heisenberg to Pauli 4.3.24. SHQP, trans. CASSIDY. [1976]

40. VLECK,J.H.van [1926] BNRC 54 p.270, "Quantum principles and line spectra."

41. Ch. II, ref.29.

42. STONER,E.C. [1925] PCPS 22 p.577, "The structure of radiation".

43. Quoted by Compton in Ch. II, ref.29. See STEUWER [1975] .

44. Ehrenfest to Einstein 9.1.25. Einstein colln. trans. STEUWER.

45. See ref. 34.

46. Einstein to Ehrenfest 12.6.24. SHQP. Trans. KLEIN. [1970]

47. Einstein to Ehrenfest 12.6.24. SHQP. Trans. STEUWER. [1975]

48. Einstein to Ehrenfest 31.5.24. SHQP. Trans. STEUWER. [1975]

49. Pauli to Bohr 2.10.24. SHQP :

"1: dass bei statistischen Unabhangigkeit des Vorkommens der Elementarprozzesse an raümlich distanten Atomen eine System im Laufe der Zeit systematische Abweichungen vom ersten Hauptsach zeigen könnte. .. Er finde dies degoutant(so sagt er).

Einstein's second objection was based on energy conservation, his third on the existence of light-quanta. The fourth was the complaint about pre-established harmony referred to in the text.

50. Ibid.

51. Pauli to Bohr 21.2.24. SHQP:

"Ich habe versucht, auf Grund der Kenntnis dieser beiden Worte ... zu erraten, warüber Ihre Arbeit wohl handeln wird. Dies ist nur aber nicht gelungen. Es wird mich jedenfalls sehr interessieren, Sie zu lesen und falls ich in sprachlichen Hinsicht etwas helfen kann, ich es gerne hin."

52. Pauli to Bohr 2.10.24. SHQP.

53. Heisenberg to Bohr 8.1.25. SHQP:
"Er glaube ... nicht aber an virtuelle Oszillatoren und schimpft über die 'Virtualisierung' der Physik. Mir ist nicht klar, was er damit meint."

54. Pauli to Kramers 27.7.25. SHQP:
"Denn sie bewegen sich in einen ganz falschen Richtung: nicht der Energie-begriff ist zu modifizieren, sondern der Bewegungs- und der Kraft-begriff. Zwar kann man in Fällen, wo Interferenzerscheinungen vorhanden sind, keine bestimmten "Bahnen" von Lichtquanten definieren, man kann aber auch für die Elektronen im Atom keine solche Bahnen definieren; und ebensowenig, wie es berechtigt wäre, deshalb die Existenz von Elektronen zu bezweifeln, ist es berechtigt, wegen der Interferenzerscheinungen die Existenz der Lichtquanten zu bezweifeln."
It will be noted that Pauli had no qualms about directing his criticism at those he was criticising.

55. Though as we shall see Pauli in fact anticipated Heisenberg on the point in question.

56. In this I disagree with FORMAN [1973]: see appendix H for discussion.

57. KRAMERS,H.A. [1924] N 113 p.673, "The law of dispersion and Bohr's theory of spectra", recd. 25.3.24; rep. in WAERDEN [1967] to which all references will be made.

58. Ibid., p.179.

59. BREIT,G. [1924] N 114 p.310 objected to the negative oscillators (for which e^2/m of the classical equivalent was negative) and suggested an alternative approach which merely ignored the relevant part of the observed dispersion.

60. KRAMERS,H.A. [1924] N 114 p.310, "The quantum theory of dispersion", recd. 22.7.24; rep. in WAERDEN [1967] to which all references made.

61. Ibid.,p.200.

62. Ref. 32.

63. Ibid., p.181. We may recall that Born had run into the problem of high frequency electron-electron coupling in the previous summer; but in this paper he in fact applied the virtual oscillator technique to complex mechanical systems, and not to the single electron.

64. Ibid., p.182.

65. Ibid., p.192.

66. Ibid., p.190.

67. Ibid., p.191.

68. Ch. II, ref. 179, ZP.

69. In addition, Bohr visited Göttingen in June and many physicists carried verbal messages between the two universities. In early 1925, Pauli visited Copenhagen, and Heisenberg spent time in both universities as well as seeing Pauli in Hamburg.

70. Ref. 32, p.182.

71. WAERDEN [1969].

72. Manuscripts in SHQP on recollections of advent of spin [1959], to which are attached correspondence with Waerden.

73. Ref. 26.

74. Kramers: recd. 22.7.24, pubd. 30.8.24; Born: recd. 13.6.24, pubd. Aug. 24.

75. Ref. 26.

76. Ref. 10. He thought he had written evidence in support of this, but he could not find it.

77. See Ch. I.

78. Ref. 26.

79. Ibid.

80. BORN [1949].

81. He had used a Fourier series expansion with Pauli in 1922 (Ch. II, ref. 149), but only for a generating function and it was not followed through.

82. JAMMER [1966]

83. We should again note Born's problem with electron-electron coupling, and Pauli's suggestion of the single electron behaving as a set of oscillators, remembering however that Born was not concerned in 1924 with the single electron.

84. Heisenberg to Landé 6.7.24. SHQP.

85. Heisenberg to Pauli 8.6.24. SHQP (catalogued as 1925)

86. Ref. 60, p.201.

87. Ref. 32, p.193.

88. KRAMERS,H.A. & HEISENBERG,W. [1925] ZP 31 p.681, "Uber die Streuung von Strahlen durch Atome", recd. 5.1.25; rep. in WAERDEN [1967] to which all references will be made.

89. SMEKAL,A. [1923] dN 11 p.873, "Zür Quantentheorie der Dispersion".

90. Ref. 26.

91. Ref. 88, p.228.

92. Ibid., p.229.

93. BORN,M. & JORDAN,P. [1925] ZP 33 p.479, "Zur Quantentheorie aperiodischer Vorgange, I", recd. 11.6.25; part II is
JORDAN,P. [1925] ZP 33 p.506, recd. same date.

94. Ibid., p.479:
"Wir haben uns daher die Aufgabe gestellt, die Frage nach der Existenz von Korrespondenz beziehungen bei nicht-periodischen systematisch zu untersuchen."

95. Ibid.:
"manches davon ist bereits von anderen Autoren auf anderen Wege gefunden worden, doch legen wir im Hinblick auf weitere Anwendung-Wert darauf, dass unsere Ableitung nicht nur einfacher ist, sondern auf viel allgemeinen Voraussetzungen beruht."

96. Manuscript in Dirac Archive.

97. LANDE,A. [1923] ZP 16 p.391, "Zur Theorie der Röntgenspektren".

98. LANDE,A. [1924] ZP 24 p.88, "Das Wesen der relativistisch Rontgendubletts"

99. FORMAN [1968].

100. In general this struck physicists as being more important than the BKS theory.

101. PAULI,W. [1925] ZP 31 p.373, "Uber den Einfluss der Geschwindigkeitsabhängigkeit der Elektronenmasse auf den Zeemaneffekt", recd. 2.12.24.

102. Ibid., p.385 concludes in favour of
"eine eigentümliche, klassisch nicht beschreibbare Art von Zweideutigkeit der quantentheoretische Eigenschaften des Leuchelektrons Zustande."
This phrase is repeated in
PAULI,W. [1925] ZP 31 p.765, "Uber den Zusammenhang des Abschlusses der Elektronengruppen mit der Komplexstruktur der Spektren", recd. 16.1.25, from which the quotation in the text is taken:
"Nach der hier vorgeschlagenen Auffassung äussert sich ferner der Borsche Zwang nicht in einer Durchbrechung der Permanenz der Quantenzahlen bei der Kopplung des Serienelektrons an den Atomrest, sondern nur in der eigentümlichen Zweideutigkeit der quantentheoretischen Eigenschaften der einzelnen Elektronen in den stationären Zuständen des Atoms."

103. STONER, E.C. [1924] PM 48 p.719, "On the distribution of electrons among atomic levels".

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104. Ref 102, "Uber den Zusammenhang ..."

105. Pauli to Bohr 31.12.24. SHQP:
"Weil befriedigender wäre es, wenn man auf den Grund einen allgemeinen (von der klassischen Mechanik abweichenden) Quantenmechanik direkt einschen könnte, das diese Fälle als stationäre Zustände nicht in Betracht kommen."

106. The metaphysical implications of the exclusion principle were not considered then, and have hardly been since. The only study appears to be MARGENAU [1944]. I shall touch on this subject briefly when I examine the exclusion principle further in the next chapter.

107. Pauli to Bohr 12.12.24, 31.12.24. SHQP

108. Pauli to Bohr 31.12.24. SHQP. He suggested that Kramers could probably make this explanation even more convincing, in his popular lectures.

109. Pauli to Bohr 12.12.24. SHQP:
"Die relativistische Dublett formel scheint mir nun zweifellos zu zeigen, dass nicht nur der dynamische Kraft-begriff, sondern auch der kinematische Bewegungs-begriff der klassischen Theorie tiefgehende Modifikationen wird erfahren müssen. (Deshalb habe ich auch die Beziehung 'Bahn' in meiner Arbeit durchweg vermieden) Da dieser Bewegungs-begriff auch dem Korrespondenzprinzip zu Grunde liegt, so müssen seiner Klärung vor allem die Anstrengungen der Theoretiker gelten. Ich glaube, dass Energie- und Impulswerte der stationären Zuständ etwas viel realeres sind als 'Bahnen'. DAS (noch unerreichte) Ziel muss sein, diese und alle anderen physikalisch realen, beobachtbaren Eigenschaften der stationären Zustände aus dem (gewissen) Quantenzahlen und quantentheoretischen Gesetzen zu deduzieren. Wir dürfen aber nicht die Atome in die Fesseln unserer Vorurteile schlagen wollen (zu denen nach meiner Meinung auch die Annahme der Existenz von Elektronenbahnen im Sinne der gewöhnlichen kinematik gehört), sondern wir müssen umgekehrt unsere Begriffe der Erfahrung anpassen."

110. Pauli to Bohr 31.12.24. SHQP:
"analogie zwischen dem Ausschiess den Falle k=0 u. m=0 beim H-Atom wider meiner Ausschliess regel für den Abschluss der Gruppen."

111. Heisenberg recalled that this idea appeared in his paper with Kramers, but I cannot see this, and Pauli's letter predates this paper anyway.

112. PAULI,W. [1919] VDPG 21 p.742, "Merkorperperihelberregung und Strahlenableutung in Weyl's Gravitationstheorie."

113. Einstein saw the observability criterion in relativity theory as a guide to correct our misconceptions and so allow us to understand better the nature of a real objective world; this perspective was widely shared.

114. Also in his suggestion that our ideas should adjust to experience.

115. See Ch. II.

116. Ref. 26.

117. WOOD,R. & ELLETT,A. [1923] PRS A103 p.396, "On the influence of magnetic fields on the polarisation of resonance radiation".

118. FOOTE,P.D., RUARK,A.E. & MOHLER,F.L. [1923] JOSA 7 p.415, "The D_2 Zeeman pattern for resonance radiation".

119. BOHR,N. [1924] dN 12 p.1115, "Zur Polarisation des Flourescenzlichtes", recd. 1.11.24.

120. HEISENBERG,W. [1925] ZP 31 p.617, "Uber eine Anwendung des Korrespondenzprinzips auf die Frage nach der Polarisation des Flourescenzlichtes", recd. 30.11.24.

121. BURGER,H. & DORGELO,H. [1924] ZP 23 p.258;
ORNSTEIN,L. & BURGER H. [1924] ZP 24 p.41; 28 p.135; 29 p.241.

122. Ref. 26. One could perhaps look at Heisenberg's procedure as an extension of the use of the 'potentia' concept, but this analogy is tenuous.

123. Ref. 120, p.621:
"Trotzdem haben wir allen Grund, auszurechnen, dass diese Polarisation nicht vorhanded ist."

124. Ibid., p.617:
"Dies hat seinem Grund darin, dass die virtuellen oszillatoren nur in einer sehr symbolischen Weise mit der Bewegubg der Elektronen in den stationären Zuständen verknüpft sind."

