Historically, there are mainly three orders of problems:

1) Dualism between theoretical description (unitary evolution) and experimental observations (reduction).

2) Unification of the behaviour of matter (which is apparently corpuscular) and light (which is apparently ondulatory) both in theory and experimental findings.

3) The existence of the different formalisms (apart the Dirac Picture), Heisenberg's and Schrödinger's ones, which were thought to be about particle-like and wave-like behaviours, respectively, and in fact turned out to be equivalent (passive and active unitary transformations, respectively).

There have been two opposite directions in answering to these problems:

1) An Idealistic position,

2) a Realist one.

The idealistic position was at least partly embraced by Bohr [1928; 1929], supported in part by von Neumann [1932] and finally supported by Wigner [1961; 1963; 1964]. With a certain moderation it is become part of the Copenhagen interpretation and can be found commonly in many textbooks.

The Realistic position has three main variants [see Auletta/Tarozzi. 2004]:

1) Realism of the particles alone [Born 1953],

2) Realism of the waves alone [Schrödinger 1926; 1927],

3) Realism of both waves and particles [de Broglie 1927; 1956].

All these are forms of classical realism (locality of the entities and perfect determination of their properties) that differently fails to take the first problem seriously into account.

As a middle line between these two extreme forms of interpretation, we have two further orientations:

1) The statistical interpretation [EPR 1935; Ballentine 1970], which has been shown to be finally wrong: In the latter 30 years experiments with individual quantum systems have become possible [see Auletta 2000 for details].

2) The later Heisenberg’s interpretation [1958], which tried to solve the first problem by introducing the idea that the environment could be somehow responsible for an uncontrolled transformation during measurement and proposed to consider quantum systems (before measurement) as a form of weak reality (potential reality).

Now it is possible to pursue this research line by

1) Introducing a more objective and smooth (POVM) understanding of complementarity,

2) Taking into account a distinction between global and local (decoherence and theory of quantum open systems),

3) Distinguishing between the potential information that the initial state of a system contains and the information that can be acquired [Auletta 2005; Horodecki et al. 2005]
We reconsider the crucial 1927 Solvay conference in the context of current research in the foundations of quantum theory. Contrary to folklore, the interpretation question was not settled at this conference and no consensus was reached; instead, a range of sharply conflicting views were presented and extensively discussed. Today, there is no longer an established or dominant interpretation of quantum theory, so it is important to re-evaluate the historical sources and keep the interpretation debate open. The proceedings of the conference contain much unexpected material. After providing a general overview, we shall focus (a) on the extensive discussions of de Broglie's pilot-wave theory, which de Broglie presented for a many-body system, including the much misunderstood critique by Pauli, and (b) on Born and Heisenberg's presentation of a 'quantum mechanics' apparently lacking in wave function collapse or fundamental time evolution. This talk is based on our English translation and commentary of the proceedings of the conference, 'Quantum Theory at the Crossroads' (forthcoming with Cambridge University Press, ISBN: 9780521814218).

As it is well-known, in 1900 Planck derived the black-body distribution law by supplementing the electrodynamical analysis of the heat radiation with statistical arguments. This application of statistics was meant as an extension of Boltzmann's work in kinetic theory. But was it true? I will present an outline of Boltzmann's statistical approach to physics and a comparison with Planck's. This comparison will rely on the analysis of three different applications of statistics in Planck's papers: the derivation of the black-body distribution law, the so-called second theory and the quantum theory of the ideal gas.

The emergence of the so-called ‘Copenhagen interpretation' of quantum mechanics has been the subject of much interest, both historical and philosophical. According to the familiar historical account, what we now refer to as the ‘Copenhagen interpretation' had its origins in discussions between Niels Bohr and Werner Heisenberg in the latter part of 1926 and early 1927, and was defended by Bohr in his classic debate with Einstein culminating with the EPR paper in 1935. Bohr's 'victory' over Einstein is commonly regarded as signifying the triumph of the ‘Copenhagen' interpretation. Yet recent scholarship has shown Bohr's views were never widely accepted, let alone properly understood, by his contemporaries, many of whom actually held divergent views of the orthodoxy in quantum theory. Despite the vast literature which discusses the Copenhagen or orthodox interpretation of quantum mechanics, there remains no agreement on what it is.

The difficulties which inevitably emerge from the attempts to make sense of the Copenhagen interpretation stem from the fundamental disagreements which were never settled among the founders of quantum mechanics in the 1930s. This has led to the view, which finds its clearest expression in the work of Mara Beller, that we should deny “the very possibility of presenting the Copenhagen interpretation as a coherent philosophical framework”. However, challenging the utility of the Copenhagen Interpretation as a historiographical concept raises a number of important
questions, which to my knowledge have not as yet received any systematic treatment: How are we to account for the appearance (or should we say illusion) of consensus achieved in the absence of any genuine agreement between key protagonists? How did so many different philosophical viewpoints come under the banner of the ‘Copenhagen interpretation’? And, what purpose or what philosophical or ideological agendas did the construction of the myth of the “Copenhagen interpretation” serve?

This paper takes up the task of addressing these questions in the hope of gaining a better understanding of what Don Howard has appropriately called the ‘myth of the Copenhagen interpretation’. Here I pursue the idea, which is to be found in the writings of Chevalley and Howard, that while the theory of quantum mechanics is a product of the 1920s, the Copenhagen interpretation, contrary to the standard view, is a construction of the 1950s and 1960s. My contention is that the idea of a unitary interpretation only emerges in the 1950s in the context of the challenge of Soviet Marxist critique of quantum mechanics, Heisenberg’s announcement of the unified ‘Copenhagen interpretation’ in 1955, and Wigner’s subjectivist interpretation of the measurement problem in the early 1960s. By tracing the way the image of the Copenhagen interpretation was constructed, we can arrive at a better understanding of the way in which the hidden disagreements within the ‘Copenhagen school’ were concealed, and the strategies which helped to form the impression of consensus.

DAVID C. CASSIDY
Hofstra University

EXAMINING THE CRISIS IN QUANTUM THEORY: ITS ORIGINS, NATURE, EXTENT, AND IMPACT

The quantum was always a controversial and disquieting concept. The introduction of energy quanta, the Bohr atom, and Planck’s constant all stimulated efforts to comprehend their fundamental origins in terms of familiar, “classical” physics. Further developments, such as Sommerfeld’s quantum conditions, Bohr’s correspondence principle, and their applications to such problems as hydrogen, molecular spectra, and the normal Zeeman and Stark effects, rendered the old quantum theory a remarkably successful, if still disquieting, theory.

Beginning around 1922, the disquiet began to turn into distrust and then despair. Within a short period of time a sense of the failure of quantum theory spread across portions of the quantum community, and with it, the perception that, as Born wrote in a paper celebrating the tenth anniversary of the Bohr atom, “Not only new assumptions in the usual sense of physical hypotheses will be necessary, but also the entire system of concepts of physics must be rebuilt from the ground up.” Similar sentiments were expressed in that period by many others, including Lorentz, Van Vleck, Bohr, Pauli, Heisenberg, and Landé. As a result, there were numerous attempts at the time to obtain hints for what Born was now calling a new “quantum mechanics” through the introduction of such ad hoc hypotheses as a “mystery force,” revised quantum principles, discontinuous equations, unmechanischer Zwang, and incomprehensible Zweideutigkeit.

