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QUANTUM MECHANICS AT THE CROSSROADS

New Perspectives from History, Philosophy and Physics

With 46 Figures

Springer

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Preface

This book offers to a diverse audience the results of recent work by historians of physics, philosophers of science, and physicists working on contemporary quantum-mechanical problems. The volume has three themes: new perspectives on the historical development of quantum mechanics, recent progress in the interpretation of quantum mechanics, and current topics in quantum mechanics at the beginning of the twenty-first century. The *Crossroads* of the title can be taken in two ways. First, quantum mechanics itself came to a sort of crossroads in the 1960s, when it squarely faced the challenges of interpretation that had been ignored by the founders, and when it began, at an ever-increasing pace, to embrace and exploit a host of new quantum-mechanical phenomena. And, second, this volume, with its intersecting accounts by historians, philosophers and physicists, offers a crossroads of disciplinary approaches to quantum mechanics. All the authors have written with multiple audiences in mind – readers who may be historians, philosophers, scientists, or students of this most strangely beautiful creation that is quantum mechanics.

The volume is rich in significant topics. Chapters taking historical perspectives include John Heilbron's sympathetic but critical treatment of Max Planck, Bruce Wheaton's study of the scientific partnership of Louis and Maurice de Broglie, and Georges Lochak's very personal account of the relationship between Werner Heisenberg and Louis de Broglie. Michel Bitbol presents a philosophically nuanced study of Erwin Schrödinger's rejection of quantum discontinuity, while Roland Omnès offers a critical reappraisal of John von Neumann's axiomatization of quantum mechanics. We reflect on these figures of the founding generations of quantum mechanics as they argue over the reality of particles and quantum jumps, grapple with the question of what parts of classical physics must be renounced and what retained, and search for the Absolute while a world crumbles around them.

Chapters devoted to current topics in quantum mechanics include Wolfgang Ketterle on Bose–Einstein condensation, Howard Carmichael on wave–particle correlations, and William Wootters on quantum-mechanical entanglement as a resource for teleportation and dense coding. Chapters devoted to interpretive and foundational issues include Abner Shimony on nonlocality, Alan Thorndike on consistent histories, and Max Schlosshauer and Arthur Fine on decoherence. Some of these chapters are on challenging subjects, but all were written to serve as entrées to topics of current research and discussion for readers who are not specialists.

The chapters are arranged in the following way. The historical accounts open the volume. The chapters taking philosophical points of view follow. And the volume concludes with the chapters devoted to recent physics. But, as is appropriate in a volume designed as a crossroads at which physics, history and philosophy meet, there is a good deal of interchange and overlap. For example, Michel Bitbol's philosophical study of Schrödinger's attitudes toward particles and their purported quantum jumps is informed by a deep understanding of the history of twentieth-century physics. Maximilian Schlosshauer and Arthur Fine's overview of the role of decoherence in contemporary quantum-mechanical thinking displays not only a fine sense of the history of the subject, but also serves as an excellent introduction to the scientific literature. The concluding chapter, by Roland Omnès, on the historically evolving relation between the world of classical experience and the world of quantum-mechanical phenomena, weaves history with new physics and tries, as well, to offer a new road in the philosophy of knowledge. A crossroads indeed.

We would like to express our thanks to the authors for their generosity in responding to requests for revisions and clarifications; to Susan Fredrickson for assistance with the manuscript; to Neva Topolkski for many kinds of help with the project; to James Bernhard for serving as our computer expert; to H. James Clifford, whose early support and enthusiasm helped bring make this volume a reality; and to our editor, Angela Lahee for her encouragement, advice and skill.

Seattle, Washington — *James Evans*
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May, 2006

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Introduction: Contexts and Challenges for Quantum Mechanics

James Evans

The twentieth century produced two radical revisions of the physical worldview – relativity and quantum mechanics. Although it is the theory of relativity that has more deeply pervaded the public consciousness, in many ways quantum mechanics represented the more radical change. Relativity required its own accommodations, but at least it still allowed the retention of classical views of determinism and local causality, as well as the conceptual separation of the experimental object from the measuring apparatus. In the pages that follow, we shall see many manifestations of what the quantum-mechanical rejection of these classical concepts has entailed – not only in the doing of physics, but also in the interpretation and application of its results. This volume offers new perspectives on quantum mechanics, by historians of physics and philosophers of science, as well as physicists working at the moving frontier of quantum theory and experiment.

Some of the founding generation, notably Heisenberg, rejected classicality with a sense of liberation and exhilaration. Others, including Einstein, Schrödinger and de Broglie, were deeply worried about the implications of such a rejection. And even Bohr – though fervent and dogmatic in defense of the completeness of quantum mechanics – recognized that a genuine problem existed in the fact that the quantum world and the world of everyday experience seemed to obey different laws. This was a dichotomization of the world no less drastic than Aristotle's separation of the celestial realm from the sublunar world, or Descartes' bifurcation of existence into matter and spirit. This challenge to quantum mechanics was dealt with in the particular intellectual context of the 1920s and 30s, which seemed to determine the sort of accommodation worked out in the Bohr–Heisenberg Copenhagen interpretation and its more sophisticated, axiomatic development by von Neumann.

Now, with historical distance, we can see that the founders left some serious questions unanswered. The intellectual context of quantum mechanics changed drastically in 1960s, when physicists, stimulated by the work of John Bell, began to take foundational questions seriously once again. And it is fair to say that things have changed again in the last two decades, as physicists have warmly embraced and exploited the quantum weirdness implied by entangled states and the apparent nonlocality of quantum mechanics. We need point only to the recent experimental demonstrations of the entanglement of macroscopic objects and to the theoretical program for the "teleportation" of quantum states. What were once only theoretically possible, but practically unrealizable, bizarre phenomena have increasingly been laid open to study and perhaps even to practical application. In historiography of science, too, attitudes have changed. The early doubters tend to be treated far more sympathetically now, even if we still recognize that they had no sustainable alternatives to offer. Philosophers of science take the questions they raised with greater seriousness, even as they grapple with the implications of new experiments that seem to threaten the dissolution of the quantum–classical divide, and to promise the end of Bohr's dichotomy.

In this chapter, we shall sketch the challenges faced by the developers of quantum mechanics, laying particular stress on the challenges of indeterminism, entanglement, nonocality, and the puzzle of the quantum–classical divide. We shall sketch, too, the intellectual contexts in which successive generations of quantum mechanicians have worked. This will help us place the chapters that follow into their own historical, philosophical, and scientific contexts. The intersection of disciplinary views offered by this book is particularly timely, for, as we shall see, quantum mechanics has moved into a new and exciting period.

1.1 Periodization of Quantum Mechanics

The history of quantum mechanics can be broken conveniently into a period of searching (1900–1922), the breakthrough (1923–1928), a period of accommodation, development and application (1929–1963), and the new baroque period (1964–present). The period of searching began with Max Planck's efforts to understand the blackbody spectrum. There is a rich irony here, centered around the fact that modernist (and now also post-modernist) interpretations of early twentieth-century physics have emphasized the unsettling concepts of relativity and uncertainty and the ways in which they grew out of, as well as transformed, a certain social and political milieu [1]. But Einstein, for his part, was motivated by a search for the invariant and eternal. His striving for greater and greater degrees of generality led ultimately to equations that were invariant under arbitrary transformations of coordinates: the general theory of relativity. Max Planck, in his own way, sought for permanence and security. He imagined a physics that would be independent of human prejudices and conventions, as well as of the accidents of human history – the physics that investigators from a multiplicity of planets, all working in splendid isolation from one another, must eventually converge on. It is no doubt for this reason that Planck was so attracted to thermodynamics, an austere branch of physical reasoning that represented the culmination of the stream of thought in classical physics unalterably opposed to mechanical hypotheses. John Heilbron, in Chapter 2 of this volume, offers a moving account of Max Planck's search for the Absolute, Planck's discovery in 1900 of the quantum of action, as well as his political situation, and political choices, in Germany from one World War to the next.

Whether Planck believed in the reality of his radiation quanta is a question that has given rise to a minor industry of historical analysis [2]. But these quanta began rapidly to assume a real existence with the work of Albert Einstein, who within five years had applied Planck's quantum of action to an explanation of the photoelectric effect. Perhaps even more importantly, Einstein showed in 1917 that it was necessary to associate a particle-like momentum, and therefore a direction, with light quanta [3]. Radiation quanta were on their way to becoming particles of light.

The quantum of action and the resulting quantization of energy levels were rapidly applied also to solid-state physics. Nature had generously given humanity two problems simultaneously easy and profound – the harmonic oscillator and the hydrogen atom. The oscillator had given Planck safe passage to the solution of the blackbody problem, and the quantum oscillator also dominated work on the theory of specific heats during the period of the old quantum theory.

The great challenge of the hydrogen atom was to explain the spectral lines. Niels Bohr's impressive but bewildering calculation of the Rydberg constant in 1913 showed that a new way of working was at hand, which drew from a grab-bag of classical rules whatever worked and discarded anything unnecessary or embarrassing. Classical mechanics could be used to solve the orbit problem. But then quantization rules were invoked to select a countably infinite number of solutions from the uncountably infinite number of orbits allowed by classical mechanics.

One of these rules turned out to be equivalent to the quantization of angular momentum. As for the problem of the stability of the orbits – it was scandalous, since it was easy to calculate from classical electrodynamics that an electron in orbit around a hydrogen nucleus must radiate away its energy so rapidly that it ought to spiral into the nucleus in a fraction of a second. There was nothing for it but to forbid ordinary electrodynamics from playing any role inside the atom and to postulate that the electron could remain almost indefinitely in one of its stationary states. Radiation occurred in Bohr's theory only if an electron "jumped down" to a lower energy level. The opening of the Great War in 1914 meant that Bohr's program had for a long while no competitor, and that this approach to atomic physics dominated thinking well into the next decade [4]. With great ingenuity and difficulty, Bohr's program was extended by others to a relativistic treatment of the hydrogen atom, and to more complicated atoms. But there seemed to be no unambiguous way to generalize the quantization rules to aperiodic systems, such as the chaotic helium atom, in which each electron repels the other.

The breakthrough came on two different fronts. In 1923, Louis de Broglie suggested, using arguments based on Einstein's relativity as well as on Planck's quantum of action h, that it made sense to associate with a particle of momentum p a wave of wavelength h/p. de Broglie developed the same idea from several points of view, grouping them all together in his famous doctoral thesis of 1924. In his thesis, de Broglie showed that particles (such as electrons), which satisfy Maupertuis's principle of least action in traveling from a fixed point A to another fixed point B, automatically also satisfy Fermat's principle of least time, provided that de Broglie's new wave is taken into account. Two minimization principles of early physics (one of the eighteenth century appropriate to particles, and one of the seventeenth century appropriate to waves), which were formerly deemed incompatible, were now seen to be natural consequences of one another. Wave–particle duality was here to stay. In Chapter 3, Bruce R. Wheaton offers a nuanced account of Louis de Broglie's contribution, and lays particular stress on the influence of Maurice de Broglie on his younger brother. Maurice was a gifted experimentalist, who maintained a private laboratory in the rue Châteaubriand where he investigated x-rays and cultivated fruitful relationships with French industrialists. Louis's immersion in this world of hands-on physics played as important a role in his development as the courses he took from Paul Langevin.

Beginning from a completely different perspective, and operating under a mistaken impression of Einstein's epistemology, Werner Heisenberg sought to construct a physics of quanta that operated only with objects susceptible of actual measurement. Thus, Bohr's un-seeable electron orbits were to be banned. One was to operate entirely with transition rates and line strengths for the various atomic states, renouncing any goal of building a visualizable picture. In the summer of 1925 this resulted in Heisenberg's quantum mechanics, in the form usually called matrix mechanics. Its principles were developed rapidly by Heisenberg, Max Born, Pascual Jordan and Wolfgang Pauli.

But, as is so often the case in physics, it turned out that there was more than one way to do it. In November, 1925, Erwin Schrödinger gave a report on de Broglie's thesis about matter waves in the fortnightly colloquium at Zurich. At the end of the colloquium, according to the recollection of Felix Bloch, Pieter Debye remarked that it was rather childish to talk about waves without having a wave equation, as he had learned in Arnold Sommerfeld's course [5]. A few weeks later, Schrödinger had found his equation. In a series of famous papers, he laid out almost the entire structure of nonrelativistic quantum mechanics – the wave equation, the solution of the hydrogen spectrum as a series of eigenvalues of the wave equation, the development of perturbation theory and its application to a host of traditional problems of the old quantum theory [6].

In Heisenberg's circle, Schrödinger's wave equation aroused suspicion and distaste. These waves, which were the continuous solutions of partial differential equations, seemed too much like the classical apparatus that Heisenberg wished to banish from the world of the atom. When Schrödinger succeeded in proving the equivalence of his wave mechanics to Heisenberg's matrix mechanics, a cloud was lifted. The structure of quantum mechanics was completed very rapidly, with Born's probabilistic interpretation in 1926, Heisenberg's uncertainty paper in 1927, and Dirac's relativistic electron theory in 1928.

As is well known, the founders of quantum mechanics had profound disagreements about the meaning of their subject and the best course for its development. The Copenhagen school of Bohr, Heisenberg, Born and Pauli insisted on the impossibility of picturable mechanisms and proclaimed that quantum mechanics was complete. For them, quantum mechanics was an oracle that spoke only in probabilities and nature itself possessed features that were fundamentally discontinuous. Others still hoped for a deeper explanation of the phenomena that lay behind the successful equations of quantum mechanics. Schrödinger, committed to a world of continuous waves, doubted the very existence of particles and questioned the reality of Bohr's quantum jumps. In Chapter 5 of this volume, Michel Bitbol discusses Schrödinger's views with great clarity and points out that Schrödinger had many well-considered reasons – scientific as well as philosophical – for not believing in particles.

A famous showdown between the Copenhagen school and its doubters occurred at the Solvay Congress of 1927. de Broglie presented a version of his pilot wave theory that sought to represent particles as singularities of the wave. This theory was vigorously, some say ferociously, attacked by Heisenberg, who saw it as a sliding back into discredited classicality. de Broglie soon abandoned his theory and taught orthodox Copenhagen quantum mechanics in his courses. de Broglie gave up on pilot waves, no doubt partly because of his failure to win over his contemporaries, but most of all because he could not find a way to surmount its mathematical difficulties. In Chapter 4, Georges Lochak, once a student of Louis de Broglie, offers a personal account of de Broglie's relations with Heisenberg, which were warmer and more respectful than is often said. His chapter charmingly and insightfully sketches the differences in their personalities as well as in their attitudes to explanation in physics. Plutarch would have liked this addition to his Parallel Lives.