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125. Ibid., p.617:
"Besonderes deutlich tritt dieser Umstand hervor, wenn wir es mit einem entarteten Problem zu tun haben, da dann, wie Bohr näher ausgeführt hat, den virtuellen Oszillatoren ein höherer Grad von Freiheit zukommt als der Bewegung in den stationaren Zuständen. Wenn sich in dieser Weise gezeigt hat, dass die virtuellen Oszillatoren die Strahlung des Atoms bestimmten, in gewissem Sinne unabhängig von der Bewegung der Elektronen des Atoms in dem betreffenden stationären Zustand, so kann man erwarten, dass sich die Analogie der virtuellen Oszillatoren mit den klassischen Strahlungsgrössen in manchen Fällen schärfer durchführen lässt, als die bei einer Betrachtung der stationären Zustände allein möglich erschiene."

126. Ibid., p.617:
"Das Ziel der vorliegenden Arbeit ist, zu zeigen, wie verschiedene empirische Regeln über Intensität und Polarisation von Spektrallinien aufgefasst werden können als sinn mässige Verschärfungen des Korrespondenzprinzips, und wie dieser Gesichtspunkt dazu benutzt werden kann, die Resultate der Experimente über die Polarisation des Flourescenzlichtes quantitativ zu beschreiben."

127. Ch. II, ref. 56.

128. HEISENBERG,W.[1925] ZP 32 p.841, "Zur Quantentheorie der Multiplettstruktur und der anomalen Zeemaneffekte", recd. 10.4.25.

129. Ibid., p.842:
"I Das Elektron wirkt auf den Atomrest durch einen unmechanischer Zwang derart, dass sich der stationäre Zustand des Atomrest scheinbar verdoppelt, oder aber
II Der Atomrest wirkt auf des Elektron durch einen unmechanischer Zwang derart, dass sich der stationäre Zustand des Elektron scheinbar verdoppelt."

130. Ibid., p.856:
"Die Anwendung des Korrespondenzprinzips zur Ableitung von Auswahlregeln und Intensitätsgesetzen ist nur möglich bei Benutzung eindeutiger mechanischer Modelle. Wenn es sich daher von die Berechnung der Intensitäten von Linien, die Sprüngen eines Elektrons entsprechen handelt, so werden wir die Fourierentwicklung der Bewegung des Elektrons in dem jenigen Schema zu untersuchen haben, in welchen des äussere Elektron eindeutig beschrieben wird ..."

131. Pauli to Bohr 12.12.24. SHQP.

132. Ibid.:
"Aber ich glaube dass das, was ich hier mache, kein grösserer Unsinn ist als die bisherige Auffassung der Komplexstruktur. Mein Unsinn ist zu dem bisher üblichen Unsinn konjugiert. Eben deshalb glaube ich, dass dieser Unsinn beim jetzigen Stand des Problems notwendigerweise gemacht werden muss. Der Physiker dem es einmal gelingen wird, dieser beiden Unsinne zu addieren, der wird die Wahrheit erhalten!"

133. Heisenberg to Pauli 15.12.24. SHQP:
"Heute habe ich Ihre neue Arbeit gelesen und es ist sicher, dass ich derjenige Mensch bin, der sich am meisten darüber freut, nicht nur weil Sie den Schwindel auf eine bisher ungeahnte schwindelhafte Höhe treiben u. damit alle bisherigen Rekorde, deren Sie mich beschimpfen, speilend geschlagen haben (in dem Sie einzelne Elektronen mit 4 Freiheitsgraden empfehlen), sondern überhaupt, ich triumphiere, dass auch Sie (et tu, Brute!) mit gesenktem Haupt ins Land der formalismusphilister zurückgekehrt sind; aber seien Sie nicht traurig. Sie werden doch mit offnen Armen empfangen. Und wenn Sie selbst meinen, etwas gegen die bisherigen Arten von Schwindel geschrieben zu haben, so ist das natürlich Missverständniss, denn Schwindel x Schwindel gibt nichts richtiges -- daher können sich zwei Schwindel nie wiedersprucher, also ich gratuliere!!!!! Fröhliche Weichnachten!!"

134. Ch. II, ref. 128, p.166.

135. RAMSAUER,C. [1922] AP 64 p.519, "Uber der Wirkungsquerschnitt der Gasmoleküle gegenüber langsamen Elektronen"; 66 p.546; [1923] 72 p.345.

136. DAVISSON,C. [1923] PR 21 p.637, "The scattering of electrons by a positive nucleus of limited field";
DAVISSON,C. & KUNSMANN,C.H. [1923] PR 22 p.242, "The scattering of low speed electrons by platinum and magnesium".

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137. Born to Bohr 1.1.25. SHQP.

138. BOHR,N. [1925] Speech to the Royal Danish Academy, reported in N 116.

139. Bohr to Heisenberg 18.4.25. SHQP:
"Besonders durch Gesprache mit Pauli angeregt, quäle ich mich in diesen Tagen nach besten Kräften, mich in die Mystik der Natur einzuleben und versuche, mich auf alle Eventualitäten vorzubereiten, ja sogar auf die Annahme einer Kopplung der Quantenprozesse in entfernten Atomen. Die Kosten dieser Annahme sind allerdings so gross, dass sie sich nicht in der gewohmlichen, raum-zeitlichen Beschreibung ermessen lassen."

140. Geiger to Bohr 17.4.25. SHQP.

141. Bohr to Geiger 21.4.25. SHQP, trans. STEUWER. [1975]

142. Ref. 139.

143. Bohr to Franck 21.4.25. SHQP:
"Ich hatte schon lange die Absicht, Ihnen wieder zu schreiben, denn der Zweifel an der Richtigkeit meiner Uberlegungen über die Stosserscheinungen, dem die Nachschrift in meinem letzten Brief Ausdruck gab, hat sich seitdem immer verstärkt. Es sind besonders die Ramsauerschein Ergebnisse der Durchdringung langsamer Elektronen durch Atome, die sich anscheinend dem angenommen Gesichtspunkte nicht einfügen. In der Tat dürften diese Ergebnisse unserer gewohnlichen raumzeitlichen Naturbeschreibung Schwierigkeiten ähnlicher Art darbieten wie eine Koppelung der Zustandsänderungen entfernter Atome durch Strahlung. Dann ist aber kein Grund mehr, an einer solchen Koppelung und an den Erhaltungssatzen uberhaupt zu zweifeln. Dies ist nur eine grosse Befriedigung denn, wie Sie hervorheben, wird ja dann so vieles bei den Stössen so ungemein viel einfacher. Auch waren die thermodynamischen Betrachtungen von Einstein ja sehr beunruhigend. Ich habe schon diesen Morgen an Fowler geschrieben, dass ich eine englische Arbeit über die Bremsung der α -strahlen zurückziehen und werde Ähnliches an Scheel betreffend die ihm zugesandte Arbeit schreiben. Ausserdem habe ich eben jetzt von Geiger gehört, dass seine Versuche für die Koppelung entschieden haben, und es ist wohl nichts anderes zu tun als unseren Revolutionsversuch möglichst schmerzlos in Vergessenheit zu bringen. Unsere Ziele werden wir aber doch nicht so leicht vergessen können und in den letzten Tagen habe ich mit allerlei wilden Spekulationen gequält, um eine adäquate Grundlage der Beschreibung der Strahlungsphänomene zu finden. Daruber habe ich viel mit Pauli diskutiert, der jetzt hier ist, und dem seit langem unser "Kopenhagener Pretsch" unsympatisch war."

144. Born to Bohr 24.4.25. SHQP.

145. BOSE,S.N. [1924] ZP 26 p.178, "Plancks Gesetz und Lichtquantenhypothese". See appendix J.

146. Appendix J.

147. EINSTEIN,A. [1924] BB p.261, "Quantentheorie des einatomigen idealen Gases", recd. 20.9.24. [1925] BB p.3, same title, recd. 9.2.25.

148. Ch. I, ref. 33.

149. Born to Bohr 24.4.25. SHQP:
"Die Hauptsache ist, dass, man das Wertvolle der Bohr-Kramers-Slaterschen Theorie beibehält: nämlich die Emission der Wellenstrahlen während der stationaren Zustande. Nur gibt es aber weben dies Zeitabschritten die Sprünge, die hier als bezuentanprozesse gelten mögen. Wenn man also in erhangt versucht, die Ordnung der Vorgange in Raum und Zeit vorzuordnen, so muss man den stationären Zuständen die Wellenemission, den Sprüngen die Lichtquantenemission zuordnen."

150. Born to Einstein 15.7.25. BEC.

151. According to the letter to Einstein, Born needed Ehrenfest to help him understand the thermodynamics (Bose-Einstein statistics), and then looked at de Broglie's work.

152. Ref. 147, [1925] p.9:
"ich glaube, dass, es sich dabei um mehr als um eine blosse Analogie handelt."

153. Bohr to Born 1.5.25. SHQP:

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"Der Inhalt Ihrer Note hat uns natürlich sehr interessiert, aber ich muss gestehen, dass ich nicht glaube, dass eine widerspruchsfreie Beschreibung der Phänomene sich in der vorgeschlagenen Weise erreichen lässt. Es scheint mir, dass nach Ihrem Bilde der Verband der Lichtquanten mit den Wellen ein nicht genügend enger ist. Einerseits sehe ich nicht ein, wie es sich nach Ihrer Vorstellung erreichen lässt, dass die Bahnen der Lichtquanten mit hinreichender Genauigkeit mit der Fortpflanzung der Wellen zusammenfallen. Wenn die Wechselwirkung zwischen einem Quant und einem streuenden Atom nur von dem klassisch berechneten Moment seiner virtu..llen Resonatorenabhängt, dürfte es ja kaum zu vermeiden sein, dass Quant z.b. bei Spiegelung oder Brechung vollkommen von dem ihm ursprünglich zugeordneten Wellenzug getrennt wird. Anderseits scheint mir, dass Ihr Bild kaum die quantitativen Verhältnisse der Lichtabsorption wiedergeben kann, denn die Annahme, dass die Wahrscheinlichkeit der Auffangung eines Lichtquants durch ein Atom mit der Intesität der Wellen proportional sein sollte, führt ja zu ganz anderen Gesetzmässigkeiten als es so ie der Wellentheorie wie der korpuskularen Theorie des Lichtes entsprechen würde. Die grobe Ubereinstimmung dieser zwei Theorien, soweit es die gradlinige Ausbreitung des Lichtes betrifft, beruht ja eben darauf, dass die Anzahl der Korpuskeln, die durch die Flächeneinheit geht uberall der Intensität der Wellen proportional ist, und dass also die Absorption durch die Annahme eines konstanten wirksamen Querschnitt der Atome beschrieben werden muss."

154. De Broglie's modification does not seem to have been caused by this objection, but it did overcome it.

155. Bohr to Born 1.5.25. SHQP:

"Ganz abgesehen von der Frage der Richtigkeit derartiger Einwände gegen Ihr Theorie, möchte ich gern betonen, dass ich der Ansicht bin, dass die Annahme einer Koppelung zwischen den Zustandänderungen in entfernten Atomen durch Strahlung einer einfache Beschreibungsmöglichkeit des Physikalischen Geschehens mittels anschaulicher Bilder ausschliesst. Mit meinen Äusserungen in den Brief an Franck uber die Koppelung war nur gemeint, dass ich den Verdacht bekommen hatte, dass schon für die Stosserscheinungen solchen Bildern ein noch geringeren Anwendbarkeit zukommt als gewöhnlich angenommen. Dies ist ja zunächst eine rein negativ Aussage, aber ich fühle, besonders wenn die Koppelung wirklich eine Tatsache sein sollte, dass man dann in noch hoheren Grade wie bisher seine Zuflucht zu symbolischen Analogien nehmen muss. Eben in letzter Zeit habe ich mir den Kopfe zerbrochen in solche Analogien mich hineinzuträumen."