In one of the earliest histories of quantum mechanics, Heisenberg referred to this period in 1929 as “the crisis in quantum theory.” Did a crisis really exist? If so, what difficulties and problems induced it so suddenly around 1922? How widespread was it? What attempts were made to resolve it? And in what ways was the reception of quantum mechanics conditioned by this period? In broader historiographic terms, in what ways might this period be compared with Thomas Kuhn’s notions of crisis, extraordinary science, and paradigm shift? In addition, is there any evidence of a relationship, as Paul Forman has suggested, between the sense of crisis within the quantum-physics community and the broader intellectual milieu of Weimar culture?

The proposed talk will be based upon a research paper in which I am attempting to examine these and other questions through careful analyses of the primary sources, both published and unpublished, along with reference to suitable secondary literature on the history of this period.
In 1909, Einstein derived a formula for the mean square energy fluctuations in a small subvolume of a box filled with black-body radiation. This formula is the sum of a wave term and a particle term. Einstein concluded that a satisfactory theory of light would thus have to combine aspects of a wave theory and a particle theory. In the following decade, various attempts were made to recover this formula without Einstein’s light quanta. In a key contribution to the 1925 Dreimännerarbeit with Born and Heisenberg, Jordan showed that a straightforward application of Heisenberg’s Umdeutung procedure, which forms the core of the new matrix mechanics, to a system of waves reproduces both terms of Einstein’s fluctuation formula. Jordan thus showed that these two terms do not require separate mechanisms, one involving particles and one involving waves. In matrix mechanics, both terms arise from a single consistent dynamical framework.

The talk aims at investigating the relation between the emergence and development of quantum physics and scientific philosophy in the 1920s and 1930s. Primarily, I will focus on two central figures of scientific philosophy: Hans Reichenbach and Moritz Schlick. As their published papers, manuscripts and extant correspondence indicate both of them were very much involved in the philosophical debate with respect to the quantum revolution. Apparently, from a philosophical point of view Reichenbach and Schlick not only gave an account of the fundamental concepts of causality and probability already at the beginning of the 1920s thereby anticipating the philosophical consequences of the development of quantum mechanics. Their discussion also led to an epoch-making change in scientific philosophy itself that was echoed by many physicists soon after at the end of the 1920s and the beginning of the 1930s.

In 1951 David Bohm created a realistic interpretation of quantum theory in contradiction to the Copenhagen interpretation of quantum mechanics in which the quantum mechanical wave function describes a purely mathematical probability amplitude, established by Werner Heisenberg, Niels Bohr and Max Born in 1927. Bohm's world view, based dialectical materialism, contributed substantially to the construction of new physical entities in his theory and led him to work intensively on the philosophy of science.

Bohm took up his political engagement at Berkeley within the context of his graduate studies and war research in the thirties and forties of the last century, in an environment dominated by trade unions and left-wing parties. This engagement and a constructed spy case formed the basis of a hearing before the Committee on Un-American Activities during the McCarthy era and led in the long run to his dismissal from Princeton University, where he had been assistant and associate professor since 1947. In Princeton Bohm had dedicated himself to the standard topics of the time, in particular plasma physics and its applications to different fields of physics, for example solid state theory and astrophysics. This changed after he lost his job in 1951: Bohm left the mainstream and began to work on his interpretation of quantum theory, but he still tried to present it in a
philosophical neutral perspective. Not until he went into exile in Brazil, did he modify his quantum mechanics in such a way, that the physical theory agreed with his beliefs and his philosophy of science. Bohm stated his philosophy of science in the book *Causality and Chance in Modern Physics* following Friedrich Engels's *Dialectics of Nature*. However, he transferred the concept of an infinite number of levels, which differ by dialectic jumps in their qualities, to quantum theory and founded the new physical entity of a subquantum level without being able to substantiate this in physical terms.

Bohm's example shows that the ideology and political attitude of a physicist can play a major role in the construction of scientific theories and entities but only in a suitable social context. In Bohm's case, this context was only given in his exile, when he was withdrawn from the scientific community and the political climate of the United States.

**OTTOS TERN'S LUCKY STAR**

In 1913, Otto Stern vowed to "quit physics if there was anything in this nonsense of Bohr's [model of the atom]. Seven years later, he proposed an experiment to put Bohr's theory to a crucial test. His aim was to decide "unequivocally between the quantum theoretical and classical views," by proving or disproving the existence of space quantization.

Space quantization emerged as a curious consequence of the Bohr model. Unlike the quantization of energy, which had, arguably, an observable counterpart in the atomic spectra, space quantization was considered a theoretical concept rather than an observable property. Nevertheless, Stern took the question of the existence of space quantization literally and, in his proposal, showed a way to challenge it by a direct experiment. The question arose from Stern's interest in magnetism and atomic theory, and put one to the service of the other. The experiment, accomplished by Stern and Walther Gerlach at Frankfurt after a two-year struggle, provided definitive evidence for the reality of space quantization.

The Stern-Gerlach experiment ranks among the dozen or so canonical experiments that ushered in the heroic age of quantum physics. Perhaps no other experiment is so often cited for elegant conceptual simplicity. From it emerged both new intellectual vistas and a host of useful applications of quantum science. Yet today, even among atomic physicists, we have found very few aware of historical particulars that enhance the drama of the story and the abiding lessons it offers. Among these particulars are a warm bed, a bad cigar, a timely postcard, a railroad strike, and an uncanny 'conspiracy of Nature' that rewarded Stern's and Gerlach's audacity. Their success in splitting a beam of silver atoms by means of a magnetic field startled, elated, and confounded pioneering quantum theorists, including several who beforehand regarded an attempt to observe space quantization to be naive and foolish.

Our narrative begins by briefly outlining the background of Stern and Gerlach and the historical context that brought them to collaborate at Frankfurt. We then describe the conception, vicissitudes, and reception of their iconic experiment and conclude with an epilogue tracing the divergent paths of the protagonists.

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**QUANTUM QUANDARIES: WALTHER NERNST, ALBERT EINSTEIN, OTTO STERN, AND THE SPECIFIC HEAT OF HYDROGEN**

In 1911, the German physical chemist Walther Nernst argued that the new quantum theory promised to clear up long-standing puzzles in kinetic theory, particularly in understanding the discrepancies between the predictions of the equipartition theorem and the measured specific heats of gases. Nernst noted that hydrogen gas would be a good test case. The first measurements were published by his assistant Arnold Euken in 1912, and showed a sharp drop in
the specific heat at low temperatures, corresponding to rotational degrees of freedom “freezing out.” In his 1911 paper, Nernst also developed a theory for a quantum rotator (a tiny rotating dumbbell representing a diatomic gas). Remarkably, he did not quantize rotational energies. Instead, the specific heat fell off because the gas reached equilibrium by exchanging harmonic oscillator quanta with quantized Planck resonators. Nernst’s theory was flawed. But Einstein adopted a corrected version at the 1911 Solvay Conference, and in 1913, he and Otto Stern published a detailed treatment. Following Nernst, Einstein and Stern did not quantize the rotators. But they did explore the new zeropoint energy that Max Planck had introduced in his “second quantum theory” in 1911. Einstein and Stern calculated the specific heat of hydrogen for two cases, one that assumed a zero-point energy for a rotator and one that did not. Only the former, with a zero-point energy, led to reasonable agreement with Eucken’s data.