Accommodation, development and application proceeded rapidly. To an extent not appreciated by many today, the logical and conceptual framework of quantum mechanics was strongly influenced by John von Neumann's *Mathematical Foundations of Quantum Mechanics* of 1932 [7]. It was von Neumann who introduced the Hilbert-space formalism that is now standard in the textbooks, and who insisted on the importance of clear axiomatization. Von Neumann also introduced a simple mathematical model of measurement, in his analysis of how quantum states are amplified to yield macroscopic results. This initiated a whole line of investigation into the measurement process that continues to the present day. In Chapter 12, Roland Omnès offers a critical review of von Neumann's project, and its influence, for good and ill, on the history of quantum mechanics. Omnès concludes with an explanation of how the microscopic–macroscopic divide is dealt with in the recently developed language of consistent histories and decoherence. The axioms of measurement of the Copenhagen School are vindicated, but now emerge as theorems, good for all practical purposes, rather than as pronouncements *ex cathedra.*

1.2 Determinism, Entanglement, Locality, and the Quantum–Classical Divide

Before we introduce the remaining chapters in this volume, it will be helpful to sketch the intellectual background against which they should be viewed. The issues can be described in simple terms, but are quite serious. Let us imagine a simple system – a single electron, which can be in a state with its spin vector "up" along the z-axis, or "down" along the z-axis. We denote these two states by Dirac vectors:

$$|\uparrow\rangle \text{ spin up along } z\text{-axis}$$
$$|\downarrow\rangle \text{ spin down along } z\text{-axis.}$$

These state vectors are orthogonal, in the sense that if we know that the particle is in state $|\uparrow\rangle$, then it has zero probability of being in state $|\downarrow\rangle$. The orthogonality of the two states is often expressed by noting that the inner product of the two vectors is zero:

$$\langle\uparrow|\downarrow\rangle = 0. \text{ (orthogonality)}$$

A single electron is only a two-state system and so the two vectors $|\uparrow\rangle$ and $|\downarrow\rangle$ "span the space". This means that all possible states of the system can be written as linear combinations of these two basis states. Thus, the most general state is

$$a\,|\uparrow\rangle + b\,|\downarrow\rangle,$$

where a and b are (possibly complex) numbers. We assume unit normalization, so that

$$|a|^2 + |b|^2 = 1. \text{ (normalization)}$$

So far, there is nothing non-classical in the mathematical description. Linear combinations of basis vectors occur in many branches of classical physics. For example, the velocity of a particle can be represented by velocity components along orthogonal axes.

The essentially quantum-mechanical features arise from the axioms of measurement. If a measurement is made of the electron spin along the z-axis, only two possible results can be obtained: *up* $\uparrow$ or *down* $\downarrow$. Furthermore, if the system has been prepared in the state $a\,|\uparrow\rangle + b\,|\downarrow\rangle$, in standard quantum mechanics it is impossible *in principle* to predict whether the result of a single measurement will be $\uparrow$ or $\downarrow$. When the measurement is made, the system is forced to choose, as it were, one of the two answers $\uparrow$ or $\downarrow$. This is the famous "collapse of the state vector".

Before the measurement is made, the system is somehow potentially in both states; but when the measurement is made, the state vector collapses from $a\,|{\uparrow}\rangle + b\,|{\downarrow}\rangle$ to *either* $|{\uparrow}\rangle$ *or* $|{\downarrow}\rangle$.

Moreover, the coefficients a and b determine the probabilities of the two possible outcomes. Thus, the probability of getting $\uparrow$ is $|a|^2$ and the probability of getting $\downarrow$ is $|b|^2$. Let us associate the value 1 with result $\uparrow$ and the value -1 with result $\downarrow$. Then the mean value of a large number of measurements made on identically prepared systems will be $|a|^2 - |b|^2$. In quantum mechanics we must abandon the classical view of determinism. We are used to saying that there must be some reason why things turn out one way rather and another. (This is what Leibniz called "the principle of sufficient reason".) But in standard quantum mechanics, no reason can ever be given for why one particular measurement on an electron prepared in state $a\,|{\uparrow}\rangle + b\,|{\downarrow}\rangle$ gives $\uparrow$ rather than $\downarrow$. Of course, quantum mechanics remains deterministic in certain other ways. The probabilities of the two outcomes are predictable. And, if the electron is placed in a magnetic field, state $a\,|{\uparrow}\rangle + b\,|{\downarrow}\rangle$ will evolve in a deterministic way into another state with different values of a and b. But the outcomes of individual measurements remain indeterministic.

We have said that, before measurement, a system that has been prepared in state $a\,|{\uparrow}\rangle + b\,|{\downarrow}\rangle$ is somehow potentially in both states $|{\uparrow}\rangle$ and $|{\downarrow}\rangle$. Although competent quantum mechanicians will not disagree about the results of calculation based on such a state of affairs, or about the measurement results that might be expected, they may disagree profoundly about the nature of this unresolved potentiality.

Is it the case that the system is really in one state or the other, and that we simply do not know which one? This would be an example of a hidden-variable theory, in which it is assumed that there exists information unavailable to us (and perhaps unavailable in principle) that completes the specification of the physical state of the system. But the fates have not been kind to hidden-variable theories.

Is it the case that the system begins in the state $a\,|{\uparrow}\rangle + b\,|{\downarrow}\rangle$ and that the collapse to, say, $|{\uparrow}\rangle$ during measurement is an actual physical process that follows its own dynamical laws? In this case, the dynamical laws of quantum mechanics itself would be incomplete, and it would be necessary to seek out laws that might possibly govern the collapse of the state vector, and to find means of testing these conjectures.

Is it the case that mind plays an essential role in defining the state of the universe in the process of measurement and apprehension? In this scenario, the system has no definite state until a conscious mind (or some other object of measurement and apprehension) brings it into being.

All of these possibilities, and stranger ones besides, were maintained by distinguished physical thinkers in the course of the twentieth century. Of course, for most practicing physicists, the working position is one of agnosticism. In the daily practice of theoretical and experimental quantum physics, it simply doesn't matter what the underlying reality is, or even if there is one. Most physicists have always followed a dictum made popular by David Mermin: "Shut up and calculate!" [8]

But the problems become all the more strange when we include entanglement – another fundamentally non-classical feature of quantum-mechanical systems. Now we will need to consider a system consisting of two electrons that were once close together and interacting with one another, when they were prepared in a single state of the joint system. Let us define some terms:

$|\uparrow\rangle_1$ means "particle 1 is spin up along the z-axis"
$|\downarrow\rangle_2$ means "particle 2 is spin down along the z-axis",

and so on. The direct-product state

$$|\phi\rangle = |\uparrow\rangle_1 \, |\downarrow\rangle_2$$

describes a simple possible state of the joint system: particle 1 spin up and particle 2 spin down. Another obvious direct-product state

$$|\chi\rangle = |\downarrow\rangle_1 \, |\uparrow\rangle_2$$

has particle 1 spin down and particle 2 spin up. But direct-product states do not exhaust the space of possibilities for our system of two particles. Indeed, a linear combination of $|\phi\rangle$ and $|\chi\rangle$ is also a possible state of the system, for example the state

$$|\psi\rangle = \frac{1}{\sqrt{2}} \, |\uparrow\rangle_1 \, |\downarrow\rangle_2 - \frac{1}{\sqrt{2}} \, |\downarrow\rangle_1 \, |\uparrow\rangle_2 .$$

State $|\psi\rangle$ is an *entangled state.* (The factors $1/\sqrt{2}$ are for normalization – like the a and b mentioned above.)

Now, entangled states turn up all the time in classical physics, so there is nothing especially strange about the mathematical form of state $|\psi\rangle$. For example, if we need to solve for the electric potential on the surface of a two-dimensional conductor that lies in the x-y plane, we typically expand the mathematical expression for the potential into a sum of products of functions: $F(x)G(y) + H(x)I(y) + J(x)K(y) + \ldots$, an expression of the same form as our quantum-mechanical state $|\psi\rangle$.

As before, the quantum weirdness comes in when we apply the axioms of measurement. Let us see just what entanglement entails in the case of state $|\psi\rangle$. The properties of particles 1 and 2 are entangled in the following sense. We cannot know in advance what result we will get if we measure the spin of particle 1. Indeed, particle 1 has a 1/2 chance of being found spin up and a 1/2 chance of being found spin down. (1/2 is the square of the coefficient $1/\sqrt{2}$.) The odds for particle 2 are just the same. However, once we measure the spin component of particle 1, we can say with certainty what the spin component of particle 2 must be if *it* is measured later. For, if we know that particle 1 is spin up, then it is clear that the state of the joint system has collapsed from $|\psi\rangle$ to $|\uparrow\rangle_1 \, |\downarrow\rangle_2$. So particle 2 will be found to be spin down, with 100% certainty.

Entangled states popped up early and often in the history of quantum mechanics. But it was a famous 1935 paper of Schrödinger that drew particular attention to the paradoxical properties of these states and that, in fact, introduced the term entanglement [9]. Entangled states can easily be made to outrage our classical sense of propriety.

First, let us consider the effect of entanglement on the quantum-classical divide. Let there be a cat in a closed box containing a vial of toxic gas. Inside the box there is also an unstable atom, which can undergo radioactive decay. If the atom does decay, this is sensed by a detector, which is wired to break the glass vial and release the gas, which will, unfortunately, kill the cat. The atom has two possible states

$$
\begin{aligned}
&|o\rangle \text{ atom has not decayed} \\
&|x\rangle \text{ atom has decayed,}
\end{aligned}
$$

and the cat has two possible states,

$$
\begin{aligned}
&|A\rangle \text{ cat is alive} \\
&|D\rangle \text{ cat is dead.}
\end{aligned}
$$

But, obviously, the states of the atom and of the cat are not uncorrelated. If we know that the atom has not yet decayed, the cat must be alive and the state of the whole system is

$$|o\rangle|A\rangle.$$

On the other hand, if we know that the atom has decayed, then the cat must be dead and the state of the system is

$$|x\rangle|D\rangle.$$

The most intriguing situation occurs if we do not know the state of either the atom or the cat. (Remember that the box is closed so that we cannot look inside.) Let us suppose that the experiment has been running for one half-life of the unstable atom. That means that the atom has a 1/2 chance to have decayed already and a 1/2 chance of still being intact. Then the state of the system is the entangled state

$$|S\rangle = \frac{1}{\sqrt{2}}|o\rangle|A\rangle + \frac{1}{\sqrt{2}}|x\rangle|D\rangle.$$

The cat is in a superposition of states – and we can't know whether it is alive or dead until we open the box and make a measurement.

This is the famous "Schrödinger cat paradox". Here's what makes it a paradox: in our experience, cats are not quantum-mechanical objects that are somehow potentially both alive and dead. The world of classical experience does not appear to follow the quantum-mechanical axioms of measurement. But every physics experiment performed on a microscopic, quantum-mechanical object (such as our unstable atom) must also entail the use of macroscopic measuring instruments (meters, oscilloscopes, cats, etc.). The states of the classical measuring instrument *must* somehow be correlated with the states of the microscopic quantum-mechanical object. And if the microscopic object can be in a superposition of potential states, this seems to be required of the macroscopic instrument as well. The rules of quantum mechanics threaten to ensnare us in absurdity when they are pushed across the quantum-classical divide.

One way out of this difficulty was to accept the divide between the quantum and classical realms as a real aspect of nature, absolute and uncrossable. This was the position taken by the Copenhagen school of Niels Bohr. For Bohr, the description of real experiments entailed the existence of a classical world in which the experimenter resides with his or her instruments and which conforms to human intuitions based upon ordinary experience. But then it is not so easy to say what the cat is up to before the collapse of the state vector, or to explain what is wrong with the construction and interpretation of the entangled state $|S\rangle$.

Another way out of the difficulty is to renounce any divide between the quantum and classical realms as artificial. One must then accept that even a macroscopic object like a cat can be in a superposition of states. Since we have no idea what a superposition of a live and a dead cat might be like, one is then faced with the challenge of explaining in detail how the world of classical experience emerges from such a paradoxical state of affairs. Recent experiments have successfully produced macroscopic manifestations of quantum-mechanical phenomena. It has long been routine to use beams of atoms to demonstrate quantum-mechanical superposition and interference. An atom, with dozens of protons and neutrons in its nucleus and electrons orbiting about it is already far from a simple thing. But a real divide has been crossed by the most recent experiments. In 2000, J. R. Friedman's group reported the quantum superposition of two states of a SQUID (superconducting quantum interference device) that differed in their magnetic moments by 10^{10} Bohr magnetons [10]. Since the Bohr magneton is roughly the size of the magnetic moment of individual particles or atoms, this does truly represent a macroscopic effect. And in 2001, B. Julsgaard and collaborators reported entangling a pair of cesium gas clouds containing 10^{12} atoms each [11]. The quantum-classical divide does seem to be dissolving before our eyes.

Yet another form of quantum weirdness – nonlocality – can be developed by thinking about entangled states. Let us begin with our pair of electrons in the entangled spin state

$$|\psi\rangle = \frac{1}{\sqrt{2}}\,|\uparrow\rangle_1\,|\downarrow\rangle_2 - \frac{1}{\sqrt{2}}\,|\downarrow\rangle_1\,|\uparrow\rangle_2.$$

We suppose that these particles were put into this state when the particles were close together and interacting. But now let the particles travel, each on its own trajectory, until they are very far apart and are no longer interacting.

Suppose now that an experimenter, Alice, measures the spin of particle 1 along the z-axis and finds it to be $\uparrow$. Then, if another experimenter, Ted, located far away, later measures particle 2, he is bound to get $\downarrow$ with 100% certainty. This seems to be in conflict with the notion that particle 2 was at first potentially in both states. How could a measurement on electron 1, perhaps miles away from electron 2, suddenly determine which state electron 2 is in? Doesn't this mean that electron 2 was really in state $|\downarrow\rangle$ all along and Ted just didn't know it? This would amount to a hidden-variable theory. And, so far, we could maintain a semi-classical picture of this sort. But now things are going to get awkward for this point of view.

The basis vectors $|\uparrow\rangle$ and $|\downarrow\rangle$, which stand for spin up along the z-axis and spin down along the z-axis, are not the only one we can use, for there is nothing special about the z-axis. We could instead choose to measure everything with respect to the x-axis. Let us therefore define the following states:

$|{\rightarrow}\rangle_1$ means "particle 1 is spin up along the x-axis"
$|{\leftarrow}\rangle_2$ means "particle 2 is spin down along the x-axis",

These two states also span the space of all possibilities for a single electron. This means that any other state (including the states that are spin up or down along the z-axis) must be expressible in terms of these x-states. Indeed, it turns out that

$$|{\uparrow}\rangle = \frac{1}{\sqrt{2}}\,|{\rightarrow}\rangle + \frac{1}{\sqrt{2}}\,|{\leftarrow}\rangle$$

$$|{\downarrow}\rangle = \frac{1}{\sqrt{2}}\,|{\rightarrow}\rangle - \frac{1}{\sqrt{2}}\,|{\leftarrow}\rangle$$

If we make similar decompositions for both electron 1 and electron 2 then substitute these expressions into the expression for our usual entangled two-particle state,

$$|\psi\rangle = \tfrac{1}{\sqrt{2}}\,|{\uparrow}\rangle_1\,|{\downarrow}\rangle_2 - \tfrac{1}{\sqrt{2}}\,|{\downarrow}\rangle_1\,|{\uparrow}\rangle_2 \text{ (first form)}$$

we find that $|\psi\rangle$ can also be expressed in the form

$$|\psi\rangle = \tfrac{1}{\sqrt{2}}\,|{\leftarrow}\rangle_1\,|{\rightarrow}\rangle_2 - \tfrac{1}{\sqrt{2}}\,|{\rightarrow}\rangle_1\,|{\leftarrow}\rangle_2 \text{ (second form)}$$

These two forms for $|\psi\rangle$ are mathematically equivalent and represent the same physical state of the entangled two-electron system. The only difference is that in the first form we have expressed everything in terms of basis vectors that are spin up or down along the z-axis, while in the second form we have used basis vectors that are spin up or down along the x-axis. Note that, either way you look at it, $|\psi\rangle$ is a state of total spin zero.