156. Interviews with Franck, Born, Elsasser, [1962] SHQP. All agree on this story.

157. Hund, in Göttingen, had already tried to explain the results on the basis of electron shells in the atom with gaps between, this model giving a periodicity of the scattered rays with scattering angle as required.

158. ELSASSER, W. [1925] dN 13 p.711, "Bemerkung zur Quantenmechanik freier Elektronen".

159. I cannot see this in Heisenberg and Kramers, and can see only a suggestion of it in Born and Jordan.

160. I have been unable to find any direct influence of this upon the physics, but it is an interesting feature of the conceptual background.

161. Pauli to Bohr 11.2.24. SHQP:

"Wenn ich über seine Ideen nachdenke, so kommen sie mir grässlich vor und ich schimpfe innerlich sehr darüber. Denn er ist sehr unphilosophisch, er achtet nicht auf klar Herausarbeitung der Grundannahmen und ihren Zusammenhang mit den bisherigen Theorien. Wenn ich aber mit ihm spreche, so gefällt er mir sehr gut und ich sehe, er allerlei neue Argumente -- wenigstens im Herzen. Ich halte ihn dann -- abgesehen davon, dass er persönlich auch ein sehr netter Mensch ist -- für sehr bedenkend, sogar für genial und glaube dass er die Wissenschaft noch einmal sehr vorwärts bringen wird."

162. See below

163. Ref. 26.

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164. HEISENBERG,W. [1925] ZP 33 p.879, "Uber quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen", recd. 29.7.25; trans. in WAERDEN [1967] to which all references will be made.

165. Ibid., p.261.

166. Ibid., p.261.

167. Ibid., p.262.

168. Though without using this terminology.

169. The above outline of Heisenberg's paper has been kept very brief. For a fuller discussio see WAERDEN [1967].

170. KUHN,W. [1925] ZP 33 p.408, "Uber die Gesamstärke der von einem Zustande ausgehenden Absorptionslinien", recd. 14.5.25;
THOMAS,W. [1925] dN 13 p.627, "Uber die Zahl der Dispersinselektronen, die einem stationären Zustande zugeordnet sind."

171. Ref. 26.

172. Heisenberg to Pauli 9.7.25. SHQP:
"mein ganzen Kümmerlichen Bestrebungen gehen dahin, den Begriff den Bahnen, die man doch nicht beobachtet kann, restlos auszubringen und geeignet zu ersetzen."

173. Heisenberg to Kronig 5.6.25. in KRONIG [1960]:

174. Ibid., p.25:
"dass man wirklich alle Wechselwirkungen zwischen Atom und Ausserwelt dann auf die Ubergangswahrscheinlichkeiten reduzieren kann"

175. Ibid., p.25:
"Der Grundendanke ist: In der klassischen Theorie genügt die Kenntnis der Fourierreihe der Bewegung im alles auszurechnen."

176. Heisenberg's multiplication also appears in the Kronig letter. Heisenberg recalled that he first came across the multiplication problem when he was still concerned with the hydrogen atom.

177. Heisenberg to Pauli 24.6.25. SHQP:
"Uber mein eigenen Arbeiten habe ich fast keine Lust zu schreiben, weil mir selbst alles noch unklar ist und ich nur ungefähr ohne, wie es werden wird, aber vielleicht sind die Grungedanken doch richtig. Grunsatz ist: Bei der Berechnung von irgendwelchen Grössen, als Energie, Frequenz u.s.w. dürfen nur Beziehungen zwischen prinzipiel kontrollierbaren Grössen vorkommen."

178. Heisenberg to Pauli 24.6.25. SHQP is a thankyou letter.
"Die physikalische Bedeutung des obengenannten Schemas zu Berechnung der Intensitaten gibt auch wieder sehr sonderbare Gesichtspunkle."

179. There is internal evidence to support this also. In the first part of the paper, Heisenberg used the notation $\mathfrak{a}$ for the Fourier amplitudes, switching the notation later to a ; the latter was related to the coordinate amplitudes and the former, apparently, to the electrical moments of the virtual oscillators. Although the two are closely related ($\mathfrak{a} \sim -ea$), the a which also appear in the letters to Kronig and Pauli, are not directly observable, suggesting that the criterion was not important when he worked out the applications of the theory (in the letters and later in the paper); the $\mathfrak{a}$, however, which appear for the first time in the paper, are directly observable, and may heve been introduced only after the scheme was worked out, when he sought a physical interpretation of it.

180. Ref. 173, p.25:

181. Heisenberg to Pauli 24.6.25. SHQP, trans. CASSIDY

182. Heisenberg to Pauli 24.6.25. SHQP:
"Auch ich würde gerne verstehen, was eigentlich die Bewegungsgleichungen bedeuten, wenn man sie als Relation zwischen den Ubergangswahrscheinlichkeiten auffasst."

183. Ref. 173, p.27:
"ich bin im Herzen wieder uberzeugt, dass diese Quantenmechanik schon richtig ist".

184. Heisenberg to Pauli 9.7.25. SHQP:
"Es ist wirklich meine Uberzeugung, dass eine Interpretation der Rydberg-Formel im Sinne von Freis und Ellipsenbahnen in klassischer Geometrie nicht den geringsten physikalischen Sinn hat."

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185. Ibid.:
"von Tag zu Tag radikaler".

186. Heisenberg to Bohr 18.5.25. SHQP, trans. CASSIDY [1976].

187. RAMAN & FORMAN [1969]. They take the view that the Göttingen physicists were totally opposed to de Broglie's ideas, and do not even consider Elsasser's work -- a curious omission.

188. Schrödinger to Landé 16.11.25, SHQP:
"Ganz besonders freut mich ihre Mitteilung, dass Ihre Arbeit ein "Zurück zur Wellentheorie" sein sollte. Auch ich neige sehr dazu. Ich habe mich dieser Tage stark mit Louis de Broglies geistvollen Theses beschäftigt. Ist ausserordentlich anregend, hat aber doch noch sehr grosse Härten. "
RAMAN & FORMAN [1969] discuss Landé's work briefly.

189. HANLE [1977].

190. These relationships are discussed in RAMAN & FORMAN [1969]. Schrödinger's notebooks on Hamilton's analogy are in SHQP, "Tensoranalytische Mechanik", and the parallel with de Broglie's work is to be found in
SCHRODINGER,E. [1922] ZP 12 p.13, "Uber eine bemerkenswerte Eigenschaft der Quantenbahnen eines einzelnen Elektrons".

191. See appendix H.

192. SCHRODINGER,E. [1926] AP 79 p.361, "Quantisierung als Eigenwertproblem", recd. 27.1.26. ; rep. in DdN 4; trans. in Collected papers on wave mechanics, to which all references will be made. This quotation from p.9.

193. Ibid.

194. Perhaps because of Schrödinger's emphasis on the classical nature of his theory, this obvious point has not apparently been grasped by historians.

195. Ref. 192, p.9.

196. SCHRÖDINGER,E. [1926] AP 79 p.489, title, reprints, translation and references made as in ref.192; recd. 23.2.26. Quotation from p.13, with ammendments: I have ammended the translation to agree more closely with the original German (AP , p.489). The official English translation omits the words "intrinsic" and "spiritual". The latter omission is particularly important, for Hamilton's work was largely based upon spiritual (philosophical, religious) analogies. This means that those who study it often see it as far more important than would be expected from purely technical considerations, and become 'disciples' of Hamilton. This, I suggest, is what happened to Schrödinger, and is part of the explanation for his delight in de Broglie's work. As for the translation, it is possible that the spiritual side of science was thought to be less appealing to English than to German readers.

197. In analogy with the image points in the wave optics of Debye and von Laue, with whom Schrödinger was working.
DEBYE,P. [1909] AP 30 p.755,
LAUE, M. von [1914] AP 44 p.1197.

198. SCHRODINGER,E. [1926] AP 79 p.734, "Uber das Verhältnis der Heisenberg-Born- Jordanschen Quantenmechanik zu der meinen", recd. 18.3.26, reps., trans. and refs. as in ref.192.
The third communication of the theory itself was
SCHRODINGER,E. [1926] AP 80 p.437, recd. 10.5.26, reps., trans., refs. and title as ref.192.

199. SCHRODINGER,E. [1926] AP 81 p.109, recd. 21.6.26, reps., trans., refs. and title as ref.192. Quote from p.120.

200. This is not recognised, for example, by JAMMER [1966], who treats Schrödinger's work as substantially original.

201. To correct another misapprehension, we might also note that de Broglie was not, and has not been since, seeking a classical explanation of quantum phenomena.

202. Ref. 198.

203. It is also interesting to note that both the wave mechanics and the matrix mechanics use in a sense 'devalued' versions of Hamilton's ideas. Schrodinger sought a wave mechanics rather than one combining wave and particle aspects on equal terms, while the matrices are much less potent than the quaternions from which they were derived. In search of a more satisfactory theory than exists at present, Born and Dirac have both suggested the use of quaternions, while these are the basic elements of both Eddington'd fundamental theory and Sachs' unified field theory.

204. For Schrödinger on Heisenberg, ref. 198 (p.735 of AP).
For Heisenberg on Schrödinger,
HEISENBERG,W. [1926] ZP 38 p.411, "Mehrkörperproblems und Resonanz in der Quantenmechanik".

IV

NOTES TO CHAPTER IV

1. BORN,M. [1948] Recollections, SHQP and WAERDEN [1967] p.36. Pauli had returned Heisenberg's draft within a few days, with favourable comments, and Heisenberg had then sent it to Born, who did not read it for a few days because of tiredness.

2. Ibid. p.21. Born claimed that Jordan confirmed this recollection.

3. Ch. III, ref.164.

4. Or rather, at this stage, an anharmonic oscillator.

5. Ref.1.

6. COURANT,R. & HILBERT,D. [1924] Methoden der mathematischen Physik, v.1, Springer, Berlin.

7. Ref.1.

8. Ref.1., SHQP only.

9. Jordan to Waerden, 1964 , WAERDEN [1967] p.40 : "eine etwas strapaziöse Sanatoriums-Kur".

10. BORN,M. & JORDAN,P. [1925] ZP 34 p.858, "Zur Quantenmechanik"; abridged trans. in WAERDEN [1967], to which all references will be made unless otherwise noted; recd. 27.9.25.

11. This tied in with the usual definition of the 'mean' of a matrix as its diagonal sum.

12. This was defined by $\frac{\partial y}{\partial x_k} = \sum_{r=1}^{s} \delta_{l_r k} \prod_{m=r+1}^{s} x_{l_m} \prod_{n=1}^{r-1} x_{l_n}$, for $y = \prod_{m=1}^{s} x_{l_m}$.

13. $h = 2\pi i \sum \left[p(n, n-\alpha)\, q(n-\alpha, n) - q(n, n+\alpha)\, p(n+\alpha, n) \right]$.

14. In the course of establishing the invariance of the formula $\dot{g} = a$ with respect to permutations, the formula $\dot{g} = \frac{2\pi i}{h}(Wg - gW)$ was achieved. The substitution $g = t$ would have given as a special case $\frac{h}{2\pi i} = Wt - tW$, the energy-time commutation relationship. Born cursed himself for having missed this many months later, apparently unaware that he had been sitting on it since this early paper. Jammer also seems to have missed it here.

15. Ref.10 (ZP)p.883:
"Wir möchte hier zum Schluss noch ausführen, in welcher Weise diese Annahme aus allgemeineren Uberlegungen heraus eine Begründung erhalten kann. Notwendig ist dazu ein Eingehen auf die Frage, wie die Grundgleichungen der Elektrodynamik im Sinne der neuen Theorie umzudeuten sind."