But Einstein’s and Stern’s physical picture was sharply different from Nernst’s. There was no question of rotators exchanging quanta with resonators. Instead, in a second and almost unrelated section of their paper, they followed a line of argument that Einstein had begun in 1909 with his papers on “wave particle duality,” and had extended in a paper written in 1910 with Ludwig Hopf. Using this fluctuation-based approach, Einstein and Stern showed that Planck’s zero-point energy might reduce or even eliminate the need to quantize physical systems. In particular, they derived Planck’s radiation law by considering resonators—with a different zero-point energy—in equilibrium with a Maxwellian electromagnetic field; and in the process, they implicitly justified their approach to the specific heat. No assumption of discontinuity was required! Einstein and Stern were tentative in making this suggestion, and in any event, Einstein abandoned it later that year at the second Solvay Conference, citing unnamed inconsistencies. Later in 1913, in a new approach to the specific heat of hydrogen, Paul Ehrenfest did quantize the rotators, following a suggestion by H. A. Lorentz at the first Solvay Conference. So did Niels Bjerrum, a Danish chemist who was working on molecular spectra in Nernst’s laboratory. Einstein could easily have followed Lorentz’s approach himself. That he did not do so shows just how fluid and uncertain quantum theory remained, even for Einstein, over a decade after Planck had first introduced his strange new law of black-body radiation into physics.

ENRICO R. A. GIANNETTO
University of Bergamo

POINCARÉ’S ELECTROMAGNETIC QUANTUM MECHANICS

The rise of quantum physics is considered by outlining the historical context in which different conceptions of Nature (mechanistic, thermodynamic and electromagnetic ones) were in competition to give a foundation to physics. In particular, the roots of quantum physics within the electromagnetic conception of Nature and Poincaré’s quantum electromagnetic mechanics are analysed.

DOMENICO GIULINI
Albert-Einstein-Institut

ELECTRON SPIN OR "KLASSISCH NICHT BESCHREIBBARE ART VON ZWEIDEUTIGKEIT"

One of the central phenomena which led to the conviction that theoretical decriptions based on mechanical models were fundamentally incorrect at atomic scales was the anomalous Zeeman effect. In 1925 Pauli argued that it must be due to a magnetic moment carried by the outer electron (rather than the inner body of the atom, as thought previously) with an anomalous g-factor of 2. He did not, however, attempt to interpret the magnetic moment and angular momentum as being produced by a rotational motion of the electron. Rather, he spoke of a "eigentuermliche, klassisch nicht beschreibbare Art von Zweideutigkeit". After Goudsmit and Uhlenbeck introduced the idea of a rotating (spinning) electron, Pauli urged to avoid falling back on mechanical analogies. Without going into details he claimed that this idea fails since on had to accept superluminal velocities (presumably of the electron’s circumference). In my talk I will discuss whether this statement,
which is still much around today, is actually true. This will show interesting connections to the
history of Relativity.

DON HOWARD
Notre Dame University

EPR AND THE HOLE ARGUMENT: IN WHAT SENSE DID EINSTEIN THINK QUANTUM MECHANICS INCOMPLETE?

Recent scholarship has located the argument for the incompleteness of quantum mechanics in the
context of a broader discussion in the late-1920s and early 1930s, involving figures like Heisenberg
and Dirac, of some form of theoretical "closure" as a desirable feature of fundamental physical
theory. This paper argues that the sense of "completeness" at work in Einstein's mid-1930s critique
of quantum mechanics has its roots not in, or not directly in, the Gödelian notion of deductive
completeness but in the requirement of "Eindeutigkeit" or univocalness that Einstein had earlier
deployed in the "hole" and "point-coincidence" arguments in general relativity. The EPR argument
therefore exhibits, in yet another way, insufficiently appreciated connections in Einstein's thinking
between the foundations of quantum mechanics and the foundations of space-time theory.

DANIAN HU AND BAICHUN ZHANG
Chinese Academy of Sciences

THE EARLY INTRODUCTION OF QUANTUM THEORY IN CHINA

The paper will focus on the introductory works by Zhou Changshou in the 1920s. Zhou majored in
physics at Tokyo Imperial University in Japan, he was among the first to introduce in China
Planck’s quantum theory, Einstein’s light quanta, and Bohr’s atomic theory. The paper will
investigate both sources and impacts of Zhou’s early introduction in China.

ANJA SKAAR JACOBSEN
Niels Bohr Archive

THE BOHR-ROSENFELD WORK ON THE EPISTEMOLOGY OF QUANTUM ELECTRODYNAMICS

In this paper I will discuss the significance of the work by Niels Bohr and his young co-worker Léon
Rosenfeld on the epistemological problem about the measurability of electromagnetic field
quantities. Their work was published in two important papers, the first appeared in 1933 and the
second came out in 1950. Apart from contextualising their work, in particular in relation to quantum
epistemology in general at the time, I will attempt to characterise Bohr’s and Rosenfeld’s different
roles in this collaboration and use it to say something more general about Bohr's characteristic way
of working.

CHRISTIAN JOAS
MPIWG

THE ADVENT OF QUANTUM FIELD THEORETICAL METHODS IN SOLID STATE PHYSICS

When in 1957, as a side-product of their consulting work on thermonuclear fusion, Murray Gell-
Mann and Keith A. Brueckner published their calculation of the ground-state energy of the
interacting electron gas in the high-density limit — arguably the first systematic application of
Feynman diagrammatic methods to the solid state — they built on knowledge of and previous
training in nuclear many-body theory and quantum electrodynamics. Little did they foresee the
imminent burst of activity in the field of solid state physics, entailing the successful application of
field-theoretic many-body techniques to a plethora of previously unsolved problems. During the late
1950s and early 1960s, solid state theory was reshaped, following a novel and unified viewpoint
toward the many-body problem in solids built on the understanding of the emergence as well as the interrelationship of quasi-particle and collective excitations in correlated systems, thus providing the firm ground for the extremely fertile development of the discipline until today. Astonishingly, the field-theoretic methods enabling this reorientation of solid state theory were firmly established in renormalized quantum electrodynamics already by the early 1950s. Their application to the solid state was considerably delayed and started to blossom only after the seminal paper by Gell-Mann and Brueckner. Why did this happen? I argue that there are two reasons for this delay. First are the technical difficulties stemming from divergences within the first terms of the perturbation series in almost all problems of interest, requiring the invention of sophisticated computational schemes. Second, also external factors were to prevent the quick adoption of quantum field theoretic methods. During the War, solid state physics was in a state of quasi-hibernation, as many physicists originally working in the field joined war-related projects, rarely connected to solid state physics. In many scientific biographies, the War triggered a shift of attention towards nuclear physics and related areas. Only slowly did the field of solid state physics regain momentum after the War, being hindered further by Cold War and McCarthyism. I will show that the relative stagnation of postwar solid state theory with respect to many of its fundamental problems was resolved by knowledge transfer from a different area of theoretical physics. Ironically, some concepts emerging from the quantum field theory of solids later were able to crossfertilize again the fields from which it had drawn its original inspiration.