Now, suppose that Alice decides to measure the spin of particle 1 along the x-axis (instead of along the z-axis as in the earlier example). We can't predict what she will get: either $\rightarrow$ or $\leftarrow$ with equal probability. Let's say she gets $\rightarrow$, that is spin up along the x-axis. Once she has done this, the entangled two-particle system collapses to $|{\rightarrow}\rangle_1\,|{\leftarrow}\rangle_2$. Thus, as far as Ted is concerned – located far away – his particle 2 is bound to behave in every respect as if it is spin down along the x-axis. A decision made by Alice (whether to measure particle 1 along x or along z) seems to affect Ted's particle 2, without Alice having done anything at all to particle 2.

We are faced with a disturbing *nonlocality* in the nature of quantum mechanics. Two entangled particles maintain their entanglement even if they are separated to great distances and they seem to be able to "interact" without any regard for the speed limit imposed by the theory of relativity. Something that happens here to one of them suddenly, without time for propagation of any signal between them (even at the velocity of light), determines the state of the other. The paradoxical character of a similar thought experiment was developed forcefully by Einstein, Podolsky and Rosen in a famous paper of 1935 [12]. (The details of their thought experiment were a bit different and involved momentum states, rather than spin vectors. The simpler and more convenient expression of the paradox in terms of spin states was introduced by David Bohm [13].) Einstein, Podolsky and Rosen were answered, obscurely, by Bohr [14]. Copenhagen quantum mechanics was not disturbed, and the real issues suggested by Einstein, Podolsky and Rosen did not receive adequate attention for nearly three decades.

1.3 Quantum Mechanics in the Baroque Age

One of von Neumann's accomplishments was a celebrated proof of the impossibility of hidden-variable theories. But the proof turned out to have some loopholes. In 1952, David Bohm succeeded in producing a successful theory of the kind deemed to be impossible [15]. Bohm's program amounted to a sort of revival of de Broglie's pilot wave theory. The key thing that such a theory offered was an explanation of the fact that a measurement gives a *particular* result. Before measurement, the wavefunction contains a multiplicity of potential outcomes. In Copenhagen quantum mechanics, it is the measurement process itself that produces a definite outcome. The attraction of Bohm's theory was that it explained measurement as the disclosure of a really existing classical state of affairs rather than as a mysterious collapse of the wave function. In its technical details, Bohm's theory was but a clever decomposition of the Schrödinger equation. Its predictions differed not at all from those of standard quantum mechanics and the theory could not be extended to the relativistic case. Since Bohm's theory offered nothing new in the way of predictions, but only a new "interpretation," it fell on deaf ears. Copenhagen quantum mechanics was securely established and few were interested in reconsidering its foundations [16].

The new, baroque period of quantum mechanics can be considered to begin with John Bell's papers of the 1960s on the Einstein–Podolsky–Rosen paradox and quantum-mechanical correlations [17]. Some years later, Bell related how shocked he had been when in 1952 he read Bohm's papers, and thus learned, belatedly, of de Broglie's pilot wave theory of 1927. He was outraged that none of his teachers had even mentioned the existence of de Broglie's attempt at a "realistic" quantum mechanics [18]. Bell's papers on quantum-mechanical correlations established conditions (the "Bell inequalities") which, it is claimed, any local hidden variable theory would have to satisfy, but which might be violated by actual quantum mechanical systems. Experiments, first by Freedman and Clauser [19] in 1972, but then by many others, have consistently upheld the predictions of quantum mechanics and made it harder and harder to sustain any sort of local hidden variable theory, except by special pleading or ingenious loopholes.

One loophole that might rescue locality involves a mysterious possible communication between particles 1 and 2. In this scenario, when Alice makes her measurement on particle 1, thus collapsing the state vector, particle 1 sends out a subluminal (slower than the speed of light) signal that reaches particle 2 and tells it how to behave before Ted has a chance to measure it. However, experiments by Aspect, Dalibard and Roger [20] (and subsequently also by others) have closed the subluminal communication loophole. Nonlocality seems to be here to stay. (However, a pilot-wave theory of the de Broglie–Bohm type is not excluded by these tests, for these are highly nonlocal theories.) In Chapter 6 volume, Abner Shimony presents a new version of the Einstein–Podolsky–Rosen argument, states and proves a generalization of Bell's theorem, and gives a brief review of the experimental evidence on the question. Shimony concludes that a deeper physics is still needed to explain the brute fact of nonlocality.

An important effect of Bohm's work was to stimulate new interest in the foundations of quantum mechanics. Slowly it dawned on people that, while the rules of Copenhagen quantum mechanics certainly worked, there might still be problems in understanding why. As a result, the climate of opinion slowly, but ultimately quite radically, changed. In the early 1960s only a tiny minority of physicists bothered with such questions. I was a graduate student in physics in the mid and late 1970s. Even at that date, not one of my professors or textbooks paid the least attention to questions of the foundations of quantum mechanics. Now the foundations of quantum mechanics is a thriving field, with its own journals and conferences. Now, practically all the textbooks, even at the undergraduate level, make at least a passing comment on the burgeoning of multiple points of view and the fact that serious issues are at stake beyond mere "interpretation". A recent paper listed nine different "formulations" of quantum mechanics, as well as several "interpretations," including the many-worlds interpretation of Everett and the transactional interpretation of Cramer [21]. This is a clear sign of the baroque.

As we have mentioned, one of the unsatisfactory aspects of Copenhagen quantum mechanics was its artificial divide between the quantum-mechanical realm and the classical world of ordinary experience. Recently, much theoretical and experimental work has focused on just how the world of ordinary experience emerges from the bizarre quantum world of entangled and superposed states. The notion of decoherence has been key to many of these efforts. According to this view, a macroscopic object, such as a voltmeter, which is entangled with a quantum-mechanical object that is subject of measurement, does indeed exist in a superposition of states. But a macroscopic object has a huge number of degrees of freedom. The large number of degrees of freedom in the state vector leads to a violent oscillation in phase, which implies rapid loss of coherence and the disappearance of interference effects. The world of ordinary experience emerges rapidly with the onset of decoherence, with the result that the live cat cannot interfere with the dead cat. It is for this reason that quantum interference effects are typically seen clearly only when the microscopic object is carefully isolated from its environment. In Chapter 7, Maximilian Schlosshauer and Arthur Fine give a broad overview of decoherence and explain how it functions in several different approaches to quantum mechanics.

A second unsatisfactory aspect of Copenhagen quantum mechanics was its reticence. In the standard interpretation, quantum mechanics answers questions about the probabilities of obtaining such and such a value for such and such a measurement in such and such an experiment. A particle released from location a at a certain time has a certain probability of being detected at location b at some time later. This is a rather restricted view. It has nothing to say about quantities that aren't measured as part of the experiment, such as locations at intermediate times. It as if the quantum mechanicians had taken to heart Ludwig Wittgenstein's admonishment at the end of the *Tractatus Logico-Philosphicus*: "What we cannot speak about we must pass over in silence." Over the last two decades, a new interpretation of quantum mechanics has been developed that addresses these restrictions. The new interpretation, called "consistent histories" was pioneered by R. B. Griffiths and further developed by Roland Omnès and James Hartle. Consistent histories play an important role in current discussions of the foundations and interpretation of quantum mechanics, not least by helping us sharpen our thinking about what constitutes a meaningful question and what does not. In Chapter 8, Alan Thorndike provides an introduction to the notion of consistent histories and gives a feeling for how the mathematics works by examining a few simple examples.

Far from being a closed subject experimentally, quantum mechanics is in the middle of boom. Features of quantum mechanics, such as entanglement, that once were regarded as disturbing oddities are now being systematically exploited, and may perhaps lead one day to practical applications. The last two decades have seen a rapid rise in interest in quantum computing and in quantum communication, in which the quantum-mechanical properties of microscopic objects can be used to advantage as an essential parts of the computing or communication processes. In Chapter 11, William K. Wootters provides a reader-friendly introduction to quantum entanglement as a resource for communication, and explains the theoretical basis for dense coding, the pooling of separated information, as well as quantum teleportation.

Wave–particle duality is one of the mantras of quantum mechanics. This is shorthand for the fact that light (as well as electrons) can manifest wave-like properties or particle-like properties, depending on what the experimenter asks it to do in the course of an experiment. If light is passed through a slit, it spreads out by diffraction in wave-like fashion. But if the light in the diffraction pattern is dim enough, it becomes clear that the light arrives particle-by-particle at the detectors. Until recently, however, it seemed that, for the purposes of a single process in a given experiment, one could always get away with thinking of light as acting *either* as a particle *or* as a wave. This represented a sort of modified classicality. However, recent experiments in quantum optics have stolen even this comfort from us. In the new experiments with light, correlations are demonstrated between particle- and wave-like properties, so that the either-or point of view has to be abandoned, at least in the analysis of certain kinds of experimental operations. In Chapter 10, Howard J. Carmichael presents a step-by-step introduction to the new situation, by drawing on a series of actual and proposed experiments.

Few recent experiments have attracted so much notice as the long-awaited production of Bose–Einstein condensation. Bose–Einstein condensation is possible for the particles called bosons, i.e., those with spin angular momentum in integral multiples of $\hbar$. Bosons have the remarkable property that they are not subject to the Pauli exclusion principle. At low enough temperatures, identical bosons can all climb down into the same state. The resulting material – a Bose–Einstein condensate – is a new state of matter with remarkable properties. This new state of matter was predicted by Einstein in 1925, but for many years it seemed that its only manifestation might be in condensed-matter physics, namely in liquid helium. It was only in 1995 that Bose–Einstein condensation was achieved with dilute gases of alkali atoms, first by a group at Boulder led by Eric A. Cornell and Carl E. Wieman, and then by Wolfgang Ketterle's group at MIT. (Cornell, Wieman and Ketterle shared the Nobel Prize for this work.) In Chapter 9, Wolfgang Ketterle presents a marvelously clear and patient explanation of what Bose–Einstein condensation is, how it depends on quantum statistics, how you go about producing a Bose–Einstein condensation experimentally, and how you know that you've done it. Prof. Ketterle's chapter provides a useful and accessible introduction to this fascinating field.

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2

Max Planck's compromises on the way to and from the Absolute

J. L. Heilbron

2.1 Scientific

Max Planck was a physicist by profession, not by birth. He had a talent for the piano that could have supported a career in music. He had an interest in several academic subjects, any one of which might have occupied his mind and time. As a university student in Munich around 1880, he narrowed his choice to three scientific fields, each of which had the merit in his eyes of dealing with fundamental laws and generalizable problems. Physics needed no special justification on this count: until our times it has vied with the bible as the fundamentalist's choice. Philology held the promise, according to German linguists of Planck's time, of revealing the universal laws of human communication. And in history, the record of mankind's achievements and stupidities, the perceptive Geschichtswissenschaftler saw not just one damn thing after another, but a data bank for the discovery of the norms of human behavior and the laws of social development [1].

Inspiring lectures by the professor of mathematics at Munich tilted the balance in favor of physics. Planck ruled out pure mathematics as pure indulgence. He would pamper himself enough by using mathematics to write the laws of nature. He gave this Pythagorean bromide an unusual twist in conceiving, and acting on the conception, that intelligent beings everywhere in the universe would mathematize nature in the same ways. Thus his life-long task as he saw it as a young man was to approach truth by shedding all the parochial trappings of science, all the models based on human preferences and contingencies, all traces of the circumstances in which his thought developed. The remainder would be a Weltbilt, a world picture, intelligible to all creatures capable of mathematical reasoning. With this program he was to become one of the world's first theoretical physicists, a pioneer in the romantic high ground later cultivated by Einstein and Bohr.

Planck chose as the subject of his doctoral thesis of 1879 the second law of thermodynamics, then even less perfectly integrated into the body of physics than it is now. It drew him because of its generality. He understood it to require that all natural processes, without exception, in all times and places, occur in the direction of increasing entropy. His thesis, written in three months at the age of 21, declares at the beginning, "The following considerations relate to all conceivable natural processes, not just the subject of heat." In his Habilitationsschrift the following year, he came down from these clouds to discuss the temperature equilibrium of isotropic bodies, without, however, making any assumptions about the constitution of matter. For another twenty years he drove his physics as far as he could without recourse to atoms or molecules; he treated matter as a black box long before he took up the problem of the black body [2].

Around the time that Planck finished at the university, he, the mathematician Carl Runge, and two of their friends started a round-robin diary, in which each would write in turn about the matters that then concerned him. This *Brieftagebuch*, the remains of which were published only recently, offered its contributors opportunities for spontaneity. Planck seldom seized them. Here is an example of his most relaxed style: "I believe that it is more important to be clear on a few fundamental theorems . . . even if the inspiration of the moment must often find the way in the details." With these playful words he introduced his friends not to Planck's quantum of action but to the action of a new Planck quantum. The fundamental theorems concerned child rearing. Planck got them from Herbert Spencer [3]. The quantum that eluded their application was Planck's infant son Karl.

When he had wrung everything he could from thermodynamics, Planck turned to electrodynamics as newly unified through Maxwell's equations. That was around 1890, just after Heinrich Hertz's demonstration of man-made electromagnetic waves. Planck was drawn to the field – in both senses of the word – by the wide scope of its principles and the challenge of unfinished business. As he wrote in the *Brieftagebuch* in 1890, "For me the construction of the theory of electricity is now the most attractive subject in physics, since there is so much for the theorist to do; everywhere there are interesting questions of principle; because of the abandonment of action at a distance and the introduction of the energy concept everything must be turned upside down." [4] In the event, Planck did not deepen or widen the principles but applied them to a problem apparently made for him.

The problem was the calculation of the energy density in black-body radiation as a function of temperature and color. Though tough, it appeared to be well defined and solvable within the framework of the physics he knew. Black-body radiation is an equilibrium distribution. Planck knew better than anyone how to describe equilibrium via the concept of entropy. Moreover, the new electrodynamics gave a complete description of heat radiation. All he needed to do was to obtain an expression for the energy density as a function of temperature and frequency and find a way to apply the entropy condition to drive it to equilibrium. The problem had the further attraction to him that its solution did not depend on the size, shape, or material of which the cavity was made, and that its solution might offer clues to an understanding of the irreversibility described by the second law. In short, the black-body problem was a choice challenge, like scaling a mountain peak just within one's competence. Planck liked to climb mountains for the "satisfaction of overcoming difficulties in reaching a pre-assigned goal." [5] He wrote these last words in the communal diary in response to a report from one of the friends about a boating holiday. Planck regarded boating as a pastime "de gustibus," by which he meant "for sissies."

Planck started his scaling of the black body after Wilhelm Wien had proved by a brilliant argument that the conservation of energy and the pressure of radiation taken together required that the distribution function $u(\nu)$ have the form $\nu^3\phi(\nu/T)$. (u is the energy density of the field, per unit volume and per unit frequency interval, ν is the frequency, T the temperature, and ϕ an unknown function.) All students of the old radiation laws know the argument. Wien considered radiation contained in an enclosure having a perfectly reflecting mirror as one wall. The mirror moved slowly toward the opposite wall, compressing the radiation, shifting its frequencies, and altering the energy distribution. Electrodynamics gave the work done in the compression of the radiation. Equating the work with the Doppler-shifted distribution, Wien found a differential equation whose solution must have the form just given. This result withstood the quantum theory.