16. Heisenberg had travelled to Leyden and Cambridge after submitting his own paper, then returned 'home' to Munich where Born either visited him or wrote to him, telling him of the progress being made. On August 20, after Born had gone to be cured, Heisenberg wrote to Jordan enclosing a copy of his paper and requesting " a little of your work" (Heisenberg to Jordan 20.8.25 SHQP : "über Ihre Rechnungen einiges zu erfahren"). Jordan evidently replied, but inadequately for on September 10 Heisenberg wrote again, repeating his request (Heisenberg to Jordan 10.9.25 SHQP). Jordan quickly obliged, and on September 13 Heisenberg sent his thanks and comments (Heisenberg to Jordan 13.9.25 SHQP; these letters are all reviewed in WAERDEN [1967]).

17. Heisenberg to Pauli 18.9.25 SHQP, and see WAERDEN [1967].

18. Heisenberg to Jordan 13.9.25 SHQP, " " " .

19. Before Heisenberg to Jordan 29.9.25 SHQP: " .

20. Heisenberg to Jordan 29.9.25 , and see WAERDEN [1967]; Heisenberg to Pauli 12.10.25 , " " " : $S = e^{\theta}$.

21. BORN,M., HEISENBERG,W. & JORDAN,P. [1925] ZP 35 p.557, "Zur Quantenmechanik II", trans. in WAERDEN [1967], to which all references will be made; recd. 16.11.25.

22. See Heisenberg to Pauli 3.11.25, in HEISENBERG [1960].

23. Heisenberg to Pauli 9.7.25 SHQP & WAERDEN [1967]:
"Ich von dem negativen heuristischen Teil fest überzeugt bin, dass ich aber den positiven für reichlich formal und dürftig halte."

24. Heisenberg to Pauli 9.7.25 SHQP:
"Wir sind doch wohl einig, dass schon die Kinematik der Quantentheorie vollständig anders ist, als die klassische".

25. Pauli to Kramers 27.7.25 SHQP:
"Uberhaupt glaube ich, dass ich jetzt aussichtlich meinen wissenschaftlichen Ansichten Heisenberg sehr nahe gekommen bin, und dass wir ziemlich in allem übereinstimmende Meinungen haben, soweil dies überhaupt bei zwei selbständig denkenden Menschen möglich ist."
But he thought that Heisenberg was still far from saying anything.

26. Ref. 1, p.37.

27. Pauli to Kronig 9.10.25 SHQP:
"Man muss zunächst versuchen, die Heisenbergsche Mechanik noch etwas mehr vom Göttingen formalen Gelehrsamkeitsschwall zu befreien und ihren physikalischen Kern noch besser blosszulegen."
See also ref.25, and Einstein to Besso 25.12.25 EBC:
"[The matrix mechanics is] highly rigorous and sufficiently protected by great complexity against all proofs of invalidity."

28. Heisenberg to Pauli 15.11.25 SHQP:
"Ich habe mir alle Mühe gegeben, die Arbeit physikalischer zu machen, als Sie war und ich bin so halb zufrieden damit. Aber ich bin immer noch ziemlich unglücklich uber die ganze Theorie und war so froh, dass Sie micht der Ansicht uber Mathematik und Physik so ganz auf meiner Seite stehen. Hier bin ich in einer umgebung, die genau entgegengesetzt denkt und fühlt und ich weiss nicht, ob ich nur zu dumm bin, um Mathematik zu verstehen."

29. Heisenberg suggested $\frac{\partial f}{\partial x_k} = \lim_{\alpha \to 0} \frac{f(x_1, \dots x_k + \alpha, \dots) - f(x_1, \dots, x_k, \dots)}{\alpha}$
and both forms were included in the paper. In fact the authors pointed out that differentiation was unnatural in the quantum context anyway: once the key to the translation of the classical equations of motion into quantum language had been established, differentiation was completely replaced by commutators.

30. Heisenberg to Pauli 16.11.25 SHQP:
"Göttingen zerfällt in zwei Lager, die einem, die wie Hilbert (oder auch Weyl in einem Brief an Jordan) von dem grossen Erfolg reden, der durch die Einführung der Matrizenrechnung in die Physik errungen sei, die anderen, die, wie Franck, sagen, dass man die Matrizen doch nie verstehen könne."

31. The book by Courant and Hilbert linked matrices clearly with bilinear forms, and although it did not cover Hermitian forms in the first edition, it did cover real symmetric forms. The two forms are however quite analogous, and Born's choice was a natural consequence of his consideration of Hermitian matrices.

32. Ref.21, p.351.

33. See Ch. II.

34. Ref. 21, p.363.

35. More generally, $\sum_k W_n y_n y_n^* + \int W(\varphi)\, y(\varphi)\, y^*(\varphi)\, d\varphi$.

36. Ref. 21, p.358; HELLINGER,E. [1910] CJ 136 p.1.

37. Although Wiener had visited Göttingen in 1924, there is no suggestion that his work on operators was known there.

38. WIENER [1956] p.108.

39. WIENER,N. [1926] MA 95, "The operational calculus", recd.6.4.25.

40. PINCHERLE,S. [1904-6] Encyclopaedie der mathematischen Wissenschaften vol 2, p.763.

41. HEAVISIDE,O. [1899] London, Electromagnetic theory; rep. Dover [1950].

42. Ref. 21, p.363.

43. BORN,M & WIENER,N. [1926] JMP 5 p.84, "A new formulation of the laws of quantisation of periodic and aperiodic phenomena"; ZP 36 p.174, "Eine neue Formulierung der Quantengesetze für periodische und nicht periodische Vorgänge". All references to the English version, though the German is generally clearer. Quote from p.84.

IV

44. A matrix product being equivalent to two successive operators.

45. Ref. 43, p.84.

46. The summed expression for $q(t,s)$ does not in general converge, but they ignored that problem.

47. We might note one aspect of the history of operator theory. This had been developed in Germany, mainly by Hilbert and his students, in connection with integral equation theory to whcih the method of infinite matrices had been applied by Fredholm. Infinite matrices were primary to this research, operators only secondary, but Born found himself forced to drop the matrices and work only with operators, a course that was to be followed in the future development of quantum mechanics. It was not until 1929 that von Neumann, turning back from quantum mechanics to pure mathematics, found that although infinite matrix theory and operator theory were equivalent fro finite-dimensional spaces, and matrices could also be used for bounded operators on Hilbert spaces, they could not be used for operators unbounded on a Hilbert space, i.e. in the exact conditions of quantum mechanics:
NEUMANN,J.von [1929] MA 102 p.49, "Allgemeine Eigenwerte theorie Hermetischer Funktionsoperatoren."

48. Ref.43, p.86.

49. Manuscript in Dirac archive

50. Ibid.

51. Dirac noted that the energy transfer of an oscillator $A\cos\omega t + iB\sin\omega t$ was $A^2 - B^2$, or in the case of an oscillator $Ce^{i\omega t}$, zero. Thus to get any energy transfer at all, one needed a combination of $e^{i\omega t}$ and $e^{-i\omega t}$ oscillators.

52. The following analysis is my own, and due to neither Born, Wiener, nor Dirac.

53. Born once again missed the energy-time commutation relationship, which he thought would in this context have led him to Schrödinger's theory,

$$\frac{h}{2\pi i} = pq - qp \quad ; \quad \frac{h}{2\pi i} = Wt - tW \quad ; \quad H = \frac{h}{2\pi i}\,\partial/\partial t$$

leading to $p = \frac{h}{2\pi i}\,\partial/\partial q$, the crucial link between the wave and matrix mechanics, through the operator equation $(\partial/\partial q)q - q(\partial/\partial q) = 1$.

54. BORN,M. [1926] ZP 37 p.863, "Zur Quantenmechanik der Stossvorgänge", recd. 25.6.26; rep in DdN 1, to which all references will be made. Quote from p.49:
"Von den verschiedenen Formen der Theorie hat sich hierbei allein die Schrodingersche als geeignet erweisen, und ich mochte gerade aus diesem Grunde die als die tiefste Fassung der Quantengesetze ansehen"

55. Ibid., p.48:
"Viele nehmen an, dass das Problem der Ubergänge von der Quantenmechanik in der vorliegenden Form nicht erfasst wird, sondern dass hier neue Begriffsbildungen nötig sein werden. Ich selbst kam durch den Eindruck der Geschlossenheit des logischen Aufbaues der Quantenmechanik zu der Vermutung, dass diese Theorie vollständig sein und das Ubergangsproblem mit enthalten müsse."

56. Ibid., p.50:
"Das bedeutet: die Störung lässt sich im Unendlichen auffassen als als Superposition von Lösungen des ungestörten Vorgangs."

57. Ibid., p.50:
"Will man nun dieses Resultat korpuskular umdeuten, so ist nur eine Interpretation möglich: $\Phi_{\tau nm}(\alpha,\beta,\gamma)$ bestimmt die Wahrscheinlichkeit dafür, dass das aus der z-Richtung kommende Elektron in die durch α,β,γ bestimmte Richtung (und mit Phasenänderung δ) geworden wird, wobei seine Energie τ um ein Quant $h\nu^{0}_{nm}$ auf Kosten der Atomenergie zugenommen hat. ...

"Die Schrödingersche Quantenmechanik gibt also auf die Frage nach dem Effekt eines Zusammenstosses eine ganz bestimmte Antwort; aber es handelt sich um keine Kausalbeziehung. Man bekommt keine Antwort auf die Frage, "wie ist der Zustand nach dem Zusammenstosse", sondem nur auf die Frage, "wie wahrscheinlich ist ein vorgegebener Effekt des Zusammenstosses" (wobei naturlich der quantenmechanische Energiesatz gewahrt sein muss."

IV

"Hier erhebt sich die ganze Problematik des Determinismus. Vom Standpunkt unserer Quantenmechanik gibt es keine Grösse, die im Einzelfalle den Effekt eines Stosses kausal festlegt; aber auch in der Erfahrung haben wir bisher keinen Anhaltspunkt dafür, dass es innere Eigenschaften der Atome gibt die einen bestimmten Stosserfolg bedingen. Sollen wir hoffen, später solche Eigenschaften (etwa Phasen der inneren Atombewegungen) zu entdecken und im Einzelfalle zu bestimmen? Oder sollen wir glauben, dass die Ubereinstimmung von Theorie und Erfahrung in der Unfähigkeit, Bedingungen für den kausalen Ablauf anzugeben, eine prästabilierte Harmonie ist, die auf der Nichtexistenz solche Bedingungen berüht? Ich selbe neige dazu, die Determiniertheit in der atomaren Welt aufzugeben. Aber das ist eine philosophische Frage, fur die physikalische Argumente nicht allein massgebend sind."

58. BORN,M. [1926] ZP 38 p.803, "Quantenmechanik des Stossvorgänge", recd. 21.7.26; rep. Ddn 1, to which all references will be made.

59. Ibid., p.54:
"Er sagte etwa, dass die Wellen nur dazu da seien, um den korpuskularen Lichtquanten den Weg zu weisen, und er sprach in diesem Sinne von einem "Gespensterfeld"."

60. Ibid., p.76:
"Uberdies ermöglicht sie die Beibehaltung der gewöhnlichen Vorstellungen von Raum und Zeit ".

61. Ibid., p.76.

62. BORN,M. [1926] N 119 p.354, "Physical aspects of quantem mechanics", pub. 10.8.26.

63. A fourth paper clarified and expanded the theory, but added nothing new: BORN,M. [1927] ZP 40 p.167, "Das Adiabatenprinzip in der Quantenmechanik", recd. 16.10.26; rep. DdN 1. See also [1927] GN p.146, "Zur Wellenmechanik der Stossvorgänge."

64. Ref.60.

65. This was finally proved by Heisenberg, but was clear to Born immediately; HEISENBERG,W. [1927] ZP 43 p.172, "Uber den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik", recd. 23.3.27.

66. Ref.57.

67. BORN [1961] p.103.

68. BORN [1943] p.23.

69. See Ch. III.

70. Ref. 58, p.54:
"Die Bewegung der Partikeln folgt Wahrscheinlichkeitsgesetzen, die Wahrscheinlichkeit selbst aber breitet sich im Einklang mit dem Kausalgesetz aus."