MARTA JORDI AND ENRIC PÉREZ
Universitat de Barcelona
THE EHRENFEST ADIABATIC HYPOTHESIS AND THE OLD QUANTUM THEORY, BEFORE BOHR

In 1918, Bohr published the first two parts of *On the quantum theory of line spectra*. In this extensive contribution, he offered a quantum theory much more elaborated than the one contained in his seminal works of 1913. Besides innovations of his own, as for example perturbation theory, Bohr incorporated many of the crucial discoveries that had taken place in the years in between: among others, the Einstein’s transition probabilities, the quantum rules of Sommerfeld-Epstein-Schwarzschild and the Ehrenfest adiabatic hypothesis.

The latter one was, no doubt, the one that had had fewer success among Bohr’s colleagues, and, in 1918, it was hardly mentioned. Therefore, until then, its role in the development of the quantum theory had been rather scarce.

In previous works, one of us has studied both the precedents and the genesis of this Ehrenfest’s contribution. Now, in a new paper, we will discuss in detail the content of this hypothesis, focusing our attention especially on the paper that Ehrenfest published in 1916 and 1917 in three different magazines. We will also briefly revise the precedents, which must be located in an exhaustive analysis of Planck's radiation theory, and the surprising results Ehrenfest achieved in the period 1911-1914, mainly —but not only— inspired in his first utilization of adiabatic invariance. Some of these results are: the proof of the necessity to introduce discontinuities in the statistical weight function of the black-body system, the strict separation between the statistical characteristics of Einstein’s and Planck’s quanta, and the curve for the specific heat of hydrogen, obtained from the quantization of the rotation energy of diatomic molecules, which was supplying acceptable results at low temperatures. Ehrenfest also managed to relate his hypothesis to the generalization of Boltzmann’s principle, that is, to the statistical interpretation of the second principle of thermodynamics that his teacher Boltzmann had proposed in the previous century.

When Bohr incorporated the adiabatic hypothesis into his theory under the name of ‘principle of mechanical transformability’, he removed its statistical reminiscences, and stressed that the purpose of this principle in his theory was not to establish links to classical mechanics, but simply to guarantee the stability of the quantum stationary states. Besides, the Danish physicist not only overcame some of the obstacles encountered by Ehrenfest, but he even extracted profit of
them. Good examples are the ‘singular movements’, which Bohr related to the change of the
degree of degeneracy of a system.

Our work will deal with the state of the adiabatic hypothesis of Ehrenfest (its formulation,
principal faults, applications, etc.) before Bohr’s intervention, and will be mainly based on a careful
analysis of Ehrenfest’s notebooks, correspondence and publications.

ALEXEI KOJEVNIKOV
University of British Columbia

"KNABENPHYSIK": THE BIRTH OF QUANTUM MECHANICS FROM A
POSTDOCTORAL VIEWPOINT

The majority of initial contributions to quantum mechanics between 1925 and 1927, as is well
known, came from younger students of physics under the age of 30. It is thus worth analyzing
historically how the quantum revolution and the emerging new discipline looked from the
perspective of not a professor, but a recent or actual Ph.D. student just embarking on an uncertain
academic career in economically hard times. Temporary assistantships, postdoctoral positions and
their equivalents were the chief mode of existence for young academics during the period. Using
newly obtained documents from the Rockefeller Archives and other collections, the paper
describes the policies for distributing such fellowships and the resulting varying patterns of
postdoctoral traffic to the main centers of quantum physics: München, Göttingen, Copenhagen and
Berlin. By following the work of several students, such as Pauli, Heisenberg, and Jordan, who
moved between various centers of research, it is possible to see how the transitory postdoctoral
way of life influenced their choices between rival approaches in the field and important problems to
handle. Insecure career trajectories and unpredictable moves through non-stable temporary
positions thus contributed to the general outlook and interpretation of the emerging theory of
quantum mechanics.

CHRISTOPH LEHNER, JÜRGEN RENN, CHRISTIAN JOAS, MASSIMILIANO
BADINO
MPIWG

SCHRÖDINGER’S WAY TO WAVE MECHANICS

Schrödinger’s work that led to the publication of his six groundbreaking 1926 papers seems to
follow a very simple trajectory: in the fall of 1925, Schrödinger came across de Broglie’s thesis
about matter waves. He sat down, started calculating, and within a few months and a minor
struggle with relativity, he presented the world with the (nonrelativistic) Schrödinger equation,
which in an intuitive and simple way could do what matrix mechanics needed the brain power of
half of Göttingen’s mathematics community for. Upon closer inspection, the story gets more
complicated. What did Schrödinger’s interest shift so suddenly from statistical mechanics to atomic
mechanics? Which (if any) of the different arguments for the transition to wave mechanics that he
offered in his papers played a role in his own thinking? How did his approach relate to matrix
mechanics and to earlier attempts at understanding quantum conditions from a continuum theory?
What role did his ongoing struggle for a relativistic wave equation play in his thinking?