Like Planck, Wien had a strong reason to want to know exactly how the spectral distribution depended on ν/T. Wien possessed one of the very few black bodies in the world. It was a thing of beauty, of porcelain and glass, a heavy insulated cylinder with interior reflecting walls maintained at a constant temperature. It communicated with the world via a hole from which radiation could escape for measurement. This expensive piece of apparatus belonged to the Physikalisch-Technische Reichsanstalt (PTR), the federal bureau of standards, an institution then as unique as Wien's black body, a testimony to the unifying force of science and technology in the new imperial Germany. Wien worked in the physics division of the PTR during the early 1890s.

The primary mission of the PTR was to support high-tech industry by developing standards and testing products. The study of black-body radiation there in the 1890s related to the emerging electrical lighting industry. The PTR sought a reliable measure of the efficiency of the various lamps submitted to it. The black-body spectrum made a good reference point since it gives the least illumination for a given amount of heat. Planck pursued the black-body spectrum as a contribution to interplanetary enlightenment. The experimenters at the PTR pursued the spectrum as the worst possible source of domestic illumination [6].

The PTR's black-body group and their colleagues at the Technische Hochschule in Berlin proposed several forms for the functional dependence of the radiation law on ν/T before Planck concocted the winner. At first he thought that Wien had succeeded with an exponential form constructed in analogy to Maxwell's velocity distribution in gases. Wien's formula did indeed account for most of the measurements made at the PTR, which were restricted to high values of ν/T.

Planck developed a thermodynamic argument in favor of Wien's formula by introducing the fiction of the resonator, a simple harmonic oscillator by which he modeled the mechanism that changed any radiation distribution admitted into the cavity to that of a black body. The mechanism had to have resonators at all frequencies, but since Planck concerned himself with equilibrium, not with getting there, he tended to confine his attention to the group of oscillators around a single frequency. The key step in his argument concerned the relationship between the time average energy U of a resonator of frequency ν and the energy density R of the cavity radiation at that frequency. The going was tedious but classical, essentially electrodynamical, and yielded the form

$$R(\nu, T) = \text{const.}\, \nu^2 U(\nu, T).$$

From Wien's earlier thermodynamic argument, $U(\nu, T) = \text{const.}\, \nu f(\nu/T)$.

To proceed to find f, Planck did what was as natural to him as to Nature, that is, he computed the equilibrium entropy of his resonator. If he could only find how the entropy depended on the average energy he was home, since he could then obtain U via the certain thermodynamic relationship $\partial S/\partial U = 1/T$. For technical reasons he sought the clue in the second derivative of the entropy with respect to the average energy,

$\partial^2 S/\partial U^2$. By working backwards from Wien's black-body formula, $U = \alpha\nu \exp(-\beta\nu/T)$, Planck could have found $\partial^2 S/\partial U^2 = -1/\beta\nu U$, which may have looked simple enough to be true. And so he published it, as the thermodynamic ground of Wien's formula [7].

While Planck searched high and low for a justification other than apparent success for his form for $\partial^2 S/\partial U^2$, measurements of the black-body spectrum in the infra-red, at low values of ν/T, became available. They agreed with the form of f, namely const. T/ν, deducible from the principle of the equipartition of energy, and now known as Rayleigh's formula. Planck responded by devising another form for $\partial^2 S/\partial U^2$. He interpolated between the forms needed to give Rayleigh's formula in the infra-red and Wien's formula in the ultra-violet. That produced $\partial^2 S/\partial U^2 = -1/[\beta\nu(1 + U/\beta\nu)]$ and Planck's formula for black-body radiation,

$$R(\nu, T) = \alpha\nu^2 U(\nu, T) = \frac{\alpha\nu^2\beta\nu}{\exp(\beta\nu/T) - 1}.$$

The formula agreed with experiment and has stood the test of time [8]. But it was scarcely the high peak at which the deep-thinking Planck had aimed. Chasing the Absolute he had arrived at a jerry-rigged compromise between two formulas neither of which he could derive from the first principles of thermodynamics and electrodynamics. To obtain something worthy of a theoretician from this ignominious compromise, Planck had recourse to an approach to heat theory that he had opposed for two decades.

Planck had rejected the statistical mechanics of gases pioneered by Maxwell and developed by Boltzmann. In expressing entropy as a measure of the probability of the distribution of mechanical quantities among the molecules of a gas, Boltzmann had had to allow entropy to decrease occasionally and locally. Sometimes a sample of gas might briefly devolve from a state of higher to one of lower probability. But the strict constructionist Planck could not permit backsliding of the sort that the reversibility of the laws of molecular motion made possible. In upholding the law of the increase in entropy without exception, he perforce rejected both Boltzmann's probabilistic representation and the molecular model that underpinned it. But in 1900, having failed to find a thermodynamic way to justify the relationship between entropy and energy he needed to derive his successful half-empirical formula, Planck tried where Boltzmann might take him.

Boltzmann's method applied to a material gas divides the entire available energy into a very large number of very small elements ϵ; supposes a distribution in which each of the N molecules in the gas bears a certain number of energy elements; and finds the distribution that can be realized in the largest number of ways by interchanging the molecules while keeping their total number and total energy constant. The expression kT enters in the integration of the differential equation that arises in determining the maximum distribution. The interpretation of kT comes from identifying the entropy with the maximum distribution. In taking over this procedure, Planck divided the energy possessed by the N resonators at frequency ν in the equilibrium situation into P units each of size ϵ, and he calculated the average entropy of a single oscillator from the ever-useful equation $\partial S/\partial U = 1/T$ and the average energy U recoverable from his black-body formula. Multiplying the average entropy of a single resonator thus found by N to obtain the average entropy for the collection of resonators at frequency ν, Planck had the suggestive formula

$$S_N = k \log \frac{(N + P\epsilon/h\nu)^{N+P\epsilon/h\nu}}{N^N (P\epsilon/h\nu)^{P\epsilon/h\nu}},$$

where the new constants are multiples of the old. The formula may not immediately suggest the next step to the modern reader; but to Planck, whose head was full of Boltzmann, it was plain as a pikestaff. Boltzmann expressed entropy as the logarithm of an unnormalized probability. To obtain an expression similar to those that occurred in the gas theory, Planck took k to be the gas constant per molecule and set $\epsilon = h\nu$. This last maneuver was not a revolutionary act but the obvious way to make the argument of the logarithm a whole number, as required by Boltzmann's combinatorics. For with this prescription for ϵ and Stirling's relation between powers and factorials, Planck had for the entropy

$$S_N = k \log \frac{(N + P)!}{N!P!}.$$

The argument of the logarithm now is (for large N and P) the number of ways P indistinguishable elements can be distributed among N distinguishable entities. Planck had only to turn the derivation around, introduce this distribution as the way to count elements among resonators, and take $\epsilon = h\nu$ as the means to couple the calculation to the measurements. He did not think that the stipulation about oscillator energy or the counting procedure deviated in any fundamental way from Boltzmann's approach [9].

Nonetheless, Planck understood that his formula and its theoretical justification contained something of great importance. Planck's younger son Erwin, who was seven in 1900, remembered his father's telling him then, "today I have made the greatest discovery since Newton" or perhaps – the story has variants – since Copernicus [10]. Planck probably had in mind the means of measuring the universal constants h and k and through them establishing the fundamental dimensions of the world picture. From the link between entropy and combinatorics, Planck could calculate k, the gas constant per molecule, and thence Loschmidt's or Avogadro's number and the value of the electronic charge. *Esse est computari.* Planck's new method of evaluating suppositious atomic constants confirmed his belief in the molecular picture to which he had turned in desperation when blocked in his quest for the black-body formula. As to the meaning of h apart from a route to k, Planck did not volunteer a conjecture. As Runge pointed out later, h at first was "not much more than a mathematical device." [11] It required several years and the intervention of Einstein and Lorentz to identify where Planck had violated the principles of electrodynamics as delivered by Maxwell and thermodynamics as interpreted by Boltzmann.

As the measurements supporting Planck's formula improved, so did confidence in the values of the atomic constants deduced from it. In 1908 the Nobel prize committees of the Swedish Academy of Sciences recommended Planck for the prize in physics and Rutherford for the prize in chemistry, both as rewards for their contributions to atomistics. In the joint decision of the committees, the agreement between the value of the electronic charge deduced by Planck from his radiation formula, and that found by Rutherford from counting alpha particles, figured prominently. Unfortunately for Planck, by then Einstein and Lorentz had discovered that if h had any value other than zero it menaced accepted physical theory. The Swedish Academy of Sciences took fright. Rutherford received his prize for chemistry, which mystified him, but not Planck the corresponding (and explanatory) prize for physics. He had to wait another decade before the tribunal in Stockholm rewarded the work that opened the way to the quantum theory [12].

Just after the "tragi-comedy" (as Planck called it) of the false report of the Nobel prize, he gave a lecture in Leyden at the invitation of Lorentz. He introduced himself to his first major audience outside Germany by setting forth his considered views about the nature of his work. Pointing to his pride and joy, that is, to h and k, he observed that physicists in the new century were continuing the unification of the world picture brought near by the electromagnetic theory of light and heat and the atomic-molecular concept of matter. Dimensionless universal constants could now be constructed that necessarily would have the same value for all physicists, human or not, irrespective of their systems of measurement. The discovery of the constants both sharpened and dehumanized the world picture.

In this respect the constants made common cause with the theory of relativity, which Planck recommended to his Dutch audience for its universalizing and dehumanizing values. These were among the reasons that he himself had championed relativity theory from the moment that he, as editor of the Germany's leading physics journal, had read Einstein's paper in manuscript. What disturbed most people about relativity, its rejection of ordinary intuitions of space and time, was to Planck its greatest attraction. The theory showed that a correct world picture could not be built on common intuitions, even on those in which the whole human race concurred, and that the theoretical physicist could transcend the limitations of his species [13]. Runge took the same point of view. "It is indeed a triumph [he wrote in the *Brieftagebuch* after complimenting Planck on an extension of relativity] that we have managed to overcome even so established a dogma as the constancy of mass." [14]

The most influential philosopher of science in Germany took an altogether different line. Ernst Mach, whose teachings had influenced Planck as well as Einstein, had emphasized the ineluctability of the human element in science. For him, the purpose of physics was to describe and predict economically what our senses would experience in any given situation, and to do so in the same general terms as the physiologist would use in accounting for psychological phenomena. The Machist cares nothing for the physics of Martians. Science begins and ends with human needs and capacities. It must avoid inhuman commitments, like metaphysics, and misleading fictions like matter and molecules. Only sense impressions and the laws deduced from them have any reality.

Planck took the occasion of his Leyden lecture to criticize Mach's poor interplanetary citizenship. In subsequent lectures and papers he impugned Mach's reputation as a physicist and lampooned his reduction of science to a calculus of sense impressions. No one, Planck said, had ever found anything worthwhile in physics by practicing Machist philosophy. Commitment to a world picture, particularly belief in atoms and molecules, not to colorless descriptions free from models and metaphysics, was needed to advance. It is with physicists as with prophets: "By their fruits ye shall know them." [15]

Planck's attack on Mach happened to coincide with an even more strident bombardment by Lenin, who castigated the worthy old man for subverting science and therefore dialectical materialism by abolishing matter from the world. "The philosophy of the scientist Mach is to science what the kiss of the Christian Judas was to Christ." [16] Planck's joining battle on the side of Lenin later made him a hero in East Germany when doubts about the correctness of his behavior during the Nazi period clouded his reputation in the West. More recently, Mach has been invoked as a champion of the individual in science and a symbol for resistance to thought control by establishment figures like Planck [17].

Some months before he attacked Mach, Planck admitted to his friends through the Brieftagebuch that his discoveries about cavity radiation had undermined the principles of physics. Responding in February 1908 to a question posed by Runge, Planck wrote:

> My ideas about the elementary quantum are still rather meager; but I can say that the dimensions of the quantum are not energy but "action" (energy times time) so that its complete explanation will come not from considerations of a state but from considerations of a process. In other words, we are dealing with atomism not in space but in time since processes that we used to consider as steady in time really show temporal discontinuities. Perhaps Minkowski's four-dimensional space ... can be applied successfully to representing the quantum of action. In any case I was interested that this natural constant remains invariant according to the relativity principle when transferring from a resting to a moving frame of reference although almost all other quantities like space, time, and energy change. It was just this fact that led me to a closer investigation of the relativity principle. I'm fully convinced that the problem of spectral lines [on which Runge worked] is intimately tied to the question of the nature of the quantum of action, as are all problems concerning processes in which very fast electromagnetic oscillations occur. There is no doubt that the laws of ordinary mechanics and electrodynamics, which always assume continuity in time, do not suffice in these circumstances [18].

Planck tried to make the needed departures from ordinary physics as small as possible. He developed a theory in which only the emission of radiation occurred in spurts, while absorption took place continually and classically. But as he rightly anticipated, the key to the quantum would be found in spectral lines; and his compromise asymmetry between absorption and emission, which had a brief success in a theory of the photo-effect worked out by Arnold Sommerfeld, did not long survive Bohr's inspired conjectures about the spectrum of hydrogen [19].

While he saved absorption for ordinary physics, Planck maintained his belief in the unrestricted validity of the second law. He first distanced himself publicly from that long-cherished view in a celebratory lecture he gave as rector of the University of Berlin on the anniversary of its foundation. The lecture had its drama. It took place on 2 August 1914, the day Germany declared war on France. Planck gave as the main reason for his change of mind the measurements on Brownian motion made by the Frenchman Jean Perrin [20].

Planck did not play a direct part in the creation of quantum mechanics. He helped behind the scenes by raising resources for academic science and directing some of them to Sommerfeld and Born for research in atomic physics. He warmed to quantum mechanics in its wave formulation by Erwin Schrödinger and did not oppose Born's probabilistic interpretation of the wave function. But he rejected firmly and frequently the introduction of acausality into the world picture and the subjective elements of complementarity and uncertainty. He regarded these doctrines not only as throwbacks to the dangerous dogmas of Mach, but also as a pessimistic and premature surrender to scientific difficulties. He chided Bohr, Born, Heisenberg, and their followers for confusing the sensory world, where the limitations of measurement do impose practical limitations on knowledge, with the world picture, which is a free creation of the human mind [21].

Planck needed only a slight shift in viewpoint to see the probabilistic interpretation of Schrödinger's waves as another major step on the road to the Absolute. Like relativity theory, the probabilistic interpretation deanthropomorphized physics, in its case by removing particles and their trajectories from the world picture. The loss could not have upset an interplanetary intellectual like Planck. Our long arms and love of fighting give us an exaggerated interest in projectiles. Schrödinger's waves did not consult human interests. They evolved imperturbably in accordance with a differential equation in the manner approved by terrestrial physicists since the 18th century. The new world picture as Planck painted it around 1930 contained no acausality or subjectivity and no indeterminism, provided the viewer did not ask for what it could not give. It could not furnish the future course of a particle. The particle belonged to another world picture. Bohr and his school erred in renouncing the possibility of world pictures different from their own. Instead of renunciation, they should be filled with gladness by the prospect of further erasures of the peculiarly human from their representations of reality [22].

2.2 Personal

Planck was twelve when Germany became a nation and began to aspire to Empire. When he became a professor, he swore an oath of allegiance to the Kaiser as did all civil servants. He took this oath, like everything else, seriously. He had no reason to think that he would regret it. He was entirely at home in the imperial capital. His family had served the state for generations as jurists and pastors. His father was a professor; his wife was the daughter of a banker; his brothers were lawyers. He enjoyed the academic social life of Berlin and the many opportunities it afforded to hear and play classical music. His tastes ran to German composers, not, as might be expected, Bach and Beethoven, but Schubert, Schumann, and Brahms. He loved the mountains of Bavaria and the park-like suburb, the Grunewald, in which he had his home. He rested his ideas of duty and decency, his ideals of truth and knowledge, and his sense of comfort and security, in the culture and institutions of the united Germany [23].