71. Ref 59.

72. Ref.1 (SHQP), and Einstein to Born 6.12.26, BEC.

73. Ref.58, p.54:
"der vollständigen Analogie zwischen Lichtquant und Elektron".

74. Ch. III, ref.32.

75. See Ch.II.

76. FORMAN [1971]: see appendix H.

77. See BEC.

78. Ref.63 (ZP).

79. Ibid.

80. See FORMAN [1971] for references.

81. Weyl to Jordan 13.11.25, 23.11.25, 25.11.25 SHQP. For Weyl's views on the subject see Appendix H; for his relationship to the Göttingen mathematicians, see interview with Courant [1962] SHQP.

82. Ch.II, ref.112.

83. Having first insisted that the interpretation followed from the formalism Born later talked, according to Heisenberg (Ch.III, ref.26) of "my interpretation", and got very annoyed if anyone suggested that it did follow from the formalism.

84. JORDAN,P. [1926] ZP 37 p.383, "Uber kanonische Transformationen in der Quantenmechnik", recd. 27.4.26.

85. LONDON,F. [1927] ZP 40 p.193, "Winkelvariable und kanonische Transformationen in der Undulationsmechanik", recd. 19.9.27.

86. LANCZOS,K. [1926] ZP 35 p.812, "Uber eine feldmässige Darstellung der neuen Quantenmechanik", recd. 22.12.25.

87. Interview with Dirac [1962] SHQP.

88. Ibid.; he chose this in preference to "algebraic".

89. Ibid.

90. DIRAC,P.A.M. [1925] PRS A109 p.642, "The fundamental equations of quantum mechanics", recd. 7.11.25; rep. WAERDEN [1967].

91. DIRAC,P.A.M. [1926] PRS A110 p.561, "Quantum mechanics and a preliminary investigation of the hydrogen atom", recd. 22.1.26; rep. WAERDEN [1967], to which all references will be made.

92. Ibid., p.418.

93. Ibid., p.418.

94. Ibid., p.421

95. For a system $H(J)$, $\dot{x} = [x, H]$, with canonical variables J, ω, functions of multiply periodic p, q, he had in analogy with the classical theory $\dot{\omega} = [\omega, H] = \partial H/\partial J$. The ω were q-numbers, related to orbital frequencies, but he found that the q-number expressions $(\alpha\omega)h = H(J) - H(J - \alpha h)$ also related to frequencies, for $\frac{d}{dt} e^{i(\alpha\omega)} = i(\alpha\omega) e^{i(\alpha\omega)}$. These transition frequencies were therefore a natural part of the theory: to compare them with experiment, he found that the substitution $J = nh$ satisfied the requirements for a c-number representation,

$$(\alpha\omega)(J) = \omega(J, J - \alpha h) \rightarrow \omega(nh, nh - \alpha h).$$

96. DIRAC,P.A.M. [1926] PRS A111 p.281, "The elimination of nodes in quantum mechanics", recd. 27.3.26; p.405, "Relativity quantum mechanics with an approximation to Compton scattering", recd. 29.4.26.

97. DIRAC,P.A.M. [1927] PRS A113 p.621, "The physical interpretation of the quantum dynamics", recd. 2.12.26.

98. JORDAN,P. [1927] ZP 40 p.809, "Ubér eine neue Begrundung der Quantenmechanik", recd. 18.12.26.

99. Ref.97, p.621

100. Ibid., p.641.

101. He liked to give similar quantities similar symbols.

102. Dirac to Jordan 24.12.26. SHQP.

103. Ibid.

104. Ibid.

105. Ibid.

106. Ibid.

107. Ref.97.

108. Ref.98, p.810:

"Stall der p, q mögen durch eine kanonische Transformation neue Veränderliche P, Q eingeführt werden, wobei $H(pq) = \bar{H}(P, Q)$ werden möge. Dann wollen wir mit $\bar{H}$ die neue Wellengleichung ... bilden.

"Wie verhalten sich diese $\psi(x)$ zu der ursprünglichen Funktion $\varphi(y)$?"

109. Ibid., p.811:

"Ist $\varphi_n(q)$ normiert, so gibt $|\varphi_n(q)|^2 dq$ die Wahrscheinlichkeit an, dass wenn des System sich in Zustand n befindet, die Koordinate q einen Wert im Intervall $q, q + dq$ besitzt. ...

"Pauli hat folgende Verallgemeinerung im Auge gefasst: seien q, β zwei hermetische quantenmechanische Grössen, die wir hier her Bequemlichkeit halber beide als stetig veränderlich annehmen wollen; dann wird es stets eine Funktion $\varphi(q, \beta)$ geben, derart, dass $|\varphi(q_0, \beta_0)|^2 dq$ die (relative) Wahrscheinlichkeit misst, dass bei gegebenem Zahlwert β_0 von β die Grösse q einem Zahlwert im Intervall $q_0, q_0 + dq$ besitzt. Die Funktion $\varphi(q, \beta)$ wird von Pauli als Wahrscheinlichkeitsamplitude bezeichnet."

IV

110. Putting $Q \to \rho$ we can reciver the Dirac -function.

111. Dirac to Jordan 24.12.26. SHQP .

112. Ibid.

113. This was the same group as had begun the struggle towards a quantum mechanics at the beginning of the decade, minus Kramers (now in Utrecht) and plus Dirac and Jordan.

114. HEISENBERG,W. [1927] ZP 43 p.172, "Uber den anschaulichen Inhalt der quantentheoretisch. Kinematik und Mechanik".

115. I have not described this, as it is very well known: an account is given in JAMMER [1966].

116. Ref.114 ,p.174:
"Wenn man sich darüber klar werden will, was unter dem Worte 'Ort des Gegenstundes', z.B. des Elektrons ..., zu verstehen sei, so muss man bestimmte Experimente angeben, mit deren Hilfe man den 'Ort des Elektrons' zu messen gedanket; ander hat dieses Wort keinen Sinn."

117. Ibid., p.172:
"Diese Ungenauigkeit ist der eigentliche Grund für das Auftreten statistisch Zusammenhänge in der Quantenmechanik."

118. As we shall see later, Heisenberg's position is in fact more subtle than this.

119. Ref.114, p.197:
"Aber solche Spekulationen scheinen uns, das betonen wir aus drucklich, unfruchtbar und sinnloss. Die Physik soll nur den Zusammenhang der Wahrnehmungen formal beschreiben.

"Dass die Quantentheorie in Gegensatz zur klassischen eine wesentlich statistische Theorie sei in dem Sinne, dass aus exakt gegebene Daten nur statistische Schlüsse gezogen werden könnten, haben wir nicht angenommen. ... Aber an der scharfen Formulierung des Kausalgesetzes:"Wenn wir die Gegenwert genau kennen, können wir die Zukunft brechnen", ist nicht der Nachsatz, sondern die Voraussetzung falschen."

120. Ibid.

121. This is not the popular impression of Born's interpretation, according to which he was concerned only with our knowledge of the system.

122. Jordan also attacked Schrödinger elsewhere, and Born had to apologise for him: Born to Schrödinger 16.5.27 SHQP. This attack was in Jordan to Schrödinger May 1926 SHQP.

123. Ch.III, ref.26.

124. Ref.1.(SHQP).

125. Born to Schrödinger 16.5.27. SHQP:
"Heisenberg war von vornherein nicht meiner Meinung, dass Ihre Wellenmechanik physikalisch mehr bedeute, als unsere Quantenmechanik."

126. Heisenberg to Jordan 28.7.26 SHQP, trans. CASSIDY. [1976]
HEISENBERG,W. [1926] ZP 38, p.411, "Mehrkorperproblem und Resonanz in der Quantenmechanik", recd. 11.6.26, saw the best hope in a compromise between the two theories.

127. Bohr to Kronig 28.10.26. SHQP.

128. Ch.III, ref.26.

129. Heisenberg to Dirac 26.5.26. SHQP.

130. Heisenberg to Pauli 8.6.26. SHQP. The spin concept had been introduced by UHLENBECK,G.E. & GOUDSMIT,S. [1925] dN 13 p.953, "Ersetzung der Hypothese vom unmechanischen Zwang durch eine Forderung bezüglich des inneren Verhaltens jedes einzelnen Elektrons". Pauli and Heisenberg did not accept the concept at first, but did so between 26.3.26 and 5.4.26: Bohr to Kramers, and Heisenberg to Bohr respectively, SHQP.

131. FERMI,E. [1926] ZP 36 p.902, "Zur Quantelung des idealen einatomigen Gases", recd.24.3.26, pubd.11.5.26.

132. Ref. 126 (ZP).

133. DIRAC,P.A.M. [1926] PRS A112 p.261, "On the theory of quantum mechanics", recd. 26.8.26.

134. Ref.131, p.902:

"In der vorliegended Arbeit wird nur die von Pauli zuerst ausgesprochene und auf zahlreihe spektroskopische Tatsachen begrunerte Annahme berutzt, dass in einem System nie zwei gleichwertige Elemente vorkommen können, deren Quantenzahlen vollständig übereinstimmen. Mit diesen Hypothese werden die Zustandsgleichung und die innere Energie des idealen Gases abgeleitet."

135. Ref. 126, p.422:
"Wenn nur das eine der beiden Systeme in der Natur vorkommt, so gibt dies einerseits zu einer Reduktion der statistischen Gewichte eben in dem von Bose vorgeschlagenen Sinne Anloss; andererseits ist bei der richtigen wahl des Systems Pauli's Verbot äquivälenter Bahnen von selbst erfühlt."

136. Ref.133, p.667.

137. Ibid., p.662.

138. I shall refer to the combination of the exclusion principle and Bose-Einstein statistics as the generalised Pauli (or exclusion) principle.

139. Heisenber to Pauli 15.11.26.

140. Not of course that distinguishibility could be satisfactorily understood, even in this way.

141. See Appendix K.

142. HEISENBERG [1960].

143. PAULI,W. [1927] ZP 41, p.81, "Uber Gasentartung und Paramagnetismus", recd. 16.12.26.

144. Heisenberg to Pauli 28.10.26 SHQP.

145. Heisenberg to Pauli 4.11.26. SHQP:
"Aber nach dieser unerfreulichen Einleitung möcht ich Ihnen andern mal schreiben, dass ich über den Inhalt Ihres letzten Briefs mehr und mehr begeistend bin; je mehr ich darüber nachdenke. Man soll also allgemein sagen: jedes Scheme, das $\rho q - q\rho = h/2\pi i$ erfühlt, ist "richtig" und physikalische brauchbar. Dabei hat man vollig freie Wahl, wie man diese Gleichung erfühlt, 'Matrizen', Operatoren oder irgendetwas anderes. Ubrigens Ihre Wellenfunktion χ im p-Raum scheint mir die Laplacesche Transformierte der Schröd. Funktion ψ im q-Raum zu sein. ... Aber Ihre Funktionen in anderen Räumen, z.B. J u. ω sind natürlich etwas anderes. Das Problem: kanonische Transformationen in der Wellenvorstell ist damit ja wohl auch gelöst!"

146. HEISENBERG,W. [1927] ZP 40 p.501, "Schwankungserscheinungen und Quantenmechanik", recd. 6.11.26.

147. Ref. 87. Dirac described his theory as a development of Lanczos' work, but this seems to have been excessively modest.

148. Heisenberg to Pauli 28.10.26. SHQP.

149. JORDAN,P. [1927] N 119 p.566, "Philosophical foundation of quantum theory", pubd. 16.4.27; see also p.779; dN 15 p.105, "Kausalität und Statistisch in der modernen Physik", recd. 5.2.27.

150. Ibid.(N).

151. We might note that Jordan attributed the idea that and were not simultaneously observable to Pauli, rather than to Dirac or Heisenberg.