Obviously, the first place to look for answers to questions about Schrödinger’s way to wave
mechanics are his research notes, luckily quite plentifully preserved at the Vienna Central Physics
Library. Surprisingly, the existing literature about these questions has treated the notebooks in a
rather cursory way. In a joint research project of the MPIWG team, we are taking a closer look at
the published and unpublished material.
Although the main lines of Louis de Broglie’s attempt to build a wave-particle theory are well known, the development of his views has not received a detailed analysis. This paper will focus upon the conflicts and hesitations that occurred in his work, from 1923 (his first papers on wave-particle duality) to 1925 (before the development of quantum mechanics proper). Starting from the relations $E=mc^2$ and $E=hn$, de Broglie attempted to associate a pulsating phenomenon to electrons and other particles, but at first he could not obtain suitable relativistic equations. He found a solution to this difficulty by introducing a wave, instead of a beat. The meaning of this wave was not clear, and its speed was greater than $c$, therefore introducing a new conflict with relativity. De Broglie introduced the idea of wave groups to solve this problem, and proved that the group velocity (and energy velocity) was equal to the velocity of the electron. The wave group also allowed de Broglie to ascribe a localization to the electron. However, this introduced a new conceptual problem. In a classical framework, to each electron should be assigned a well-defined speed, and therefore a well-defined energy and frequency. It was difficult to understand what meaning could be ascribed to a wave group, with a set of similar but different frequencies. This problem occurred four years before the proposal of Heisenberg’s uncertainty principle, and de Broglie did not arrive to similar ideas, at that time. In his thesis, de Broglie proposed a new approach to the problem, describing the electron itself as an extended system, since its electromagnetic energy is not concentrated in a point, but is spread over the whole space around the charge (with a stronger concentration around a centre). This concept is not equivalent to Schrödinger’s later proposal of an electron with extended charge, and it was not altogether clear in de Broglie’s thesis whether the charge itself was localized or spread around a centre. In the rest frame of the electron, its whole (infinite) structure was supposed to be pulsating with a frequency given by $\hbar n_0=m_0c^2$. Applying the Lorentz transformation to this pulsation, de Broglie readily showed that the beat would transform to a wave, relative to other reference frames, and obtained the speed, frequency and other properties of the wave. In this new approach, since the electron has a strong energy concentration around a centre, the wave associated to its pulsation will also have a strong amplitude concentration around a centre traveling with the speed of the electron. This is mathematically equivalent to a wave group, but conceptually it is quite different, because in the rest frame it does have a single, well-defined frequency, keeping the classical (non-probabilistic) outlook that guided de Broglie’s work. The strong link between wave and extended particle, as presented in the thesis, did not solve other problems, however. De Broglie was attempting to develop a theory which as intended to describe both electromagnetic radiation (light quanta) and electrons and other particles. He envisaged light quanta as energy chunks with speed smaller than $c$ (but very close to $c$) associated to electromagnetic waves with speed slightly greater than $c$ (of course, this required a correction of Maxwell’s equations). To explain interference phenomena – both in light experiments and with electrons – he introduced a probabilistic relation between waves and energy quanta. The probability of absorption or emission of a light quantum (or electron) should be proportional to the intensity (square of the amplitude) of the wave at each region. This hypothesis was necessary to obtain an agreement between his theory and the classical optical results, but could not be derived from (and was hardly compatible with) his fundamental concept of wave-particle duality. In the papers he published before the thesis, in his thesis, and in papers published shortly after it, de Broglie was fighting against severe conceptual difficulties such as those. He kept changing some of his fundamental hypotheses, maintaining only a few basic assumptions, such as relativistic dynamics and the relation $E=hn$. Instead of a coherent and final theory, his papers exhibited a changeable work in progress, with deep and unsolved conceptual problems.
Einstein's prediction of Bose-Einstein condensation, published in 1925, did not bear any fruit until 1938, when Fritz London began applying it to explain the superfluidity of He-II. According to several sources, this delay was due to a criticism levelled at Einstein’s argument by George Uhlenbeck. which persuaded Paul Ehrenfest as well as Einstein himself. Subsequent work by Uhlenbeck and Boris Kahn showed, however, that Uhlenbeck’s initial objection did not hold in the thermodynamic limit, thus opening the way to London’s work.

JAUME NAVARRO
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PLANCK AND DE BROGLIE IN THE THOMSON FAMILY

In an ironical remark made in 1930 about the situation of physics in the previous years Joseph John Thomson stated that “when the waves are taken into account, the classical theory of dynamics gives the requisite distribution of orbit [of the electrons] in the atom, and as far as these go the properties of the atom are not more inconsistent with classical dynamics than are the properties of organ pipes and violin strings, in which, as in the case of the electron, waves have to be accommodated within a certain distance. It is too much to expect even from classical dynamics that it should give the right result when supplied with the wrong material”.

The context of these remarks is the following: in 1927 J.J.’s son, George Paget Thomson, had obtained experimental evidence of the diffraction of cathode rays in the terms predicted by Louis de Broglie and developed by Erwin Schrödinger. These experiments, which were regarded as proof for one of the basic principles of the new quantum mechanics, were used by J.J. to argue in favour of the old classical physics. In his mindset, electron diffraction did not prove the principles on which Schrödinger’s theory was based, but only the results that theory involved. And these results could also be accounted for with a physical theory—i.e., a mechanical theory—in which the electron had an inner structure.

Contrary to the stubbornness of the father, the son’s reaction to his own experiments was to change his views on the validity of the new physics. Trained in the old physics and an advocate for it in the 1920s, G.P. Thomson was no active actor in the development or acceptance of the new quantum mechanics until electron diffraction appeared on stage.

The history of the early developments of quantum mechanics is, among many other things, the history of the clash of two generations, with different metaphysics and with different epistemologies. In this paper I want to explore the responses of J.J. Thomson and G.P. Thomson to Planck’s law and to de Broglie’s principle. I will compare both responses analysing the way in which the experimental evidence for the new physics was interpreted on the basis of ad hoc mechanical models in the case of the father, and in pragmatic and phenomenological terms by the son. The struggles of both father and son to come to terms with the new physics promises to be an interesting case study to understand the spreading of quantum physics far from the centres where it was being developed.

JOHN NORTON
University of Pittsburgh

EINSTEIN’S MIRACULOUS ARGUMENT OF 1905

Of all the papers of his 1905 year of miracles, in May 1905 Einstein reserved the term “very revolutionary” for just one, his paper that proposes the light quantum. That appraisal seems fair and even modest if we recall that the great achievement of 19th century physics had been
Maxwell's electrodynamics and its successful assimilation and verification of the wave theory of light. How could Einstein have the courage to overturn the best that the previous century of physics could offer? We arrive at a different perspective if we locate Einstein's proposal of the light quantum in the work he had been pursuing in statistical physics since his first publication of 1901. There Einstein had shown how one could read the microscopic structure of matter from its measurable, macroscopic properties. His earliest paper tries to infer microscopic intermolecular forces from the macroscopic phenomena of capillarity; and his dissertation sought the size of sugar molecules in the macroscopic properties of sugar solutions. The centerpiece of the light quantum paper is what I call his "miraculous argument": the inference to a spatial localization of the energy of high frequency radiation from the volume dependence of the entropy of the radiation. Once again Einstein read the microscopic structure of matter from it macroscopic properties.

GABOR PALLO
Hungarian Academy of Sciences
EARLY IMPACT OF QUANTUM PHYSICS ON CHEMISTRY: GEORGE HEVESY’S WORK ON RARE EARTH ELEMENTS AND MICHAEL POLANYI’S ABSORPTION THEORY

After Heitler and London published their pioneering work on the application of quantum mechanics on chemistry in 1927, it became an almost unquestioned dogma that chemistry will soon disappear as a discipline of its own rights. Reductionism felt victorious in the hope of analytically describing chemical bond and the structure of molecules. Old quantum theory has already produced a widely applied model for the structure of atoms and the explanation of the periodic system. This paper will show two examples of quantum physics entry into more classical fields of chemistry: inorganic chemistry and physical chemistry. Due to their professional networking, George Hevesy and Michael Polanyi found their ways to Niels Bohr and Fritz London respectively to cooperate in solving together some problems of classical chemistry. Their works on rare earth elements and adsorption theory throws light to the application of quantum physics outside the reductionist areas. They prove the heuristic and persuasive value of quantum thinking in the 1920-1930s. Looking at Polanyi's later oeuvre, his experience with adsorption theory could be a starting point of his non-justificationist philosophy.