He therefore welcomed World War I. Yes, welcomed it. It countered the centrifugal forces that increasingly distressed him: party politics, labor unrest, and a loss of direction among the young, including his son Karl. As rector of the University of Berlin in 1914 he found much to admire in the events that had emptied his university of students and junior staff. "The German people has found itself again." He wrote to Wien just after leaving the rectorate that the war had brought more to applaud than to regret. "Besides much that is horrible, there is also much that is unexpectedly great and beautiful: the smooth solution of the most difficult domestic political problems by the unification of all parties [and]...the extolling of everything good and noble." [24]Karl, who had not been able to settle on any of the high callings expected of him, rushed to the colors and died at Verdun. Planck wrote in the Brieftagebuch: "[Karl] was among those made sound by the war. Never before had his condition and development given me such satisfaction as in those months in which he concentrated all his strength in dedication to the highest purpose, and even he was astonished at how much he could accomplish." This brave talk hid a father's anguish. Planck realized that his identification with values that Karl did not share had caused him to undervalue his son. "Without the war I would never have known his value, and now that I know it, I must lose him." [25]

Planck's commitment to the German state and its values suffered another shattering blow when he discovered that his government had systematically lied about the origin and conduct of the war. During the first month of hostility Planck had accepted the official account of the invasion of Belgium and joined with others in a manifesto declaring the unity of the German people with the German army and repudiating accusations that their troops had committed atrocities against Belgian civilians. This Manifesto of the 93 Intellectuals soon came to haunt Planck. From Lorentz in neutral Holland he learned about the brutality of the invasion, the war against civilians, and the destruction of monuments of European civilization. While these facts accumulated, Planck was trying to prevent hotheads at the Berlin Academy of Sciences, of which he was a permanent secretary, from ejecting all foreign members who belonged to enemy countries. He opposed them successfully and had the courage, which none of the other 92 intellectuals did, to repudiate the Manifesto. He did so in an open letter to Lorentz published in several newspapers. He also tried to ease the plight of Belgian colleagues [26].

When the war came to a close, however, he could not act on his political convictions. He wrote to Einstein that nothing would be better for Germany than the voluntary abdication of the Kaiser. "But the word 'voluntary' makes it impossible for me to come forward in the matter; for first I think of my sworn oath, and second I feel something that you will not understand at all, . . . namely, piety and an unbreakable attachment to the state to which I belong and which is embodied in the person of the monarch." [27] The emperor managed to abdicate without Planck's help.

Although he did not care for the Weimar Republic, Planck worked with it to rebuild the infrastructure of German science. He also unintentionally became an instrument in its efforts to normalize relations with the Entente. Because of his brave stand on the Manifesto, he could pass abroad where other German scientists were not welcome. Even in Belgium. He was the first German scientist invited to participate in the postwar Solvay conferences in Brussels. That precipitated a crisis of conscience. He hesitated to accept because he perceived, no doubt correctly, that a political test had been applied to prefer him to Sommerfeld, who knew more than Planck did about the subject of the conference. Sommerfeld had made the mistake of advocating the annexation of Belgium during the war. Lorentz helped Planck to see that either way, whether he attended or not, he would be making a political decision. Planck chose going as the lesser evil since it offered an opportunity to help thaw relations between German scientists and their counterparts in the Entente countries [28].

As Planck now was to experience over and over again, Mach was right, though in a sense he had not intended: human concerns can not be removed from science. Planck came to experience this indissoluble association more and more, as his eminence, his position in the Academy, and, after 1930, his presidency of the Kaiser-Wilhelm-Gesellschaft (KWG), the premier organization in Germany for research in science and technology, plunged him ever deeper into the politics of science. It is rightly said, politics is the art of compromise. Planck had to moderate his positions and even to sacrifice much that he stood for in order to have a chance of achieving his goals of rebuilding physics during the Weimar Republic and preserving it under the Nazis [29].

During the early years of Weimar, politicians and scientists liked to point to the strength of German science as an indication that the fatherland still had an honorable place in the wider world. The clean sweep by Germans of the Nobel prizes in physics and chemistry announced in 1919 became a symbol of this latent power. Planck was one of the winners of the year (he received the reserved prize for 1918 retrospectively). At the ceremony in Stockholm he offered a toast to "international, but especially Swedish and German, science." [30] Back home he continued his slide from the severe, international, even transpecific ideal that he had promulgated in his wrangle with Mach. In 1926 he told the Berlin Academy that "science, just like art and music, can prosper only on national soil." Another six years and Planck was talking almost like Mach. "The scientific and the purely human cannot be divided." This he wrote in 1932 when particularly troubled by the tendencies of the new positivistic world picture. It now appeared important to him to insist that the scientist bring his conscience and personal values into his work. Thus the pursuit of the eternal Absolute was compromised by the need to respond to short-term threats from home and abroad [31].

More sinister compromises were in store. Although well beyond retirement age, Planck decided to keep his positions at the Academy and the KWG after the Nazis came to power. He supposed that he would be able to moderate the effects of anti-Semitic laws on the organizations he led. At first he had a little success, at the cost of a few Hitler salutes and the degradation of having to plea with petty vicious people of a sort he would not have spoken to in earlier days. Ultimately, however, his policy of compromise failed. How skimpy the gain was in human terms appears from recent trawling in the archives of the KWG [32].

In order to avoid the takeover of the Society by the regime, Planck thought it necessary to impose the Nazi law for the "rebuilding" of the civil service promptly and literally. This *Selbstgleichschaltung* (voluntary alignment with Nazi policy) resulted in the forced retirement of the low-level staff affected and the invocation of every loophole and exemption in the law to retain upper-echelon Jews – institute directors, section leaders, and senators. Other options were available. For example, since only half of the MPG's income came from the state, threatened personnel might have been shielded by switching their salaries to private funds. But Planck saw the preservation of German science as an overwhelming good. He adopted the policy of bending with the wind (his phrase), complying when compliance appeared unavoidable and straightening up to support the most productive individuals and institutes when the storm subsided [33].

Even after age and the Nazis forced him from his various offices, Planck did not retire. He went around the country lecturing on science and religion and urging hard work against a better day. He carried this message into occupied territory during the war. Sober, distinguished, upright in character but bent with age, Planck offered and apparently gave comfort to many in despair. The message was equivocal, however. The Gestapo could not make out whether Planck's quietism encouraged passivity or passive resistance [34].

Planck believed it to be his duty to help alleviate the sufferings of others. In discharging it, however, he made colorable the charge that he collaborated with the regime he detested. Indeed, by staying in office after retirement (he remained active at the KWG as an "honorary senator"), and by giving lay sermons in occupied territory, he in effect served the regime. This was the most grievous and least successful of the compromises made by a man of exemplary probity and intellect in a long life directed as far as possible by the highest ideals and a painful sense of duty. If we apply to him the test he brought to bear on Mach, "By their fruits ye shall know them," what shall we say?

The man who planted the seedling of the quantum theory need not fear to be judged by its fruit. In science Planck's reputation is fair and secure. As for his program to salvage German science, its fruits are conspicuous in the name by which the Kaiser-Wilhelm-Gesellschaft has been known since World War II. That is, of course, the Max-Planck-Society, Germany's leading domestic and international research establishment.

Judgment by fruits, however, is ambiguous. If we judge by intent instead, we might conclude that Planck's quantum seedling was misbegotten. He had wanted to perfect the laws of thermodynamics and electrodynamics, and to confirm their unexceptional character, by applying them to a major problem at their interface, the equilibrium radiation of a black body. He failed.

Planck's intention in remaining in office under the Nazis, to preserve German science in its time of troubles, was fully, and more than fully, realized in the formation of the Max-Planck-Society. However, if the reckoning had been made during the war, even Planck would have judged his intention a miserable mistake. To take but one example, the Kaiser-Wilhelm-Institut für Physik, which he had prevailed upon the Rockefeller Foundation to build during the 1930s, had become the center of the uranium project directed by Heisenberg. Planck knew about the possibility of atomic bombs but not about the activity underway in the institute that he had created. Judgment by fruit would have agreed with judgment by intent in 1946 but not in 1942 [35].

Judgment by fruit is dangerous as well as ambiguous. It too easily slips to justification of the means by the end. This was Heisenberg's catchword, by which he justified his most extravagant hypotheses in physics and his most questionable political behavior [36]. Planck avoided this slip. Through all the compromises forced upon him during his long and active life, he retained an ability to think and act for the greater good-the national, the international, even the cosmic good-that kept him from confusing justification by fruits with the compromising doctrine that the end justifies the means. Still, on the tactical level there may be little difference. Will he nil he, the extravagant value Planck placed on German science and its preservation forced him into actions and inactions incompatible with his worldview.

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3

Atomic Waves in Private Practice

Bruce R. Wheaton

In the beginning God . . . said,
"Let there be a firmament in the midst of the waters."

Late in the fall of 1929, the stock market crash on Wall Street did little to dampen the spirits and accoutrements offered in the rue de Varenne salon of Pauline de Pange to celebrate the award of a Nobel Prize for physics to her beloved younger brother Louis de Broglie [1]. It came, after some false starts, for his audacious proposal six years earlier that ascribed a fundamental wave property to atoms of matter [2]. It was more than a complement to Albert Einstein's earlier (and at the time quite unacceptable) idea that a particle nature must be assigned to light [3]. De Broglie's matter waves completed a transition in physics from a venerable Platonic assumption that events beneath our ability to perceive follow the same rules as do those we can perceive: that the microscopic realm recapitulates the macroscopic. With matter waves we face inevitably the so-called "wave–particle duality" in which neither particle nor wave alone correctly describes events on the microscopic level. Rather, in a way totally at odds with our experiences, both wave and particle descriptions cohabit micro-reality, each only one of two facets more or less visible depending on the precise nature of our experimental inquiry [4]. The consequences of de Broglie's inspiration are immense.

There is little more obvious to all of us than that matter, the stuff we all deal with every day, however finely Anaxagoras might have divided and however widely distributed its seeds, consists of ponderous material particles. That eponymous advocate of human rule by individual opinion pioneered by Klysthenes, Democritus, is credited with

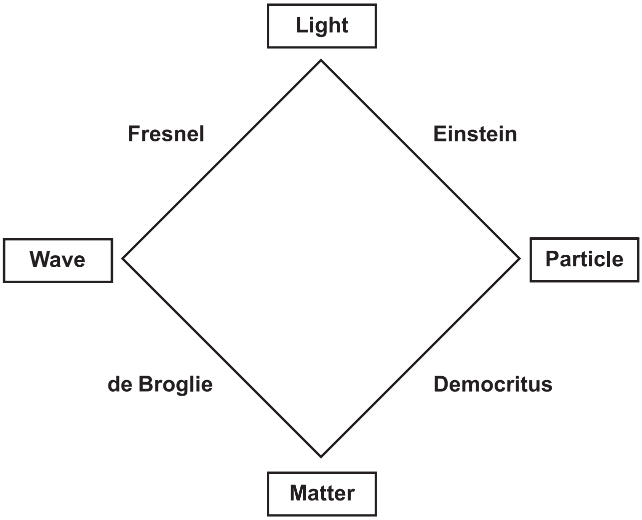

Fig. 3.1. Substance–form quadrilateral. ©TAPSHA

one notion of individual atoms in nature, whose "intestine motions" Lucretius exploited, and which we all accept intuitively as an atomic view of matter. In this article, I relate how this venerable viewpoint of our world crumbled through appeal to the most basic of our intrinsic Platonic ideals about the world.

The affirmative answer came in 1923 from the mind of Louis de Broglie, a virtual unknown in the field of physics. De Broglie's contribution is demarcated in time between Planck's fudging introduction of statistical quantization in 1900, related by Professor Heilbron in the preceding chapter, and Erwin Schrödinger's elaboration of de Broglie's viewpoint into full-fledged quantum wave mechanics by 1926, discussed by Michel Bitbol in the following chapter. Of de Broglie, Einstein is frequently quoted as saying "He has lifted a corner of the great veil." [5] He inferred, perhaps, a quadrilateral formed of wave and particle, matter and light, like Fig. 3.1. Einstein referred to Louis de Broglie's inspired completion of this quadrivium. But it was no coincidence that matter waves emerged from an environment dedicated to the practical application of contemporary research in physics.

At the turn of the last century Einstein's theory of relativity was immediately hailed as the culmination of a long-fought program to rid electrodynamics of velocity-dependent forces. But his lightquantum, which he saw as his "most revolutionary" concept, seemed a recursive reflection of the ancient duality concerning light and was quite reasonably not accepted. Even if light can be both particle and wave, matter is just – seemingly must just be – particulate. Thus the stage was set for what really was the most revolutionary of all changes in physical theory in the twentieth century. As I shall describe, it was the industrially inspired efforts of a dedicated group in Paris that broke the two-millennium mold confining matter to particulate atom [6]. How Louis de Broglie came unsteadily, even recklessly, to cast the foundation of quantum wave mechanics by rejecting Democritus in favor of Thales owes as much to industry and the practical need of application as it does to abstract inspiration.

Armed with the confidence born of noble stature in a society still partly yearning for the stable Leviathan of monarchy and profiting from the very backwardness of contemporary French physics, the reluctant prince Louis de Broglie neglected prevailing opinion circa 1920 that matter is atoms and only atoms and by doing so revolutionized our world [7]. Just what he did, how and why he did it depends to a great extent upon his elder brother Maurice, also a physicist, also an amateur, but one who had set himself up in private practice to aid French industry at the beginning of the last century. Perhaps his greatest success was to encourage his modest and introverted younger brother to propose atomic waves [8].

To the world of physics circa 1920, Louis de Broglie's heterodoxical proposal seemed to come unexpectedly like a lightning bolt out of the blue. Only a handful of physicists in France were concerned with these modern issues contested in Germany and England [9]. By casting the light deserved on this remarkable contribution to our understanding of the world, we shall see that the route to the proposal of matter waves is logical and understandable. At its center is the unjustly little-known influence on Louis de Broglie of his elder brother Maurice, member of the Académie des Sciences since 1924, of the Académie Française since 1934, and well before that founder of the first French laboratory devoted to study of the new radiations that revolutionized early twentieth-century physics.

3.1 Family Life

At the outset I need to stress the very special situation of the Broglie brothers and their family [10]. The family is of the highest social stature: aristocratic, reserved, literate, isolated, traditionally highly-placed in French politics, numbering many deputies, ambassadors, state counselors, eighteen officers of the Légion d'Honneur, even three field marshals of the nation (Victor-Maurice the first, 1724) amongst their generations [11].

Originating in Piedmont, the family took the decision to move to France following Cardinal Mazarin about 1654 and to *franciser* their name to "de Broglie" (pronounced "Bro-ye"), from the Italian "Broglia." A member of the family (another Victor) fought with LaFayette for the colonies in the American Revolution, although he later lost his head to the guillotine during the Terror. The third duc de Broglie, Louis and Maurice's great grandfather (yet another Victor), married Albertine de Staël, daughter of the famous salon novelist [12]. Two close relatives of Louis and Maurice have even been assassinated in the last century [13]! Grandfather Albert – 4th Duc de Broglie, prodigious political historian, like his father before him and like his son and his son's sons member of the Académie française, deputy like his father, government minister like his father and like his father former French ambassador to England – Albert was the patriarch of the family when Maurice was growing up in Paris and at the family château in Normandy [14].