152. HEISENBERG [1971].

153. Heisenberg to Pauli 28.10.26. SHQP.

154. Heisenberg to Pauli 15.11.26. SHQP:
"Die allgemeine Zelleneinteilung des Phasenraumes in Zellen irgendwelcher Volumina h ist zweifellos ein richtiges Prinzip. Aber, und nun kommt das Bedenken, das ich habe, "wenn Sie die Wände den Zellen scharf vorgeben, und doch bestimmen können, wie viele Partikel in jeder Zelle sind, kann man dann, durch Wahl der Zellwände benarchbart, nicht die Anzahl der Atome in beliebig kleinen Zellen finden? ... Ich meine, ist die Wahl bestimmter Zellwände physikalisch sinnvoll?"

155. Ibid.:
"Vielleicht ist est so, dass man nur z.B. das Verhältnis der beiden Zellwände a/b vorgeben kann, nicht aber die Lage einer bestimmten Zellwand."

IV

156. Ibid.:
"Der gleiche Einwand gibt nun auch für die E,t Einteilung. Für den Spezialfall bestimmtes t ist aber wieder alles in Ordnung. Ich halte es für sehr vernünftig, eben diesem Spezialfall näher zu studieren, vielleicht bekommt man etwas wohl die kinematische Bedeutung der Matrizen heraus. Schliesslich bin ich auch Ihren Meinung, dass am Schluss der dunkle Punkt ein sehr lichter Punkt sein wird. Ich meine: wenn schon einmal Raum-Zeit irgendwie diskontinuierlich sind, so ist es doch sehr befriedigend, dass es keinen Sinn hat, von zB Geschwindigkeit $\dot{x}$ in einem bestimmten Punkt x zusprechen. Denn im Geschwindigkeit zu definieren, braucht man durch mindestens 2 Punkte, die in einer Diskontinuumsvelt aber nicht unendlich benachbart liegen können. Wenn wir von Ort, oder Geschwindigkeit reden, brauchen wir eben einmal ..., die in einer Diskontinuumsvelt offenbar genünft anständig definiert sind."

157. Heisenberg to Pauli 5.2.27. SHQP:
"beschräftigt ich mir immer mal wieder mit den logischen Grundlagen des ganzen pq - qp Schwindels."

158. Ibid.:
"wenn der Begriff "Ort des Elektrons" nicht nicht anständig definiert wird."

159. Ch.III, ref.26.

160. Since the development of transformation theory. He may well have needed some time on his own to absorb this, as well as to think about the physical problem.

161. Heisenberg to Pauli 23.2.27. SHQP.

162. Ibid.:
"Was versteht man unter dem Wort "Ort des Elektrons"? Diese Frage wird nach bekannten Muster durch die andere zu ersetzen sein: "Wie bestimmt man den Ort des Elektrons?""

163. Ref.114 (quoted as being slightly clearer than, though essentially the same as, the letter) p.172:
"Im Augenblick der Ortsbestimmung, also denn Augenblick, in dem das Lichtquant vom Elektron abgebeugt wird, verändert des Elektron seinen Impuls vorstetig. Diese Ardenz ist nun so grössen, je kleinen die Wellenlänge des benutzten lichtes, d.h. je genauer die Ortsbestimmung ist. In dem Moment, in dem das Ort des Elektrons bekannt ist, kamm daher sein Impuls nur bis auf Grössen, die jenen unstetigen Ardenz entsprechnen, bekommt sein."

164. Ref.161:
"Also hat es keinen Sinn, über sagen wir, zB die 1S-"Bahn" des Elektrons im Wasserstoffatom zu sprechen. ... Das Wort 1S-"Bahn ist schon rein experimentell sozusagen, d.h. ohne Kenntnis der Theorie ein Unsinn."

165. Ibid.:
"Man kann, wie Jordan, sagen, dass die Naturgesetze statistisch seien. Man kann aber, und das scheint mir wesentlich tiefer, mit Dirac sagen, dass alle Statistik erst durch unsere Experimente hereingebracht ist."

166. Ibid.:
"Die Lösung kann nun, glaub ich, prägnent durch den Satz ausgedrückt werden: Die Bahn entsteht erst dadurch, dass wir sie beobachten."

167. Jordan could of course have held an opinion privately, known to Heisenberg but not to us.

168. Ditto for Dirac.

169. Ch.III, ref.26.

170. Jordan claimed in recollection (see JAMMER [1974]) that only Pauli's intervention prevented a serious conflivt. But although Heisenberg's letters to Pauli reveal a slightly tense situation they do not give the impression of a serious crisis, except for a small incident concerning Oscar Klein. Klein saw a paper that Pauli had sent Heisenberg, but that Heisenberg had not shown him; as Heisenberg recalled that Klein always sided with Bohr , this may not have helped the Heisenberg-Bohr relationship. But Heisenberg emphasised that he and Bohr remained good friends throughout. See Ch.III, ref.26, and Heisenberg to Pauli, 16.5.27, 31.5.27, SHQP.

IV

171. HEISENBERG [1951,1956].

172. BOHR,N. [1928] Bologna, Atti del Congresso Internazionale dei Fisica, Como 11-20 Sept. 1927 vol.2 p.565; rep. in German [1928] dN 16 p.245, and in English in BOHR [1934]; see also [1928] N 121 p.580.

173. Ibid.(1934) p.54.

174. Ibid. p.53-4.

175. Ibid. p.54.

176. Ibid. p.68.

177. Ibid. p.56.

178. Ibid. p.56.

179. Ibid. p.72.

180. Ibid. p.53.

181. Ibid. p.53.

182. App.E, ref.19: see ch.II.

183. Ibid., see ch.II.

184. Ch. II, ref.128. See ch.II.

185. He never doubted the need for a wave aspect to light.

186. Ref.172 (1934) p.91.

187. Act V, scene V. The situation here is analogous to that discussed by Møller: see below.

188. HØFFDING,H. [1900] Macmillan, History of modern philosophy, vol.2, p.286. See also JAMMER [1966] p.84, for further references.

189. JAMES,W. [1890] Holt, NY, The principles of psychology, p.206.

190. Ibid., p.401.

191. MØLLER,P.M. [1893] Gijldudal, Cop., En Dansk Students Eventyr; see JAMMER [1966] p.175, and MOORE [1966].

192. Felix Klein, patron saint of Göttingen mathematics, did die that year.

193. Hilbert recalled with relish how he was the only mathematician or physicist, just about, to escape being expelled by the Nazis — and he was full of Jewish blood. See Reid's biography of Hilbert.

194. HILBERT,D., NEUMANN,J.von & NORDHEIM,L. [1927] MA 98 p.1, "Uber die Grundlagen der Quantenmechanik", recd. 4.4.27.

195. NEUMANN,J.von [1927] GN p.1, "Mathematische Begründung der Quantenmechanik", recd. 20.5.27; p.245, "Wahrscheinlichkeitstheoretischer Aufbau der Quantenmechanik", recd. 11.11.27; [1929] ZP 57 p.30, "Beweis des Ergodensatzes und das H-Theorem in der neuen Mechanik".

196. Ibid.

197. [1928] Gauthier-Villars, Electrons et photons: Rapports et discussions du Cinquième Conseil de Physique tenu à Bruxelles du 24 au 29 Oct.1927 sous les auspices de l'Institute Internationale de Physique Solvay.

198. Ch.III, ref.26.

199. Schrödinger to Born 2.11.26 SHQP:
"Ihre Ansicht teilen, zu tief im Banne derjenigen Begriffe stehen (wie stationäre Zustände, Quantensprünge usw.), die sich in den letzten zwölf Jahren Burgewecht in unserem Denken erwoben haben."

200. SCHRODINGER,E. [1927] AP 83 p.956, "Energieaustausch nach der Wellenmechanik"; trans in Collected papers on wave mechanics, to which all references will be made.

201. Schrödinger to Planck 31.5.26 LWM.

202. Schrödinger to Lorentz 6.6.26 LWM.

203. Ref. 200, p.141.

204. BROGLIE,L.de [1926] CR 183 p.447, "Sur la possibilité de relier des phénomènes d'interference et de diffraction à la Théorie des quanta de lumières"; rep. in BROGLIE [1953]

205. BROGLIE,L.de [1927] CR 184 p.273, "La structure de la matière et du rayonnement et la mécanique ondulatoire"; rep. BROGLIE [1953].

IV

206. BROGLIE,L. de [1927] JPR 8 p.225, "La mécanique ondulatoire et la structure atomique de la matière et du rayonnement"; rep. BROGLIE [1953] trans. BROGLIE & BRILLOUIN [1928], to which references will be made.

207. Ref. 205, p.274:
"En Micromécanique comme en Optique, les solutions continues des équations de propagation ne doivent fournier qu'une représentation statistique, la description microscipique exacte des phénomènes exigeant sans doute l'emploi de solutions à singularités traduirant la nature atomique de la matière et de rayonnement."

208. Ref. 206, p.113.

209. Ibid., p.117.

210. Ibid., p.119.

211. Ibid., p.122.

212. Ibid., p.128.

213. Ibid., p.114.

214. Ibid., p.114.

215. Ibid., p.114.

216. Ibid., p.135.

217. Ibid., p.125.

218. LEWIS,G.N. [1926] Yale, The anatomy of science; PNAS 12 p.24, "The nature of light"; N 117 p.236,"Light waves and light corpuscles".

219. Ch.II, ref.124.

220. Ref. 218 (N), p.236.

221. KLEIN,O. [1926] ZP 36 p.895, "Quantentheorie und funfdimensionale Relativitätstheorie".

222. BROGLIE,L. de [1927] JPR 8 p.65; trans. BROGLIE & BRILLOUIN [1928] as "The universe of five dimensions and the wave mechanics".

223. KALUZA,T. [1921] BB p.966, "Zum Unitätsproblem der Physik".

224. MADELUNG,E [1926] ZP 40 p.332, "Quantentheorie in hydrodynamische Form".

225. KORN,A. [1927] ZP 44 p.745, "Schrödingers Wellenmechanik und meine mechanische Theorien".

226. Ref. 114.

227. Lorentz to Schrödinger 27.5.26. LWM.

228. Ref.206, p.131.

229. Einstein to Schrödinger 31.5.28. LWM .

230. See JAMMER [1974].

231. This was the basis of the Copenhagen group of interpretations.

232. Ref.197, p.178:
"Dans la question de la "validité de la loi de causalité" notre opinion est celle-ci: Aussi longtemps qu'on ne considère que les expériences qui tombent dans la domaine de nos connaissances physiques et mécaniques acquises jusqu'ici, notre hypothèse fondamentale de l'indéterminisme essentiel est d'accord avec l'experience. Le développement ultérieur de la théorie du rayonnement ne changera rien à cet état de choses. Car le dualisme entre corpuscules et ondes, qui dans la mécanique des quanta apparaît comme une partie d'une théorie complète, sans contradictions, existe d'une forme tout à fait semblable pour le rayonnement."

233. BORN,M. [1928] dN 17 p.109, "Uber den Sinn der physikalisch Theorien".

234. Ref.232.

235. Ref. 206, p.114.

236. Schrödinger to Planck 4.7.27 LWM.

237. SCHRODINGER [1928] Blackie, Lectures on wave mechanics:
"Is it not rather bold to interpret measurement according to a picture which we know to be wrong?"

238. Einstein to Schrödinger 31.5.28 LWM.

IV

239. Ref.197, p.254:
"La théorie a la prétention d'être unc théorie complète des processus individuels."

240. Ibid.:
"une mécanisme d'action à distance tout particulier".

241. Einstein to Born 6.12.26 BEC:
"inner voice says He wouldn't play dice."

242. Ref. 197:
"Je pourrais toujours garder ma foi déterministe pour les phénomènes fondamenteuse, dont je n'ai pas parlé."

243. Ref. 197.

244. Ibid., pp.251, 264.

245. Ibid., p.178. In a sense of course they were quite right. The introduction of relativistic considerations led to Quantum Field Theory, while the problem of reconciling quantum theory and relativity theory is one that still causes difficulties today.