BUHM SOON PARK
Johns Hopkins University
COMPUTATIONAL IMPERATIVES IN QUANTUM CHEMISTRY

A few years after the advent of quantum mechanics, Paul Dirac made a famous dictum: “The underlying physical laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known, and the difficulty is only that the exact application of these laws leads to equations much too complicated to be soluble.” In this paper, I explore that “difficulty” of the “exact application” in dealing with many-electron atoms and molecules, and examine various “approximation” methods developed in the early years of quantum chemistry. I focus on those who endeavored to calculate physical and chemical properties without using empirical data—i.e., the pioneers of the ab initio methods.

To be sure, the hydrogen molecule served as a testing ground for accuracy of quantum-mechanical calculations. Shortly after the publication of Walter Heitler and Fritz London’s 1927 paper, which explained the nature of the forces that bind hydrogen atoms, several young scientists jumped into the task of making the Heitler-London approach acceptable quantitatively or developing alternative methods. They were mostly graduate students and visiting fellows from various countries, eager to learn and use the new mechanical system: e.g., Yoshikatsu Sugiura, Shou Chin Wang, Nathan Rosen (later, one of the co-authors of the EPR paradox), Sidney Weinbaum, and Hubert M. James. Incremental improvements they made for better agreement between theory and experiment usually involved an enormous amount of labor in computations. I show that this computational constraint became an important factor in limiting the application of
quantum mechanics to more complex molecules until the appearance of electronic digital computers.

On the other hand, the point of departure for the problem of many-electron atoms was helium. The Norwegian physicist Egil A. Hylleraas’s innovative method of calculating helium’s ionization potential—by introducing a new coordinate of the inter-electronic distance into a trial function—was much hailed in the early 1930s, but it was not easily adaptable for heavier atoms because the number terms that had to be computed increased very rapidly with increasing numbers of electrons. Less accurate but more applicable than Hylleraas’s method was the one developed by Douglas R. Hartree. I trace the evolution of Hartree’s idea of the Self-Consistent-Field method and the modification of this method by Vladmir Fock and John Slater. Commonly called the Hartree-Fock method, this approximation method began to be used for the molecular problems in the early 1950s.

The main stream quantum theorists tended to undervalue this labor-intensive effort just as “a bone-headed calculation,” or question theoretical justifications of the approximation methods. I argue that, despite the existence of this cynical attitude, computational imperatives made pivotal contributions in spreading the validity of quantum mechanics across disciplinary and national boundaries and producing the practitioners not too much worried about its philosophical ramifications.

SLOBODAN PEROVIC
Carleton University

WHY WERE LOGICALLY DISTINCT THEORIES DEEMED EQUIVALENT IN EARLY QUANTUM MECHANICS?

Recently, several philosophers have carefully scrutinized the key arguments pursued by physicists at the beginning of the Quantum Revolution. Based on their analysis of the physicists’ arguments, these philosophers have characterized some of the essential agreements between physicists as unsubstantiated and unjustified. Most notably, the supposed equivalence of the two competing accounts of quantum phenomena, namely V. Heisenberg’s Matrix Mechanics (MM) and E. Schrödinger’s Wave Mechanics (WM), was recently debunked by the rethinking of the history of the debate over the foundations of quantum theory.

As MM and WM differed substantially both in terms of the mathematical techniques they employed and in terms of ontological assumptions about the microphysical systems at stake, and were successful in accounting for two distinct sets of experimental results, the supposed equivalence of the two was perceived as a major breakthrough when first conceptualized.

F. A. Muller (1997), however, argues, that the agreement concerning equivalence was unjustified and that only later developments in the late 1920s and early 1930s, and especially the work of a mathematician John von Neumman (1932) provided the sound proof of equivalence, as opposed to the famous proof provided by Schrödinger (1927) and the attempts by others (Eckart; Dirac; Pauli). If this re-evaluation is true, it would imply that the wide agreement among physicists on the equivalence of two formalisms in the mid-1920s, on which further developments of the theory were critically predicated, was an unjustified, indeed an irrational, act of faith (“a myth” – Muller) on the part of the physics community.

Thus, given that even in its best moments, the practice of physics does not live up to the minimal standards of rigor, as such standards are established in the practice of logic, mathematics, and mathematical physics, one might well question the very foundations of rationality in physics.

In response, I will argue that rationality in physics appears elusive even in its key moments only if we premise our analysis of actual scientific practice on narrow models of scientific knowledge. These models, such as that of P. Suppes (1957, 1960) used in the above-outlined analysis of the equivalence case, focus their conceptual and historical analysis on the aspects of scientific knowledge committed to the mathematical-logical analysis of the formalisms (such as MM and WM), which, although indispensable in scientific practice, may not be even the most important
mark of its rationality. Such a narrowly-focused analysis is bound to miss some key aspects of the physicists’ arguments, embedded as they are in philosophical and historical contexts, contexts which must be unraveled if one is to do justice to the physicists’ thinking.

More specifically, with respect to the case of equivalence, the kind of equivalence that was pursued at a later stage by Von Neumann, and which allegedly represents a moment of lucidity in the overwhelming messiness of the development of QM, was only a one possible refinement of the previous agreement on the preliminary, yet both experimentally and conceptually sufficiently substantiated, concept of equivalence. Some of the key early experimental results demonstrated that the N. Bohr’s (1985) model of atom provides a satisfactory common ground, unifying the opposed formalisms of MM and WM in very tangible terms and setting the groundwork for further exploration of their relationship. The formal proofs were only a part of this overall effort. Only at a later stage of development were the proofs worked out in precisely the terms that the above-mentioned historical and conceptual analyses that I criticize, takes to be the subject of the agreement on equivalence of the mid-1920s. Indeed, the claim of equivalence was based on the postulates of Bohr’s model concerning the so-called stationary states (i.e., the permitted energy states) of the atom that proved to be indispensable in both formalisms, which, while formulated in different ways, turned out to be reconcilable within the framework of Bohr’s model. Thus, the 1920s agreement on equivalence appears to be a myth only if we leave out the ontological (and focus exclusively on a purely formal goal) which was shared by the physics community of that time, of providing a coherent overall model of the atom. Although the equivalence of the 1920s was provisional, it was justified in virtue of its ontological aim.

Both the textbook tradition and vast segment of the secondary historical literature speaks as though there is such a thing em The Correspondence Principle, which (i) was first articulated explicitly by Bohr (1917) in Part I. of “On the Quantum Theory of Line Spectra,” (QTLS) (ii) guided the subsequent development of the “old” quantum theory up through Heisenberg’s 1925 “Umdeutung” paper, and (iii) remained embodied in some fashion or other in the subsequent “new” quantum mechanics. The question remains, though, whether much of this assessment derives primarily from Bohr’s post-1925 propaganda concerning interpretation of the new mechanics.