Indeed, the family was steeped in and virtually epitomized French political history. Great grandfather Victor François, deemed the "ablest monarchist politician" of his time, organized the French cabinet under Marshal McMahon following the political dislocation of the Commune [15]. He was so widely recognized an historical icon that Huysmans' des Esseintes of 1884, searching in the past for relief from the superficial excesses of his time, invoked great grandfather de Broglie twice as such [16], and the family name fairly represented France even in Dickens's novels [17]. The Broglie family library of some fifty thousand volumes still at the château is generally considered one of the best French historical collections in private hands [18].

The 242-meter-wide château sits in what was domaine Ferrières, now the Bois de Broglie overlooking the village of Chambrais to the west, founded by William the Conqueror and since 1742, following the elevation by Louis XV of François-Marie de Broglie (one of the marshals) to the status of Duc, called simply "Broglee," a hundred forty kilometers west of Paris in the valley of the Charentonne river. By one of those remarkable coincidences in history of science, Augustin Fresnel, originator of the modern wave theory of light, was born in Broglie in 1788 while his mason father Jacques added a new wing to the château [19]. The main east-west road through the village is named rue Fresnel, although the western portion was renamed by the mayor Maurice de Broglie "route des Canadiens" after their role of liberation in 1944. With the venerable château, town houses in Paris, hôtels in the country – at Dieppe on the coast, at Saint-Amadour in Anjou, and elsewhere – English governesses and nurses in residence, a temperamental but talented chef, exclusive access to the then-private Wallace park "Bagatelle" above the Bois de Boulogne in Paris [20], the surviving children of Victor and Pauline de Broglie seemed to have every advantage that society could provide [21].

Note, however, that in the aristocratic tradition, Albert's eldest son, even after he married in 1871, stayed for many years in his father's home and certainly under his influence: the four children were raised by their maternal grandmother (née Ségur) and under the strong sway of their usually absent grandfather as much as they were raised by their own parents. Father Victor, like his father before him first prize *concours général* and deputy, was very conservative indeed, more so than most of his recent family. His daughter Pauline portrays him as frustrated by the longevity of his own father [22]. Reading accounts of family life, one is struck today by how remarkably similar it sounds to lives of the aristocracy at the end of the eighteenth century, or as depicted by Tolstoy. Very closely guarded social interaction with only a select circle of families, resident tutors for the children, 7 to 8-course dinners in "smoking et decollétage" at precisely 8 every evening where the children were forbidden to speak at the table [23]. Indeed, until age 7 and proven well behaved, the children were not even seated at the table. When the family repaired to their property in Dieppe for the summers, where "little Louis" was born in 1892, it was like an "expedition to the Pole," with enough provision for the four-hour train trip "to travel to China!" sister Pauline recalled [24]. Grandmother Célestine Armaillé [25], direct descendent of Louis XV and in whose Paris townhouse the family dwelt, rather summed up the demeanor of family life with a moral she presented in turn to each grandchild: when faced in life by a choice, you never go wrong to pick the alternative least agreeable to you! And she was far and away the most liberal of all in the family. As we shall see, Maurice engineered the removal of himself and later of his siblings from this environment, a life that became harder on his bachelor brother during the two-decade widowhood of his mother.

3.2 The Modern World

The parents and the grandfather patriarch were dismayed when eldest son and ducal heir Maurice early on expressed more interest in science than in politics. Maurice was fascinated by the new understanding of the micro-world: the potato-starch grains that the Lumière brothers used in color photography [26], an objective form of the *pointillisme* of Seurat, then horrifying the establishment in France [27]; the intricate dance of exploding kinetic gases in the Otto engine, soon scattering the chickens along the weekend roadsides [28]; the incomprehensible rapidity by which the magnetic field changes through alternating current fed to one of the new Gramme or Tesla a.c. motors [29]; the incredible economic effects of the railroads in France for "industrializing time and space" [30]; the much rumored possibility of wireless communication by tickling the aether, that is by modulating the flow of electricity in step with a telegraph key, precisely the subject of Maurice's first publication [31]. His grandfather called such interests mere "bricolage sans avenir," and prohibited him from pursuing his investigations at the château and his parents tried the same at the capacious Parisian townhouse.

A military career might set him straight; in 1893 he graduated from the École Navale and served in the Mediterranean on the Iphigénie. Imagine the effect on the 17-year old to spend much of the decade in Provence; it was like finding an entirely new world from the reserved "langue d'or" of the north. Investigating on leave the Rhône river delta, Maurice encountered the Solvay salt and soda works at Salin de Giraud in the Camargue, where the Brussels entrepreneur had encamped workers in state-of-the-art residences. They extracted the raw materials for the newly-established chemical industries of Europe. It was at practical facilities like this that the principles of science could be applied to make a real difference in the world, Maurice concluded.

Fascinated by the modern, he propelled a motor-powered tricycle across the Tunisian desert, terrifying the locals. He also carried on investigations of the then-new wireless telegraph, preferring Rutherford's magnetic detector to those electro-resistive Branly "coherers" (tubes of iron filings) then in use, leading to its adoption by the French Navy. He also managed to carry on physics studies in Toulon and Marseilles with Léopold Brizard.

When grandfather Albert died in 1901 the resolve of Maurice's parents was strongly tested: A deal was cut. If the 25-year-old bachelor Maurice would agree to meet eligible ladies as befit a future Duke, he could continue his experimental work, aided by the clever assistant and man-servant, Alexis Caro. Actually Maurice did pretty well! He continued his work in physics at the family townhouse, installing in his rooms in Louis XV cabinets and armoires the "enormous coils of brass attached to an interrupter that hummed like a hornet," "electrical batteries like pots of jam, mysterious spinning disks, great spheres of copper spraying tremendous sparks that lit up the whole room," as his sister and chronicler recalled [33]. But on the other hand of the deal he also managed to woo one of the richest eligible young ladies of the time, Camille du Rochetalliée, whom he married in 1904. Within a year they bore a daughter. More important to us, Alexis helped move the growing collection of physics equipment out of the family home into Maurice's own.

The new mother-in-law had recently bought and refurbished two elegant hôtels cornering the rue Balzac at 27–29 rue Châteaubriand, just off the Champs Elysées in the afternoon shadow of the Arc de Triomphe and comfortably situated in the fashionable 8th arrondissement. Maurice installed his laboratory in the basement, away from the discouraging influence of his parents [34]. The pieces of laboratory equipment that years ago he had begun to assemble at the family château – vacuum pumps, electrometers, x-ray generators, tubes to measure ion mobility, electrostatic generators and induction coils, cathode beam tubes – now could serve science in a concerted way. The point is this: with a self-confidence fostered by his regal upbringing and with coffers massively reinforced by the fabulous Rochetaillée fortune, Maurice was unstoppable. In the first decade of the century, he and Caro (whom he described as "becoming more and more a physicist") built up a laboratory of world-class proportion for the study of ionization phenomena and x-rays. Indeed, when he explained to his mother-in-law that he studied Brownian motion of ionized smoke particles, she exclaimed him a genius who could even "make discoveries just by smoking a cigarette."

3.3 Sibling Partnership

Maurice was 31 and Louis 14 in 1906 when, five years after grandfather Albert's death, diabetic father Victor also died. And life changed dramatically for the two younger de Broglies who remained with the family. Maurice became the 6th Duke de Broglie, and he took immediate responsibility for the education of his brother Louis, who for the first time in his life soon entered a private Lycée. Louis was delighted to meet there for the first time "sons of bankers and industrialists." [35] Newly emancipated himself from the stodgy tradition-bound life of the past, Maurice soon found means to draw his brother and sister from the same. They became his family: when Maurice took his Sorbonne doctorate in 1908, they with Camille, not his mother or grandmother, constituted the audience. He had, by 1911, an even stronger personal motive. In June his only child, 7-year-old daughter Laure, died before his very eyes just as had his 8-year younger brother Phillippe died when Maurice was 15 years old. Indeed sister Pauline later made the sad fact clear that their parents' decision to conceive the fifth (and Nobel prize-winning) child Louis was very closely linked to the loss of Phillippe to appendicitis the year before. In a very real sense, Louis, to whom Maurice had always been a father figure, became Maurice's only offspring. This father-son relationship would have far-reaching significance to both de Broglie brothers and to physics.

Maurice, sixth Duc de Broglie, never completed the filiation that strong family pressures from both sides must have dictated. We have no documentation of this issue (or non-issue) whatsoever. It may have been that the health of Camille prevented it, and she had to remain content walking her caniche around l'Etoile; it may have been that Maurice so emphatically rejected the aristocratic mode into which he had reluctantly been thrust; it may also have been that Maurice saw young Louis as the proper avenue of family advancement. Apparently Louis felt the pressure too, for he never married [36]. He stayed in his mother's home in the square Messine near the Parc Monceau until her death when he was 36, and only then bought his home in Neuilly sur Seine [37]. I recall with pleasure his graciousness in welcoming my visit there in 1979 where we discussed these researches, and his gift to me of his *Nouvelles perspectives en microphysique*, from which he carefully removed page 237 [38]. But his only companion was his unobtrusive man-servant. Indeed, on Maurice's death in 1960 while Louis became the seventh Duc de Broglie, the château passed to the brothers' nephew, Jean, in a different branch of the Broglie family [39]. Perhaps relevant documentation exists at Broglie, but since Jean's assassination 30 years ago the family has shut all doors to discovery. We shall soon divine the fruitful side of Maurice's fraternité.

In 1911, as the manifestly intelligent Louis pursued a predicted path toward political history, a great turning point came. Maurice's mentor Henri Poincaré invited Maurice to collaborate with him as secretary to the first Solvay Congress of physics to be held in Brussels [40]. (See Fig. 3.2.) Recall that when Maurice had been in the French navy, stationed in and around the Camargue, he had become familiar with the extensive salt works that the Solvay interests ran there, financial resources now applied to better understanding the new physics of the micro-world. The results were explosive! All the leading physicists of Europe were there; Maurice introduced Louis to their most current works [41]. And Louis's path in life changed abruptly toward the mysteries of the new physics, and away from the political/historical paths of his tradition-bound family [42].

Fig. 3.2. First Solvay Congress, Brussels 1911. Maurice de Broglie is standing in the back row, sixth from the left. Photo reprinted by permission of the International Institutes for Physics and Chemistry (founded by E. Solvay).

Because Maurice's approach to contemporary experimental physics was unique in France, many young aspirants began to frequent the private laboratory in Paris [43]. Maurice occasionally invited the group out in the summer to the château for games and play-acting to entertain themselves, but particularly to enliven Louis and his sister Pauline. Maurice's younger sister, who spent much time at the Paris laboratory, called the enthusiastic group her "flirts." Among the first, Armand de Gramont founded the Institute d'Optique in 1919. Maurice also saw to it that his own teacher from Marseilles, now removed to Paris, worked with Louis to prepare him for higher studies in physics.

3.4 Benefits of War

When hostilities broke out in 1914, Maurice returned to the Navy where, first in the Camargue and later at the international wireless station at Bordeaux, he considerably improved the telegraphic service. He was one of the first in France to apply the new and revolutionary recognition that the triode could be put into self-sustained electrical oscillation, generating radio frequency signals as well as detecting them; this put those immense and wasteful predecessors by Poulsen and Alexanderson in the dust heap [44]. Mobilized under minister des Inventions Paul Painlevé in 1915, he and Caro also worked with Perrin and Fabry on artillery sound-ranging [45] and later at Toulon developed a successful long-wave means for wireless communication with submarines. This is no simple problem, since seawater conducts electricity and consequently shields submarines from electromagnetic waves. Later in the war Maurice became the French naval attaché to the British Admiralty, in close contact with Frederick Lindemann [46].

Louis was mobilized in 1913 and, in part motivated by the death of his nephew early in World War I, joined the 8th corps of engineers and fulfilled his duty as a *sans-filiste* (or wireless) telegraph operator under family acquaintance General Ferrié at (actually underground [47] at) the Eiffel Tower [48]. He lived with his widowed mother and grandmother at the square Méssine [49], and carried on clandestine wireless conversations with his brother, 700 kilometers distant. His sister even claims that Louis was the first Frenchman to hear of the armistice, when he received the wireless message concerning the eleventh hour on 11 November of 1918 [50]! The evident power of the new electronics in wartime had a decisive effect on Louis. Even though he had failed physics on war's eve, at its end "he decided to follow physics and only physics." [51]

After the war, as Louis resumed his physics studies at the Collège de France and honed archival skills at the Ecole des Chartes, Maurice concentrated on the x-ray studies he had begun in 1913 and made first-rate contributions to the use of x-ray absorption in the study of atomic structure, work equivalent to that done in the leading government supported centers of Munich, Berlin, and Lund [52]. His efforts to build up what would eventually become the "Laboratoire française des rayons x" around the corner at 12 rue Lord Byron drew even more interested students. (See Fig. 3.3.)

Among the earliest were Jean-Jacques Trillat, at Byron from 1924–33, who later pioneered an electron microscope at Besançon. He later recalled that at Byron "a large part of my scientific activity was focused on the combination of diffraction of x rays and that of electrons." [53] Others there were Jean Thibaud who worked on x-ray spectroscopy; René Lucas; Pierre Dupré-Latour; Claude Magnan; the talented mass-spectroscopist Louis Cartan, later assassinated by the occupying Germans; Alexandre Dauvillier, whose arrival in 1920 figures heavily in our story and who later worked extensively in astrophysics; Louis Leprince-Ringuet, an engineer who met Maurice while improving French subma-

Fig. 3.3. Site of the laboratory at 12, rue Lord Byron. The façade was modernized after World War II. ©TAPSHA

rine cables and was convinced by him in 1929 to take up the cosmic ray studies that brought him fame [54]; Bruno Rossi to confirm his preliminary work on coincidence counters for study of nuclear decay; Dmitriy Skobel'tsyn; Pierre Auger and dozens of others, several from abroad [55]. Marie Curie came to visit and admire the advanced equipment as well as value Maurice's connections with French industry.

Marcel Proust, who modeled the duc and duchesse de Guermantes in his *À la recherche du temps perdu* on close relatives of the de Broglies' grandmother, claimed knowledge of the importance of Maurice's work done at rue Châteaubriand. He even characterized the perplexity that Maurice's rejection of his aristocratic upbringing caused the older generation, citing

> l'affliction d'un père voyant un de ses infants, pour l'éducation duquel il a fait les plus grandes sacrifices, ruiner voluntairement la magnifique situation qu'il lui a faite et déshonorer, par des frasques que les principes ou les préjugés de la famillie ne pouvent admettre, un nom réspecté. [56]

Young Louis could hardly have had an environment better calculated to confirm and solidify his study of the new physics!

3.5 Industry

While Maurice could well afford to support the private laboratory, his real purpose was to foster use of the new techniques of electronic physics for the benefit of industry, as had already begun to occur in the U.S. and Germany [57]. So Maurice used his social connections to arrange for many of the laboratory aspirants to obtain internships from French industry: Electricité de France, les aciéries de Saint-Chamond, les manufactures Saint-Gobain, Institut de Caoutchouc, Les charbonnages de France, Société Pathé, Kodak, Institut de Pétrole, Pechiney, Michelin, Lip, Rhône-Poulenc, Société française de photographie, Renault, Franche-Comté, and other organizations provided modest compensation so that interns could undertake investigations in the lab half-time, and then apply the new techniques to problems faced in operations of that industry [58].