NOTES TO APPENDIX A

1. JAMMER [1966] gives a good account.
2. WIEN,W. [1894] WAP 52 p.132.
3. PLANCK,M. [1949] Phil.Lib.,NY [1950] Wms. & Norgate, London, Scientific autobiography and other papers.
4. E.g. MEISSNER,W [1951] S 113 p.75, BORN,M. [1948] ONFRS 6 p.161, ANDRADE,E.N.daC. [1948] N 161 p.284.
5. Ref. 3, p.187 ("Religion and Natural Science" 1937).
6. Ref. 3, p.13 ("A scientific autobiography").
7. Ibid. p.20.
8. This is suggested by HERMANN [1969].
9. By Maxwell in 186o and 1878, and by Boltzmann in 1868.
10. See below.
11. This is discussed especially by HIEBERT[1968].
12. PLANCK,M. [1882] AP 15 p.446, "Verdampfen Schnelzen und Sublimieren". Quote from p.474:

    "Der zweite Haupsatz der mechanische Wärmetheorie, consequent durchgeführt, ist unverträglich mit der Annahme endlicher Atome. Es ist daher vorauszusehen, das es im laufe der weiteren Entwicklung der Theorie zu einem Kampfe zwischen diesen beiden Hypotheses kommen wird, der einer von ihnen das Leben kostet. Das resultat dieses Kampfes jetzt schon mit Bestimmtheit voraussagen zu wollen, wäre allerdings verführt widers scheinen mir augenblicklich verscheidenartige Anzeichen darauf hinzudenken, dass man trotz der grossen bisherigen Erfolge der atomistischen Theorie sich schliesslich durch noch einmal zu einer Aufgabe derselben und zur Annahme einer continuerlichen Materie wird entschiessen müssen."
13. CLAUSIUS,R. [1889-91] Vieweg, Die Mechanische Wärmetheorie, vol.3.
14. PLANCK,M. [1891] ZPC 8 p.647, "Allgemeins zur neuen Entwicklung der Wärmetheorie":

    "Jeder, der die Arbeiten derjenigen beiden Forscher studiert, die wohl am tiefsten in der Analyse der Molekularbewegungen eingedrungen sind: Maxwell und Boltzmann, mit sich des Eindrucks nicht erwehren können, dass der bei der Bewältingung dieser Probleme zu Tage getretene bewunderungswürdige Aufwand von physikalischen Scharfsinn und mathematischer Geschwindigkeit nicht im wünschenswerten Verhältnis steht zu der Fruchtbarkeit der gewonnen Resultate."
15. ZERMELO,E. [1896] AP 57 p.485; 59 p.793.
    BOLTZMANN,L. [1896] AP 57 p.773;[1897] 60 p.392.
16. PLANCK,M. [1897] BB pp.57, 715, 1121; [1898] p.449; [1899] p.440. Summary in [1900] AP 1 p.69. Boltzmann's main criticisms came in BOLTZMANN,L. [1897] BB p.660; [1898] p.182.
17. PLANCK,M. [1897] Vorlesungen uber Thermodynamik, in the preface:

    "Die erste greift am tiefsten hinein in das Wesen der betrachten Vorgängen, sie wäre daher, wenn sie sich exakt durchführen liesse, jedenfalls als die vollkommenste zu bezeichnen."
18. Ibid:

    "... scheint aber in ihrer weiteren Entwicklung auf vorläufig unuberwindliche Hinderwisse zu stossen, die nicht nur in der hochgradig komplizierten mathematischen Durchführung der angenommen Hypothesen, sondern vor allen Dingen in prinzipiellen, hier nicht naher zu erörternden Schwierigkeiten bei der mechanische Deutung der thermodynamischen Hauptsätze begründet sind."
19. Ref.16, [1898]p.449.

A \

20. Ref.16, [1900]p.69. On p.73:
"Will man sich also mit der Thermodynamik, und auch mit der Erfahrung, in Übereinstimmung setzen, so ist das nur möglich auf Grund einer neuen, von den Maxwell'schen Gleichungen unabhängigen Hypothese. Eine derartige Hypothese ist enthalten in dem unten eingeführten Begriff der "natürlichen Strahlung". Wenn gesagt wird, ein elektromagnetischer Strahl besitze die Eigenschaften der natürlichen Strahlung, so soll dies kurz gesagt heissen: Die Energie der Strahlung vertheilt sich vollkommen unregelmässig auf die einzelnen Partialschwingungen, aus denen der Strahl zusammengesetzt gedacht werden kann."

21. Ibid, p.74:
"Die hier unternommene elektrodynamische Deutung des zweiten Hauptsatzes der Thermodynamik legt es nahe, einen kurzen vergleichenden Blick auch auf dessen mechanische Deutung, namentlich auf die entsprechenden Fragen in der kinetischen Gastheorie, zu werfen. Auch hier finden wir bekanntlich denselben, oft besprochenen Gegensatz zwischen der Grundgleichungen der Mechanik, welche einen vollkommen reversiblen Charakter haben, und dem Inhalt des zweiten Hauptsatzes, welcher für alle wirklichen Vorgänge Irreversibilität fordert. Aber auch hier lässt sich der Gegensatz in ganz ähnlicher Weise losen durch die Einführing einer besonderen Hypothese, welche, solange sie in Gültigkeit bleibt, alle Consequenzen des zweiten Hauptsatzes in sich birgt. Es ist dies in der Ausdrucksweise von L.Boltzmann die Hypothese der "molecularen Unordnung"."

22. RAYLEIGH,J.W. [1900] PM 49 p.539, "Remarks upon the law of complete radiation".

23. RUBENS,H. & KURLBAUM,F. [1901] AP 4 p.649, "Anwendung der Methode der Reststrahlen zur Prüfung des Strahlungsgesetzes".

24. Jeans merely corrected a constant in Rayleigh's paper.

25. PLANCK,M. [1900] VDPG 2 p.202, "Uber ein Verbesserung der Wienschen Spektralgleichung"; trans. TER HAAR [1967].

26. PLANCK,M. [1900] VDPG 2 p.237, [1901] AP 4 p.553, "Zur Theorie das Gesetzes der Energieverteilung im Normalspektrum"; trans. TER HAAR [1967]

27. Planck's formula corresponds to indistinguishable particles, and this concept had not yet been developed. Particles had always been treated as distinguishable in kinetic theory, corresponding to

28. See ANDRADE [1948], ref 30.

29. Ref.3, p.44.

30. Ref.26, Ter Haar p.88

31. PLANCK,M. [1920] Oxford, Nobel Lecture.

32. Planck to R.W.Wood, 7-10-31, quoted by HERMANN [1969].

33. Ref.26, Ter Haar p.87.

34. Ibid. p.88.

NOTES TO APPENDIX B

1. McCORMMACH [1967] has given a brief survey of Thomson's ideas on the structure of light.

2. RÖNTGEN,W.A. [1896] N 53 p.274.

3. STOKES,G.G. [1897] MMLPS 41, The Wilde Lecture 2-7-1897, "On the nature of the Röntgen rays"; rep. [1905] C.U.P., Mathematical and Physical Papers, vol.5 (Papers), p.256. Quote from p.258.

4. VILLARD,P. [1900] CR 130 p.1178.

5. Ref.2. The same suggestion was made by BOLTZMANN,L. [1896] JG 39 p.71. Many explanations of the rays were proposed in 1896, including aether vortices (MICHELSON,A.A., AJS 1 p.312), de-electrified cathode rays (BUTTELLI,A. & GARBOSSO,A., NC 4 p.289), and transvers vibrations (e.g. MALTEZOS,C., CR 122 p.1474, GOLDHAMMER,D.A., WAP 57 p.635).

6. GALITZIN,B. & KARNOJITZKY,A.de [1896] CR 122 p.717.

7. STOKES,G.G. [1896] PCPS 9 p.215, "On the nature of the Röntgen rays"; rep. Papers (ref.3) p.254.

8. WIECHART,E. [1896] SPGKP 37 p.1, "Die Theorie der Elektrodynamik und die Röntgensche Entdeckung".

9. Ref.7, Papers p.265-6.

10. THOMSON,J.J. [1904] Constable, Electricity and Matter, the Silliman lectures of May 1903.

11. Ibid. p.7.

12. FARADAY,M. [1846] PM, "Thoughts on ray vibrations"; rep. [1855] Experimental researches on electricity, vol.3 (Researches), p.447. Quote from p.451.

13. FARADAY,M. [1852] PM, "On the physical character of the lines of magnetic force"; RIP, "On the physical lines of magnetic force"; rep. Researches (ref.12) pp.407,438 resp..

14. THOMSON,J.J. [1893] Oxford, Notes on recent researches in Electricity and Magnetism.

15. Ref.10.

16. Ibid. p.62-3.

17. THOMSON,J.J. [1907] PCPS 14 p.421, "On the ionisation of gases by ultra-violet light and on the evidence as to the structure of light afforded by its electrical effects".

NOTES TO APPENDIX C

1. Ch.I, ref.90 (AP) p.644:
"Nicht als ob für die Emission keine Kausalität angenommen würde; aber die Vorgänge, welche die Emission kausal bedingen, sollen so verborgener Natur sein, dass ihre Gesetze einstweilen nicht anders als auf statistischen Wege zu ermitteln sind."

2. This formula was equivalent to his original formula for W, and could be easily deduced, e.g., from Ehrenfest's analysis.

3. App A, eqn. [3].

4. Ch I, ref. 149

NOTES TO APPENDIX D

1. Ch I, ref 8, p.99: I have corrected a misprint in the original which reads " $N(\nu') - N(\nu) / m$ ". Also on p.103, the original "bis q = 0" should be "bis q = ∞".

2. It can easily be deduced that L is $h\nu$.

NOTES TO APPENDIX E

1. THOMSON,J.J. [1910] PM 20 p.238, "The theory of radiation."
2. JEANS,J.H. [1910] PM 20 p.280, "On the motion of a particle about a doublet."
3. THOMSON,J.J. [1913] PM 26 p.792, "On the structure of the atom"; see also p.1044 and PCPS 16 p.643.
4. Ibid., p.793.
5. JEANS,J.H. [1914] PM 27 p.14, "Interaction between radiation and free electrons."
6. McLAREN,S.B. [1912] PM 25 p.43 "The theory of radiation". This paper was described as an "attempt to save the classical theory of radiation as a continuous wave motion." McLaren's ideas were expounded more fully at the 1913 meeting of the British Association, ref. 54, p.391.
7. The quotations are from the full version of McLaren's 1913 paper, not given in the above reference, but in McLAREN,S.B. [1925] C.U.P. Scientific papers.
8. Ibid., p.54.
9. BERNOULLI,A.L. [1916] AdS 42 p.24.
10. ALLEN,H.S. [1921] PRSE 41 p.34, "Aether and the quantum"; PM 42 p.523, "Faraday's magentic lines as quanta."
11. Ibid., (PM) p.537.
12. ALLEN,H.S. [1925] PM 49 p.981, "Quantum magnetic tubes in rotation."
13. WHITTAKER,E.T. [1921] PRSE 42 p.1.
14. ALLEN,H.S. [1921] N 108 p.341, "Faraday and the quantum."
15. From the Greek, calamos = reed pipe.
16. THOMSON,J.J. [1924] PM 48 p.737, "A suggestion as to the structure of light."; [1925] PM 50 p.1181; Engineering 119 p.602.
17. WHITTAKER,E.T. [1922] PRSE 42 p.129, "On the quantum mechanism in the atom."
    Whittaker also suggested a magnetic doublet model of the quantum, [1926] PRSE 46 p.306, "On a polarised light quantum", but he prefered Thomson's electric ring concept, and looked at the modifications of the classical theory that this involved himself, Ibid., p.116, "On the adjustment of Sir J.J.Thomson's theory of light to the classical electromagnetic theory." We should add that the wave-particle duality played a part in these conceptions; thus Thomson wrote (ref. 106, Engineering, p.604) that "light does not consist of one constituent but of two - the ring of electric force and its accompaniment of Maxwell waves."
18. Ibid., pp.213,221,223.
19. BOHR,N. [1923] ZP 13 p.117; trans as supplement to PCPS [1924], as "On the application of the quantum theory to atomic structure: Part I, The fundamental postulates." All references will be to this translation; the work was completed in 1922, November.
20. LORENTZ,H.A. [1923] PASA 25 p.414.
21. Ref 17.
22. Ibid.
23. Ibid.
24. EWING,J.A. [1922] PRS A100 p.449, "The atomic process in ferromagnetic induction."; PM 43 p.493, "A new model of ferromagnetic induction."
25. Ref. 17 .
26. In fact, the magnetic wheel would itself have to perform oscillations to make this analogy fully valid, though Whittaker did not mention this.