In QTLS, Bohr articulated three principles for guiding the construction of the quantum theory, (or more specifically, the treatment of the relation between radiation and material systems). The first, implicit in his seminal 1913 work establishing the Bohr model, addresses the frequencies of emitted and absorbed radiation. The second, sought a guide to determining the probabilities of transitions (Einstein’s A and B coefficients), and thus predicting the intensity of the radiation. The third addressed the problem of handling polarization. One question that naturally arises is whether these are instances of a truly principled general rule (as suggested by Bohr’s subsequent introduction in 1920 of the title “the correspondence principle” ) or instead are logically independent, albeit analogously motivated moves for extending the quantum theory. The ultimate irony, though, is that, no matter what, the dialectic of Heisenberg’s “Umdeutung” paper proceeds from the observation that the unobservability of various quantities appearing in the quantum theory would be tolerable if “the formal rules which are used in the quantum theory . . . were internally consistent and applicable to a clearly defined range of quantum mechanical problems.” This suggests that Heisenberg saw his matrix mechanics as principled replacement for a previous patchwork of ad hoc maneuvers. The paradox, then, is how Bohr could have (cogently and without equivocation) understood matrix mechanics (and the new quantum mechanics in general) to embody and vindicate “the” correspondence principle. One comes to suspect that, to the extent that the content of “the” correspondence principle can be articulated, that content must be seen to evolve hand in hand with the evolution of the quantum theory.

My talk will review the role of “the” correspondence principle in QTLS, then survey its subsequent use, elaboration, and “extensions” in the years leading up to 1925, and finally explore the paradox
In 1920, Einstein was appointed visiting professor at the University of Leiden. A major motivation for this appointment was his perception as a leading quantum theorist and the expectation that he would provide theoretical guidance to the research at Kamerlingh Onnes' cryogenic laboratory. In the paper, I will discuss Einstein's interpretation of the phenomenon of superconductivity in the early twenties. On a phenomenological level, he explored consequences of Maxwell's equations for the case of perfect conductivity. On a microscopic level, he suggested a model of superconductive charge transport which made explicit use of the Bohr-Sommerfeld quantum theory of electron orbits in the atom. Along both these lines of investigations, he suggested to do certain experiments. At least one of them was indeed carried out in Leiden.

In the context of the slow reception of the Bohr atom – both within the physics community and within the publications of science popularization – experimental and theoretical investigations in the possible reality of planetary atoms with properties governed by quantum principles are examined. A close reading of Peter Debye's writings alone and with Paul Scherrer of the years from 1914 to 1923 exhibits a characteristic flexibility and at times opportunism to the interpretation of experimental results. In this way, however, Debye was able at least for some period of time to convince even a number of otherwise reluctant scientists of the promise of the "Bohr-Debye model" about the atom. The development of Debye's claims of evidence for the appropriateness of Bohr type atomic models can be compared with similar processes of reinterpretation e.g. of the Franck-Hertz experiment, the Stern-Gerlach experiment or even of certain aspects of the creation of quantum mechanics by Born, Heisenberg and Jordan.

The introduction of group-theoretic methods by Heisenberg, Wigner and Weyl was part of the process of the mathematization of quantum mechanics. These methods were not only highly effective for a mathematical deduction of the quantum numbers, i.e. the qualitative part of the spectrum of an atom, but their conceptual potential for quantum mechanics was also pointed out by Wigner and Weyl. Nevertheless, there was a mixed reaction within the physicists' community to the use of group theory. The term ‘Gruppenpest’ (group pestilence) was coined.

During the 1920s and 1930s, the young Dutch mathematician Bartel Leendert van der Waerden (1903-1996) took part in the development of ‘modern algebra’, in which group theory as part of representation theory, established by Frobenius and Schur around the turn of the century, was being restructured. He took up the research on quantum mechanics on request of Ehrenfest, trying to clarify group-theoretic methods for the physicists. He invented spinor calculus (1929), wrote a
monograph on group-theoretical methods in quantum mechanics (1932) and, together with Infeld, worked out a spinor calculus adapted to the needs of general relativity theory (1933). In my talk I will analyse these works in the context of the on-going debate about group-theoretic methods and in the context of the rise of ‘modern algebra’, comparing van der Waerden’s approach to that of Wigner and Weyl. The case study thus enhances the understanding of the process of mathematization in quantum mechanics from the perspective of history of mathematics.

ERHARD SCHOLZ
Universität Wuppertal

H. WEYL ENTERING THE "NEW" QUANTUM MECHANICS DISCOURSE

Abstract: Early in 1925 H. Weyl finished his great series of publications on the representation of Lie groups and started the studies for his "Philosophie der Mathematik und Naturwissenschaften" delivered to the editors in summer 1926. He was in touch with M. Born and got to know of the developments in the Goettingen group around Born, Heisenberg and Jordan in early summer 1925. After a conversation with Born in September 1925 he started to develop ideas of his own how to quantize the mechanical observables of a system and communicated them to Born and Jordan in October 1925. In these letters he proposed the basic idea of a group theoretic approach to quantization, which he presented to the scientific public in his 1927 paper "Quantenmechanik und Gruppentheorie". This paper had a long and difficult reception history for several decades.

SKÚLI SIGURDSSON
Universität Wien

QUANTUM MECHANICS AND A NEW THOUGHT STYLE: DIE NATURWISSENSCHAFTEN IN THE INTERWAR YEARS

The scientific journal Die Naturwissenschaften was an important platform for producing and disseminating results at the frontiers of research in the interwar years. It was published weekly by Springer Verlag starting in 1913. The chief editor for 22 years was Arnold Berliner. In my contribution I will use this journal as a lens for analyzing changes in the scientific worldview resulting from the establishment of quantum mechanics in the 1920s. The German-speaking members of the quantum-mechanical network used Die Naturwissenschaften as a vehicle for communications, overview articles, celebrations, and book reviews. Taking a lead from the Polish-Jewish theoretician Ludwik Fleck, the German-speaking network might be characterized as a Denkkollektiv, the new scientific worldview a new Denkstil, and Die Naturwissenschaften a kind of Denkorgan or thought organ.

CHRIS SMEENK
UCLA

THE EARLY DAYS OF THE “GROUP PLAGUE”

Eugene Wigner and Hermann Weyl introduced the theory of group representations to the new quantum mechanics shortly after its formulation. Group theory proved to be an invaluable analytic tool in calculating atomic spectra and in elucidating the kinematics of a system based on its symmetry properties. These new techniques were not immediately embraced by the physics community, however; instead, some physicists complained about the pestilential spread of esoteric formalism through the physics literature, calling it the “Gruppenpest.” The first aim of this paper is to study the initial outbreak and early stages of the plague, focusing on the early research of Wigner and Weyl in the late 20s leading up to their influential textbooks. Although several others also helped to spread the plague, Weyl and Wigner serve as useful focal points not only because of the clear significance and influence of their work, but also because of their very distinctive approaches. Mackey (1993) characterizes their approaches as falling into two distinctive “programs,” differing primarily in terms of what the application of group theory was expected to yield. A second aim is to clarify the impact of group theory by looking at the different aspects
identified by Mackey: on the one hand, group theory was a powerful mathematical tool that allowed physicists to solve various problems in the new quantum mechanics, but on the other hand, it also led to critical reflection on the foundations and mathematical formalism of the theory.