Trillat was by all accounts the most active laboratory aspirant in this regard, consulting for virtually all of the companies listed above. He had worked on artillery-ranging and meteorology during the war, and his father taught chemistry at Louis's old school. At Byron he used x-ray diffraction to study lubrication on the microscopic level. It was also he who hid in a mountain prison cell France's supply of heavy water in 1940 upon the German occupation. I was fortunate in 1983 to be invited by him and his American wife to their home in Versailles for several hours' useful discussion of his consulting work. Louis is quoted to the effect that it "evolved the border where one meshes in a fruitful collaboration fundamental with applied science;" [59] certainly a worthy corroboration of Maurice's over-riding goals.

Indeed Maurice's private enterprise represents the earliest application in France of what in Germany and in the United States was becoming an essential stimulus to physics: the industrial electronics research laboratory [60]. This is an extremely important development, not yet addressed adequately in the historical literature, primarily for lack of available documentation [61]. Primary sources for historical study of Maurice's laboratory and its industrial connections are exceedingly limited. It may be that essential correspondence and records exist at the family château or lie unknown in the industrial archives of many companies. Unfortunately almost none has come to light: I should be delighted for any information or assistance that might provide authoritative documentation of this pioneering interaction between industry and modern physics in France. It begs for detailed investigation into contemporary documents not yet to hand.

3.6 Physique Redux

This issue addresses an important historiographical point: the perceived backwardness of French science and technology at the turn of the 20th century [62]. In the Grandes Ecoles, cutting-edge research of the sort Maurice's school carried on was not given sufficient credit and recognition by an academic staff frequently more interested in adding yet another *cumul* position and income than in studying nature, or teaching others to do so [63]. Louis Leprince-Ringuet, well educated at the Ecole Polytechnique as an engineer, reported that Einstein's theory of relativity was never mentioned [64]. As well, French forays into modern topics at *fin-de-siècle*, like reputed N-rays and black light had cast something of a pall over the efforts of French academic scientists [65].

For the most part, those industrial organizations in France that tried to establish scientific quality control and production efficiencies did so with little help from academic scientists [66]. Terry Shinn has discussed the efforts of many: color production at Société des produits chimiques de St. Denis; Air liquide; foundry Société d'Ougrée Marihaye; ammonia by-products at Société chimique de la grande Paroisse; wireless by Société française radio-electrique and Compagnie sans-fil; Société d'Electro-chimie; Pont-à-mousson; Jeumont. Most of these companies paid license fees for use of foreign patents (like the Solvay soda process) rather than develop their own [67]. "Pedagogical programs tailored to ready students for careers as industrial researchers were shockingly absent," [68] not that directors of industry would have welcomed them in any event [69]. The point here is that Maurice broke this mold, being neither dependent on the academic establishment, the cumul, or tradition, gaining the respect of industrialists because of his relatively conservative politics, certainly in comparison to most republican French physicists, some of whom were openly Communist [70]. Maurice was under none of the traditional constraints [71].

Physics was hardly a widely-developed field of intellectual endeavor in France in this period [72]. Our studies of almost 4,000 physicists active in the first half of the century reveal that, before Maurice's influence, there was little growth in numbers of French physicists compared to that in other countries, as shown by Fig. 3.4 [73]. But the spate of new discoveries by the turn of the century – from photography to the wireless, from radioactivity to x-rays, from bicycles to motorcars, from flying machines to electronics – all contributed to a new appreciation of science and its technical possibilities [74]. Maurice de Broglie was one of the true prophets and pioneers. If we had birth-cohort data for the next few decades in France we would see a rapid rise in numbers, and much of it due to Maurice's laboratory and activities [75].

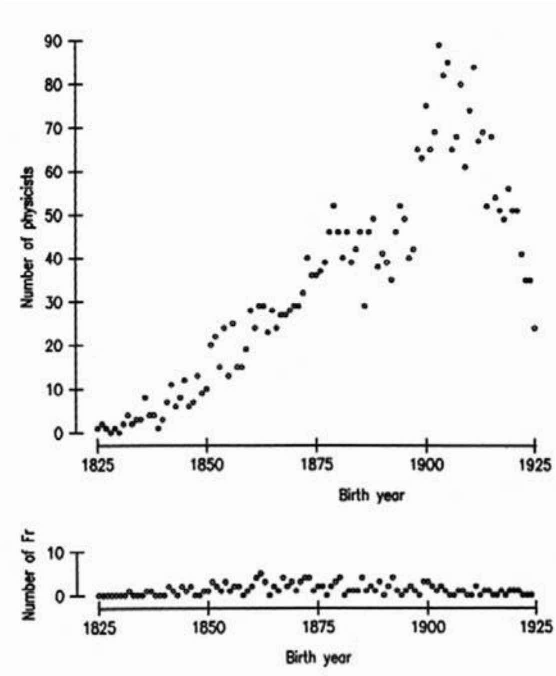

Fig. 3.4. Birth cohorts of physicists of all nations (top) and of French physicists (bottom). From Wheaton, *Inventory of sources* (Ref 73), pp 146-8. ©TAPSHA

3.7 X-Ray Photoeffect

Just at the moment that Louis de Broglie was preparing himself for his own doctorate in physics, Alexandre Dauvillier came to rue Châteaubriand [76]. He had investigated the well-known photoelectric effect for his thesis but did so using energetic x-rays rather than light [77]. Dauvillier wanted to see if the velocity with which electrons are expelled from matter by x-rays could be used for study of the properties of atoms, but he lacked an efficient means to measure those velocities [78]. Maurice knew of the beta-particle velocity spectrometer that Rutherford and his students had developed at Manchester [79] and built his own, shown in Fig. 3.5. Early in 1921, he and Dauvillier undertook detailed photographic studies of what they called "spectres corpusculaires," or the variety of different electron velocities produced in matter by monochromatic x-rays and recorded as delineated bands on a photoplate. The results were discussed and analyzed not just by them, but also with Louis. Their goal was a detailed understanding of the energy levels of the irradiated atoms. The net result by 1923 was that Louis revolutionized all of physics.

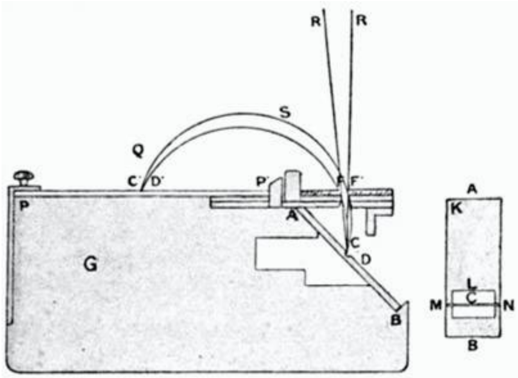

Fig. 3.5. M. de Broglie and Dauvillier's β-spectrometer. Thin strip MN of sample at C, magnetic field into page, bands on photoplate PP'. From M. de Broglie, *X-Rays* (Methuen, London 1925). The French edition was published in 1922 (Ref 90).

Maurice saw it first across the laboratory bench [80]. It was, as I have shown elsewhere, not a new problem, but it was a real puzzler, and many physicists in the preceding two decades had pondered it and fled. The difficulty was this: x-rays are the aetherial consequence produced by stopping speeding electrons. Those electrons have velocities that are easily measured. The x-rays they produce can, by ionizing matter, stimulate the release of other electrons whose velocities are just as easily measured. When a sample is irradiated with x-rays, the released electrons can be bent into circular paths by an imposed magnetic field, as in Fig. 3.5. Their banded image on the photoplate gives their velocity with remarkable accuracy.

The problem is that the velocity of the electrons that produce the x-rays is no greater than the velocity of the electrons released by those same x-rays! Why is this a problem? Because if the x-rays are waves or singular impulses spreading spherically from their point of production, they have no business at all granting any electron they hit as much energy as they started out with. Their energy is presumably being dissipated over larger and larger regions of 3-dimensional space before they ionize the atom. As another student of this paradox put it: It is as if a log falls in the ocean, and the wave that results concentrates its effort on another log a thousand kilometers away sufficient to propel the second log up into the air! Maurice found it all “remarkable.” He concluded that the x-rays could not be normal waves at all, that they must be like particles, or at least like “concentrated points on the surface of a wave.” [81]

This brings us to the younger, somewhat hesitant, neophyte brother de Broglie [82], who had begun to publish jointly-authored papers in the *Comptes rendus* under his older brother’s aegis in 1921, including one directly relevant to our story [83]. Louis’s approach has been termed “strange,” [84] “unorthodox,” [85] “audacious,” [86] and worse. I describe it as rather like the solution for a 2-ton elephant crossing a 1-ton bridge: “in the first approximation we shall neglect the mass of the elephant.” The talk amongst the other novices at Maurice’s private laboratory decidedly neglected the elephant. There was little talk of radiation as just waves or of matter as just atoms. Iconoclast Einstein’s lightquantum was almost eagerly adopted there despite foreign criticism. And Dauvillier did not share Louis’s *pudeur*; he wished to make his mark. The year before he had taken on the titans, challenging Bohr’s orbital justification of element 72 [87] and disputing Sommerfeld’s interpretation of relativistic x-ray doublets [88]. Young Louis, ignorant of the political consequences but emboldened by the controversy, completed the square by audaciously considering Maurice’s possible waves of matter.

The third Solvay Congress in April of 1921 devoted most of its discussions to Maurice’s results and comparable results for gamma-rays found by Charles Ellis [89]. In his book on x-rays the next year Maurice published a table that compared x-rays and gamma-rays to electrons traveling at specific velocities, and described extremely fast electrons as having a wavelength. “One finds certain kinetic properties in undulatory radiation and certain periodic properties in the directedness of electrons,” Maurice concluded [90]. At the same time Louis was trying to understand it all on the basis of theory. “At every stage of my life and career,” Louis told his elder brother, “I found you near as guide and support.” [91] “We debated the most baffling questions of the time, in particular the interpretation of your experiments on the x-ray photoeffect.” “The insistence with which you directed my attention to the importance and the undeniable accuracy of the dual particulate and wave properties of radiation little by little redirected my thought." [92] If waves can act like particles, perhaps particles can act like waves. See Fig. 3.6.

Fig. 3.6. Louis de Broglie, January 1, 1924. Hulton Archive/Getty Images

3.8 Relativistic Revelation

Louis knew that a synthetic interpretation that combined particle and wave was absolutely unavoidable, and sought guidance from the only theoretical source where "atoms of light" had been proposed: by Albert Einstein in 1905. Einstein's lightquantum had not been well received by physicists [93]. It was trotted out as evidence that even the greatest minds will occasionally err [94]. But it was Einstein's theory of relativity, not the lightquantum, that had the greatest effect on Louis's conundrum, it "prepared the ground." [95] For Louis spied another paradox when he compared the relativistic and the quantum interpretations to atoms of light.

The most easily understood consequence of Einstein's theory of special relativity predicts that time intervals for a moving object appear to be longer when seen from a stationary location. Thus the famous

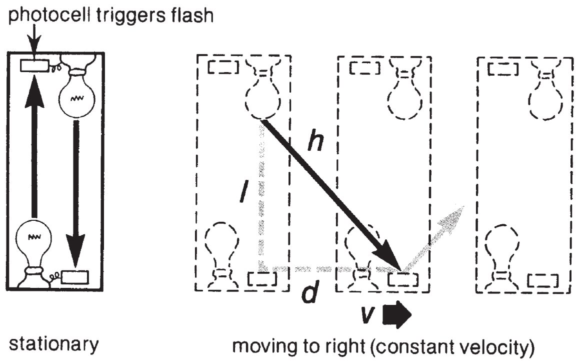

Fig. 3.7. Pythagorean light-clock. From Wheaton, *The rise and fall of the aether: Einstein in context* (Exploratorium, San Francisco 1981). Photo reprinted by permission. ©Exploratorium, www.exploratorium.edu

prediction that an astronaut on a rocket ship will age less rapidly than his twin left on earth. It's not difficult given the postulates of relativity and the Pythagorean relation to understand why. Imagine the cycling light clock in Fig. 3.7. When it moves to the right with velocity v, it takes longer for the flash to reach the photocell along diagonal h by a factor of

$$\sqrt{1 - v^2/c^2}$$

If each of a series of repeating time intervals seem longer to the fixed observer, that makes their measured frequency lower in the same ratio. So, according to relativity, if an object with an inherent frequency passes you by, you measure that frequency to be lower than that measured on the object itself. And this is how de Broglie envisioned his point mobile, just like a tiny relativistic quantum clock, another strangeness in the equation. Quantum theory, on the other hand, predicts precisely the opposite! If the object is moving relative to the observer it has more energy and should appear to have, therefore, a higher inherent frequency. Louis de Broglie's greatest contribution to physics was to realize that a resolution of this relativity/quantum paradox might clarify the dilemma of particle and wave representations not just of light [96], but even of matter itself [97].

3.9 Matter Waves

Every particle of matter or of light, he proposed, is accompanied by a wave, the famous "phase wave" that controls the motion of the particle [98]. He called it an "onde fictive" because he found it had to travel faster than light and therefore could carry no energy. Although our two observers (one stationary, the other on the particle) disagree about the inherent quantum frequency of the phase wave and the relativistic frequency of the particle, they nonetheless can agree that there is a moving point in space where the two frequencies always remain in phase, where they always remain locked in step. It seems counter-intuitive, but Louis showed that it is true [99]. This moving point essentially defines the location of the particle at each instant. Louis soon thereafter credited Léon Brillouin as "le véritable précurseur de la méchnique ondulatoire." [100] On the assumption that the phase wave is planar (more of the strangeness) and precedes an electron *around* in an elliptical atomic orbit, de Broglie derived precisely the action-integral representation of the stable atomic electron orbits that Niels Bohr had proposed in 1913 [101]. This orbital treatment of the electron within the atom was precisely what de Broglie fastened on in his *Thèse* for the doctorate in 1924 [102]. As Darrigol puts it, his "reluctance to go beyond known phenomena connects with an awareness that the main assumptions of the new theory were still in an incomplete and provisional form," [103] a judgment that Louis himself accepted at the time [104]. "Sifting, rending, and combining his intuitions into a coherent whole, Louis de Broglie closed on the goal he had set himself at age 20: to clarify the enigma of quanta."

The reader might find it interesting that de Broglie's famous equation $\lambda = h/p$ hardly figures in the thesis – it only appears fleetingly in dismissal of the degenerate and unphysical case of an electron constrained to dipolar oscillation along a line, which has nothing to do with Bohr atoms. But Louis had found what had eluded Bohr, a reasonable explanation for why there should be discrete stable electron orbits in an atom at all [106]. They correspond to the resonant standing-waves of a collar-like band that when excited is able to vibrate only at certain well-defined resonance frequencies, where an integral number of phase wavelengths exactly fills the orbit, and never at those frequencies in between [107]. "This beautiful result," he said, "is the best justification we can give for our way of addressing the problem of quanta." And Einstein said, in perhaps a more accurate translation, "he has found a fingerpost in the great mystery." [109]

Yet there was more. Louis had indeed turned Einstein's lightquantum hypothesis – that light acts like a particle – completely on its head: as Maurice had suggested, matter has an inescapable wave-like property. "A group of electrons that passes through a small aperture should show diffraction effects," Louis predicted [110]. But he was not successful in convincing the busy and practically-minded experimentalists in his brother's Paris laboratory to try the tests [111]. Dauvillier was trying to build an electronic television; and Rutherford's mistake in thinking he had evidence of diffracted gamma-rays indicated that the tests would be difficult if not impossible [112]. But shortly after Erwin Schrödinger's mathematical restatement of Louis's inspiration experimental evidence for electron diffraction was found. Davisson and Germer in New York and George Thomson in England provided the evidence needed to ensure the Nobel award to Louis in November 1929 [113].