E

27. BAKER,B.B. [1922] PM 44 p.727.
28. Ref. 20.
29. Ibid.
30. ELDRIDGE,J.A. [1925] PRSE 45 p.245, "Note on Prof. Whittaker's atomic model." p. 246.
31. LANGMUIR,I. [1921] PR 18 p.104, "Forces within a static atom."
32. THOMSON,J.J. [1921] PM 41 p.510, "On the structure of the molecule and chemical combinations." This was a modification of his 1913 model, with both the repulsive and the attractive forces acting throughout the atom.

    It was developed by WOODWARD,I. [1924] PM 47.
33. PARSONS,A.L. [1915] SMC 65 p.11, "A magnetic theory of the structure of the atom."
34. ALLEN,H.S. [1922] PCPS 34 p.198.
35. FARADAY,M. [1852] PM, "On the physical character of the lines of magnetic force.", rep. in [1855] Experimental researches in electricity vol. 3, p.407; see also [1852] RIP, "On the physical lines of magnetic force.", rep. ibid. [1855] p.438.
36. Ibid., [1855] p. 407.
37. Ibid., p.408.
38. WHITTAKER,E.T. [1925] PRSE 45 p.246.

    THOMSON,J.J. [1907] Constable, The corpuscular theory of matter , p.1:
    "From the point of view of the physicists, a theory of matter is a policy rather than a creed; its object is ... above all to suggest, stimulate, and direct experiment."
39. HEAVISIDE,O. [1885] The Electrician p.306.

NOTES TO APPENDIX F

1. App.A, eqn.[1].
2. $p_m B_m^n \rho \{ e^{-E_n/kT} - e^{-E_m/kT} \} = p_m A_m^n e^{-E_n/kT}$

   $\therefore \rho = \frac{A_m^n}{B_n^n} \left\{ 1 / \left(e^{(E_m - E_n)/kT} - 1 \right) \right\} \propto \nu^3 / \left(e^{h\nu/kT} - 1 \right)$.
3. This was not made clear by Einstein.
4. In this case, $W_n B_n^m \rho = W_m A_m^n \quad \therefore \rho = \frac{p_m A_m^n}{p_n B_n^m} e^{(E_n - E_m)/kT} \propto \nu^3 e^{-h\nu/kT}$
5. EDDINGTON, A.S. [1925] PM 50 p.803, "On the derivation of Planck's law from Einstein's equation."

NOTES TO APPENDIX G

1. Ch.II, ref. 72.
2. Ibid. (PM), p.455.
3. Ref. 76, p.104:
   "Si deux ou plusiers atomes ont des ondes de phase qui se superposent exactement dont on peut dire par suite qu'ils sont transportés par la même onde, leurs mouvements ne pourront plus être considérés comme entièrment indépendants at les atomes ne pourront plus être traités comme des unités distinctes dans les calculs de probabilité."
4. Dirac Archive

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1. Unpublished source material.

SHQP. Most of the source material used is in the archive of Sources for the History of Quantum Physics, copies of which are held in the Library of the American Philosophical Society, Philadelphia, in the Library of the University of California at Berkeley, and in the Niels Bohr Institutet, Kobenhavn. A partial catalogue is published: KUHN,T.S. et al. [1967] American Philosophical Society, Sources for History of Quantum Physics. The only items used in this work that do not appear in this catalogue are letters from Heisenberg to Pauli, which are to be found on microfilm 80.

Dirac Archive. The only unpublished source material used not to be found in the above archive is in the Dirac archive at Churchill College, Cambridge (England).

Translations of unpublished source material are my own and are accompanied by transcriptions in the original language. Where material has been used by other authors, I have taken their translations into account; those of SERWER [1977] seem to be particularly good and have occasionally been adopted verbatim. I have made occasional use of material that has been used by other authors but that I have not seen personally in the original. Here I give the published translation with a reference to the publication.

2. Published source material: manuscripts and correspondence.

BEC BORN,M. & EINSTEIN,A [1971] The Born-Einstein letters, Walker, NY; translations by Irene Born.

ESB EINSTEIN,A. & SOMMERFELD,A. [1968] Briefwechsel, Schwabe, Basel.

EBC EINSTEIN,A. & BESSO,M. [1972] Correspondance 1903-1955, Hermann, Paris; translations to French by Pierre Speziali.

LWM PRIZBAUM,K., ed. [1967] Letters on wave mechanics, Phil. Lib., NY.

BOHR Collected Works: see section (3) below.

The correspondence of Wolfgang PAULI is currently (December 1977) being prepared for publication, and the first volume is expected in 1978.

3. Published source material: reprints and translations.

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4. Published source material: contemporary publications

Full references to papers and books under this heading are given in the notes. The following abbreviations are used for periodicals:

ActM	Acta Mathematica
AdS	Archive des Sciences
AHES	Archive for the History of Exact Sciences
AIHS	Archives Internationales d'Histoire des Sciences
AJ	Astrophysical Journal
AJP	American Journal of Physics
AJS	American Journal of Science
AM	Annals of Mathematics
AP	Annalen der Physik
AS	American Scientist
BAAS	British Association for the Advancement of Science, transactions
BAS	Bulletin of the Atomic Scientist
BB	Berliner Berichte
BIARA	Bulletin of the International Atomic Research Association
BIP	Bulletin of the Institute of Physics
BJHS	British Journal for the History of Science
BJPS	British Journal for the Philosophy of Science
BNRC	Bulletin of the National Research Council
BPSW	Bulletin of the Philosophical Society of Washington
C	Centaurus
CIHS	Congrés International d'Histoire des Sciences
CJ	Crelles Journal
CMP	Communications in Mathematical Physics
CP	Contemporary Physics
CR	Comptes Rendus

D	Daedalus
DdN	Dokumente der Naturwissenschaften: HERMANN [1962-] in section (3)
Dia	Dialectica
dN	Die Naturwissenschaften
E	Ertenntnis
EncBrit	Encyclopaedia Brittanica
End	Endeavour
FM	Fundamenta Mathematica
FP	Foundations of Physics
FT	Fysik Tiddschrift
GB	Göttinger Berichte
GN	Göttinger Nachrichten
HP	Handbuch der Physik
HPA	Helvetia Physica Acta
HS	History of Science
HSPS	Historical Studies in the Physical Sciences
IJTP	International Journal for Theoretical Physics
ISIS	Isis
J	Janus
JDMV	Jahresbericht der Deutschen Mathematiker Vereinigung
JDR	Jahrbuch der Radioaktivität
JFI	Journal of the Franklin Institute
JG	Journal für Gasbedeuchtung
JHI	Journal of the History of Ideas
JHUC	Johns Hopkins University Circulars
JJP	Japanese Journal of Physics
JMP	Journal of Mathematical Physics
JM+P	Journal of Mathematics and Physics
JOSA	Journal of the Optical Society of America
JP	Journal de Physique
JPR	Journal de Physique et du Radium
JSHS	Japanese Studies in the History of Science
JSP	Journal of Speculative Philosophy
JSPCR	Journal de la Societé Physico-Chimique Russe
KDVS	Kongelige Danske Videnska Selskabs Skrifter
KNAW	Koninklijke Nederlandse Akademie von Welenschapen
KVAH	Kungliga Vetenskaps Akadamiens Handlinger
L	Lychnos
M	The Monist
MA	Mathematische Annalen
MAWB	Monatsberichte der Akademie der Wissenschaften zu Berlin
MB	Münchener Berichte
MG	Mathematical Gazette
MMLPS	Memoirs of the Manchester Literary and Philosophical Society
MNRS	Monthly Notices of the Royal Society
MNRAS	Monthly Notices of the Royal Astronomical Society
MPGZ	Mitteilungen der Physikalischen Gesellschaft - Zurich
MZ	Mathematische Zeitschrifte
N	Nature
NAM	Nouvelles Annales de Mathematiques
NatP	Natural Philosopher
NC	Nuovo Cimento
NR	Naturwissenschaftliche Rundschau
NRRS	Notes and Records of the Royal Society
NTVN	Nederlands Tijdschrift voor Naturkunde
NuP	Nuclear Physics
O	Osiris
OKW	Ostwald's Klassiker der Exakten Wissenschaften
ONFRS	Obituary Notices of Fellows of the Royal Society
Or	Organon

P	Physis
PAA	Proceedings of the American Academy
PAP	Poggendorff's Annalen der Physik
PAPS	Proceedings of the American Philosophical Society
PASA	Proceedings of the Academy of Sciences in Amsterdam
PB	Physikalische Bätter
PCPS	Proceedings of the Cambridge Philosophical Society
PICEP	Proceedings of the International Conference on Elementary Particles
PM	Philosophical Magazine
PNAS	Proceedings of the National Academy of Sciences
PPS	Proceedings of the Physical Society
PPSL	Proceedings of the Philosophical Society of London
PR	Physical Review
PRS(E)	Proceedings of the Royal Society (in Edinburgh)
PS	Philosophy of Science
PT	Physics Today
PTP	Progress of Theoretical Physics
PTRS	Philosophical Transactions of the Royal Society
PZ	Physikalische Zeitschrift
RAL	Rendiconti dell'Academia dei Lincei
RCMP	Rendiconti dei Circolo Matermatico di Palermo
RGS	Revues Generales des Sciences
RHM	Revue de Métaphysique et de Morale
RHS	Revue d'Histoire des Sciences
RIP	Royal Institution Proceedings
RMP	Reviws of Modern Physics
S	Science
SA	Scientific American
SC	Science and Culture
SHPS	Studies in the History and Philosophy of Science
SitzMun	Sitzungsberichte München
SM	Scientific Monthly
SMC	Smithsonian Miscellaneous Collections
SPAW	Sitzungsberichte der Preussischen Akademie der Wissenschaften
SPGK	Schriften der Physikalisch-ökonomischen Gesellschaft zu Konigsberg
SPRDS	Scientific Proceedings of the Royal Dublin Society
SR	Slavic Review
StM	Studia Mathematica
Sy	Synthese
TCSPS	Transactions of the C.S.Peirce Society
TO	The Observatory
TRIA	Transactions of the Royal Irish Academy
TRS	Transactions of the Royal Society
TRSE	Transactions of the Royal Society of Edinburgh
TSBK	Tokyo Sugaka Buturigakkari Kizi
VAA	Verslagen Akademie Amsterdam
VDPG	Verhandlungen der Deutschen Physikalischen Gesellschaft
VNGB	Verhandlungen der Naturforschenden Gesellschaften Basel
WAP	Wiedemannsche Annalen der Physik
WB	Wiener Berichte
ZE	Zeitschrift für Elektrochemie
ZP	Zeitschrift für Physik
ZPC	Zeitschrift für Physikalische Chemie

5. Secondary works

This is a select bibliography of the most valuable secondary works. References in the text and notes are as "FORMAN [1971]", and abbreviations used for periodicals are given in section (4) above. Physicists' recollections are included in this section when published. Unpublished interviews are deposited with SHQP (section (1)), as are Born's recollections and several other items of relevance.

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