Wigner’s early papers illustrated the power of the theory of group representations in analysing problems such as the energy spectrum for multi-electron systems. The knowledge of group theory Wigner had gleaned from crystallography was put to good use in recasting and generalizing Heisenberg’s (1926) results on two electron systems. Wigner showed how to find the eigenfunctions of the Hamiltonian based on combining permutation symmetry with rotational symmetry; a later series of papers with von Neumann incorporated electron spin. This work can be aptly characterized as an insightful application of group theory to central problems in the new quantum mechanics. By way of contrast, Weyl used the theory of group representations to clarify foundational problems. He turned to quantum mechanics after years of study of compact Lie groups culminating in the proof of the Peter-Weyl theorem. In his first paper on group theory and quantum mechanics, Weyl reformulated the canonical commutation relations by replacing the operators $\hat{P}, \hat{Q}$ with one-parameter groups of unitary operators $e^{i\lambda P}$, and he also introduced a formal approach to quantization intended to resolve operator ordering ambiguities. In this first paper and in his book published shortly thereafter, Weyl clearly applied the theory of group representations with the hope of resolving or significantly clarifying foundational problems.

To sum up, there are a wealth of historical and foundational questions related to the early days of the “group plague.” I have been able to find relatively few studies of these issues in the history of physics literature (listed below), and there is nothing comparable to accounts of the development of the theory of Lie groups in the history of mathematics literature (e.g., Hawkins 2000). Although obviously a comparably clear and detailed understanding of the developments described above is a long way off, this paper aims to give an initial survey of the territory.

**MICHAEL STÖLTZNER**

Universität Wuppertal

**VIENNA INDETERMINISM AND THE PROBLEMS OF QUANTUM MECHANICAL CAUSALITY**

In my paper, I sketch a debate between physicist-philosophers at Vienna and Berlin about the idea that the basic laws of nature are genuinely indeterministic. It had started long before the advent of quantum mechanics, but would chiefly influence how both sides reacted to the crisis of the Bohr-Sommerfeld quantum theory and the quantum mechanics of Heisenberg and Schrödinger. The debate involved two different readings of Boltzmann’s legacy statistical mechanics and two different answers to how causality and ontology ought to be combined.

“Vienna Indeterminism”, as I shall call the first tradition, characterized two generations of physicist-philosophers connected to the Vienna Institute of Physics, among them Franz Serafin Exner, Erwin Schrödinger, Philipp Frank and Richard von Mises. It involved the acceptance of the highly improbable events admitted by the second law of thermodynamics, an asent to Mach’s definition of causality in terms of functional dependencies, an empiricist shift of the burden of proof on the determinist’s shoulders, and the adoption of the relative frequency interpretation of probability. Max Planck and his student Moritz Schlick initially rejected all of these creeds and held in line with the Kantian tradition that strictly deterministic laws represented an indispensable basis for probabilistic theories.

While the Viennese, after 1926, felt themselves confirmed by the newest physics, Schlick had to take a strictly verificationist tack to reconcile his views on probability with the failure of his Kant-inspired notion of causality. Thus he compensated his qualified acceptance of indeterminism with an emphasis on the limits of language ensuing from Heisenberg’s uncertainty relations. Unlike Planck, he did not consider quantum mechanics as a transitory, or incomplete, theory of atomic phenomena.

In view of claims made by some historians that scientists’ abandonment of determinism and causality in the 1920s was caused (Paul Forman), or at least enhanced, by external factors, among
them the anti-scientific Weimar milieu and Bohr’s power politics (Mara Beller), a reconstruction of the philosophical debates between Vienna and Berlin requires historical contextualization. My argument is threefold. First, philosophical ambitions were widespread and often serious among German physicists, even though the allegiance to philosophical schools was low. The challenge arising from the cultural milieu thus also concerned the physicists’ philosophical ambitions and not just their social prestige. The opposition commonly made between the rationality and the historical contingency of physicists’ stance on causal matters misses the point. Second, this philosophical discourse followed influential role models and took place in widely-read journals, most prominently Die Naturwissenschaften. It became an important factor for the emergence of scientific philosophy. Third, since the physicist-philosophers were not strictly indebted to philosophical schools, they could form strategic alliances that emphasized one philosophical aspect considered pivotal at a time while other features, often expressing severe disagreements of fact, were played down. This process can be witnessed at the end of the discussion between the Viennese and the Berliners in the mid 1930s. Being confronted with a plethora of “metaphysical” misinterpretations and having taken the linguistic turn, Frank and Schlick developed a logical empiricist interpretation of Bohr’s complementarity. It brought them into opposition with Schrödinger’s quest for a modified ontology of quantum mechanics, although Schrödinger continued to be a staunch advocate of indeterminism.

CHEN-PANG YEANG
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ENGINEERING THE ENTANGLEMENT: QUANTUM COMPUTATION, QUANTUM COMMUNICATION, AND RE-CONCEPTUALIZING INFORMATION

When Einstein, Podolsky, and Rosen (EPR) presented the famous two-particle thought experiment in 1935, their purpose was to demonstrate the incompleteness of quantum mechanics and to challenge the Copenhagen interpretation. This EPR state, or “entangled state,” exhibited properties defying intuitive, causal explanations, which indicated how odd quantum mechanics is. From David Bohm to John Bell, those working on the EPR problem pursued the same goal: probing the conceptual foundation of quantum mechanics. At the end of the last century, however, a group of physicists and computer scientists turned the entangled state from a puzzle into practical resources. Setting aside the epistemic issues concerning whether quantum mechanics is complete and why entanglement is so "strange," they took for granted the "strange" properties of this quantum result and appropriated them to explore novel ways of information processing. Why and how did this turn occur?

This paper examines the rise and ongoing development of such endeavors—known as quantum computation and quantum communication—since the 1980s. At the core of these endeavors is the process in which the “strange” features of entanglement such as instantaneous action-at-a-distance were reinterpreted as opportunities to devise parallel computing algorithms and encrypted telecommunications. But the physicists and computer scientists were not satisfied with taking entanglement only as a technique to solve special problems. They moved further to generalize a “quantum-mechanical” view of automatic computation, modifying the logical premises of computer science that Alonzo Church and Alan Turing had laid out in the 1930s. Building upon quantum computation, moreover, they conceived a new paradigm for designing communications codes that would follow a different information theory from the one Claude Shannon had established in 1948.

The recent history of quantum computation and quantum communication marks the convergence of three interrelated scientific and engineering research programs: theoretical physicists’ pursuits of quantum mechanics’ conceptual foundation, industrial-laboratory researchers’ quests for novel physical means to implement digital computers, and atomic physicists’ experiments on coherent or single quantum states. And all of them have drawn upon the intellectual resources of quantum mechanics, computer science, and information theory developed in the past century. Although no system has yet been physically implemented (albeit strong academic interests and substantial military funding), the ongoing quantum computation and quantum communication has shown a
conspicuous tendency among the physicist community to weave the decades-old central puzzle of quantum mechanics with a reconceptualization of information.