3.10 Sinuous Legacy

Like viewing a binary star, popular history has illuminated the one brother and necessarily eclipsed the other in its retrospective view. The "de Broglie" of the phase wave is young Louis and, frankly, even most physicists know little or nothing of Maurice (Fig. 3.8). He represented both the Académie des sciences and the Académie Française [114]; he fostered the French Navy's embrace of modern technology and its role vis-à-vis the world-reigning British Admiralty; he received a second doctorate *honoris causa* from Oxford University in 1921; and he represented France's naval contribution to the American Revolution at the 1932 sesquicentennial of the 1783 victory over the British lion [115]. He represented the scientific side of the new microscopic expressionism of his time. Echoing Proust's and Huysmans' forays into deeply individual subjectivism, Maurice rejected the crass *noblesse* of his aristocratic caste in his case in favor of the aetherial delicacy of the wireless [116]. Like Fauré and Ravel, he discarded the traditional self-serving harmonies of his class in favor of a purer egalitarian tempered scale of microscopic quantum transitions [117]. Like Signac and Seurat, he saw through superficial reality to its underlying grit and with the microscopic power thus obtained leveraged his new understanding to undreamed of practical ends, and this lesson was not lost on his brother [118].

Through connection to French industry Maurice mobilized a cadre of believers in the new world of electronics and its power to control technological processes. He and his students helped engineer a renaissance in French physics, both applied and pure, that raised French contributions by the 1940s to the fecundity of a century before. At a time when the significance of the new microphysics was still unclear

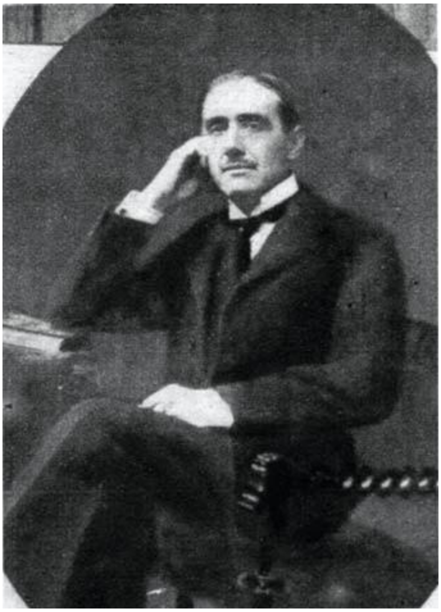

Fig. 3.8. Maurice de Broglie (1875–1960). "Voltiana," Como, Italy – September 10, 1927 issue, courtesy AIP Emilio Segrè Visual Archives

to most in France, Maurice perceived its implications and mobilized resources to guide its development. His actions were very much in line with the motto "*pour l'avenir*" of the family from which he distanced himself. Likely his greatest guidance was that which led his brother to his inspired and unorthodox conjecture of the wave nature of matter on the microscopic level.

I need not here dwell on the consequences of Louis de Broglie's phase wave, particularly in the quantum mechanical guise Schrödinger gave it, for it is extensively discussed in the literature [119]. Electron diffraction produced the electron microscope; electron waves led to band theory in metals, and to an understanding of superconductivity, superfluidity, Josephson electron tunneling through insulators, and gave the theoretical basis of the transistor [120]. What Louis de Broglie had had served up on a plate to him in Paris was quite remarkable, and he spent the rest of his teaching and writing career coming to grips with it [121].

Matter waves completed a transition in physics from a venerable Platonic assumption that events beneath our ability to perceive follow the same rules as do those we can perceive: that the microscopic realm recapitulates the macroscopic. Atomic waves epitomize and symbolize the wave-particle duality in which the subjective nature of our experimental inquiry affects which facet of matter we perceive on the microscopic level [122]. Louis had difficulty accepting the indeterminism that the later prevailing “Copenhagen” interpretation placed on his idea [123].

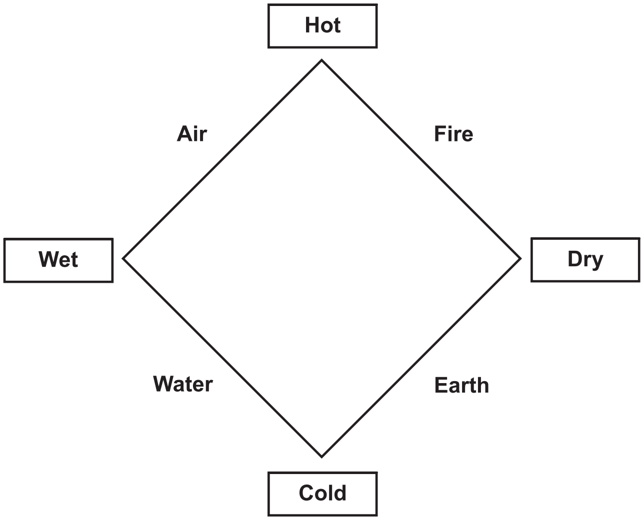

Fig. 3.9. Aristotelian quadrilateral. ©TAPSHA

Let us return in conclusion to the quadrilateral of opposing qualities that de Broglie’s inspiration completed, Fig. 3.1. Readers who have studied Aristotle’s views on natural philosophy should find it familiar. If you substitute at the vertices Aristotle’s canceling opposites of hot/cold, wet/dry, as in Fig. 3.9, the sides define Empedokles’ four elements (land, sea, wind, and fire) whose various mixtures, on the Aristotelian view, constitute and embody all generation and corruption in the cosmos. Many have questioned the soundness of de Broglie’s physics, but the inspired neglect of the elephant intrinsic to his unorthodoxy produced Götterfunken bright enough to “stick the matter right under Schrödinger’s nose.” [124] I have described elsewhere how this independent route through phase waves led to an alternate version of the new quantum mechanics by 1926 [125]. That there were, remarkably, two versions of the new mathematical ontology, matrix mechanics and wave mechanics, is due to the virtual separation of radiation theory from matter theory in the wake of the success of the Bohr atom. That separation ended in the de Broglie school of 1921–24.

The lesson here seems clear. Sometimes science advances best under judicious neglect of orthodox belief. Like Goudsmit and Uhlenbeck who published electron spin over the objections of Lorentz and Ehrenfest that it violated relativity theory, it was the very strangeness of Louis de Broglie's non-physical "onde fictive" that led to fundamental advancement in our understanding of the micro-world. Elephant or no, it yet again illustrates the venerable wisdom of Francis Bacon that truth emerges more readily from error than from confusion.

Citius emergit veritas ex errore quam ex confusione.
Novum organum, II, a. 20; ca. 1620

Acknowledgements

I am indebted to the library of the University of Wisconsin–Madison for making its collections and inter-library borrowing services freely available to me for research when that of the University of California–Berkeley did not.

In memoriam, Louis Michel, helpmate and friend.

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1. Louis Leprince-Ringuet, "Louis, Maurice et le laboratoire," La vie des sciences **9**, 325–329 (1992) on p 327; Jean de Pange, *Journal*, 3 vols. (Grasset, Paris 1964–79), vol 2
2. Bruce R. Wheaton, *The tiger and the shark: Empirical roots of wave particle dualism* (Cambridge University Press, Cambridge 1983), pp 305-306
3. Albert Einstein, "Über einen Erzeugung und Verwandlung des Lichtes betreffenden hueristischen Gezichtspunkt," Annalen der Physik **17** (1905), 132–148
4. A general overview is in Wheaton, *Tiger* (Ref 2).
5. Olivier Darrigol, "Strangeness and soundness in Louis de Broglie's early works," Physis **30** (1993), 303–372, on p 355; Wheaton, "The laboratory of Maurice de Broglie and the empirical foundations of matter waves," in Paul Germain, ed, *La découverte des ondes de matière* (Tech & Doc, Paris 1994), pp 25–40, on p 33; Mary Jo Nye, "Aristocratic culture and the pursuit of science: The de Broglies in modern France," Isis **88** (1997), 397–421, on p 413. I am indebted to Geoffrey Wheaton for images 3.1 and 3.9 and much else.

6. An influential German generation from Schelling through Wien, as an outgrowth of *Naturphilosophie* and Kantian epistemological pedagogy in Germany, rejected material atoms in favor of an "electromagnetic world-view," constituted of only aether. In this context it was an aberration, certainly worthy of study, but not significantly diverting the monumental course of the atomic river.
7. Celebrating Louis's centennial, Georges Lochak contributed his very useful book, *Louis de Broglie: Un prince de la science* (Flammarion, Paris 1992.) As director of the Louis de Broglie Foundation, M. Lochak rightly wishes to advertise the immense contribution that Louis made to physics. While very valuable, this book contains many undocumented quotations ascribed to Louis and takes little note of prior careful historical literature on the subject. I suggest its use *cum grano salis*. In answer to his complaint on p. 76, I cite Wheaton, "Le Duc dans la méchanique ondulatoire," in S. Deligeorges, ed, *Le monde quantique* (Paris 1985), pp 81-92 and Sciences & avenir, numéro special hors serie **46** (Avril, 1984), 42–47; Olivier Darrigol, "The origin of quantized matter waves," Historical Studies in the Physical Sciences **16**, 197–253 (1986). On "Whig history," see Wheaton, "The last word on science," History of Geophysics **3**, 31–3 (1987).
8. Student and colleague Jean Guitton describes Louis as imbued with *pudeur*, modesty almost to a fault: Jean Guitton, "Le duc Louis de Broglie: Témoignage sur l'homme que j'ai connu," La vie des sciences **9**, 331–4 (1992).
9. Dominique Pestre, *Physique et physiciens en France, 1918–1940* (Ed. des archives contemporaines, Paris 1984)
10. Some of this material appeared in a preliminary discussion before the Académie des Sciences in Paris on the centennial of Louis's birth in 1992. I was honored to be invited. See Wheaton, "Laboratory" (Ref 5).
11. The most authoritative family information, assembled by the archivists at the Academy, is in Anatole Abragam, "Louis Victor Pierre Raymond de Broglie, 1892-1997" Royal Society of London: *Biographical memoires of fellows* **34**, 22–41 (1988). Hélie Louis Charles de Bourdeille, *Généalogie de la branche française de la Maison de Broglie, 1610–1885* (Imp. apprentis orphelins, Paris 1885). Dominique de Broglie, *Les Broglie: leur histoire* (Ed. Palais Royal, Paris 1972). Nye, "Aristocratic" (Ref 5). Alfred Ruault, *Notice historique sur Broglie* (Dactylograph, Paris 1937). Jean de la Varende, *Les Broglie* (Fasquelle, Paris 1950)
12. Invited by the Pange family to peruse their family archives in Strasbourg, 1979, I was taken aback to have in my unsupervised hands holograph letters from Napoleon Bonaparte to Madam de Staël.
13. Jesus Ynfante, *Un crime sous Giscard: l'affaire de Broglie, l'opus Dei, Matesa*(Maspero, Paris 1981). Anon., "Assassinat de l'Abbé de Broglie," Le petit journal **236** (Paris: 26 mai, 1895)

14. Albert, duc de Broglie, *Mémoires du duc de Broglie*, 2 vols. (Calmann Levy, Paris 1938)
15. Gordon Wright, *France in modern times: 1760 to the present* (Rand McNally, Chicago 1960), p 284. The Paris Academiciens say it was Charles Victor.
16. Joris Karl Huysmans, *A rebours* (Charpentier, Paris 1884), chapter 12.
17. Charles Dickens, *Dombey and son* (London: 1867); here a French money lender.
18. Unfortunately, since its master, as minister for Algerian affairs, was shot to death in the streets of Paris in 1976, the library is no longer available to outsiders.
19. See Wheaton, "Enlightenment," The Physics Teacher (in press). Jacques married Augustine, god-daughter of the Duc and daughter of Francois Mérimée, caretaker of the estate. Maurice placed a bronze relief memorial to Fresnel by David d'Angers on the small chapel in Broglie, just off the main square. For Louis's view see his "La physique moderne et l'oeuvre de Fresnel," Révue de métaphysique et morale **34**, 421–40 (1927); "L'oeuvre de Fresnel et l'évolution actuelle de la physique," Révue d'optique théorique et instrumentale **6**, 552–569 (1927).
20. You *can* visit this – it is now public parkland.
21. We owe Mary Jo Nye ("Aristocratic culture," Ref 5) for her exploration of the intertwining family and financial connections surrounding our story.
22. [Pauline de Broglie], Comtesse de Pange, *Comment j'ai vue 1900*, 4 vols. (Grasset, Paris 1960–73), vol 1 (1960)
23. Louis recalled these dinners with horror. "*Every* night," he told Lochak, *Louis de Broglie* (Ref 7), p 32.
24. Pauline de Broglie, *1900* (Ref 22). Many of the Paris servants came too, even bringing some of the furniture.
25. Comtesse Célestine d' Armaillé, *Quand on savait vivre heureux* (Plon, Paris 1934)
26. Auguste and Louis Lumière, "Sur la photographie en couleurs, par la méthode indirecte," Comptes Rendus **120**, 875–876 (1895); for other sources see John Wood, *The art of the autochrome: The birth of color photography* (University of Iowa Press, Iowa City 1993).
27. Gérard Denizeau, *Musique et arts* (Campion, Paris 1995), pp 203–11; Marcel Marnat, *Maurice Ravel* (Fayard, Paris 1986), chapter 10, "Crépuscule"
28. After initial designs by Daimler, the automobile developed most sustainably with Lavassor and Peugeot; the first influential race, with a prize of 5000 old francs from Paris to Rouen, was advertised in Le Petite journal (1894) just as Maurice began his naval career. Peter Roberts, *Collectors' history of the automobile* (Bonanza, New York 1978); Jean Louis Loubet, *Citroen, Pugeot, Renault et les autres* (Le Monde, Paris 1995)
29. Margaret Cheney, *Tesla: Man out of time* (Dell, New York 1981)

30. The subtitle of Wolfgang Schivelbusch, *The railway journey: The industrialization of space and time in the 19th century* (University of California Press, Berkeley 1986)
31. Maurice de Broglie, “Application des galvanomètres thermiques à l’étude des ondes électriques, récherches faites à bord des bâtiments de guerre,” Comptes Rendus **134**, 349–352 (1902)
32. Grandmother Armaillé’s property, across from the Salle Gaveau, is a veritable castle over 6 stories high and extends north fully to the rue de La Baume. It had a permanent servant complement of over 15. When the Broglies sold it after 1901 it became, and still is, an office building, headquarters today of one of France’s largest banks. The photo in Nye (Ref 5 is only of the *porte cochère* and hardly indicates its grandeur.
33. P. de Pange, *1900* (Ref 22), vol 1, pp 173–177
34. There is some uncertainty about this. Maurice and Pauline refer consistently to 27 on the western corner, Louis (as recalled by Lochak) to 29 on the east. 27 was torn down in the 1970s. 29, shown here, was torn down in 1992, Louis’s centenary.
35. Quoted by Lochak, *Louis de Broglie* (Ref 7), p 29
36. Louis *jeune* was elegant and handsome. His sister recalled how, on the trip to Stockholm in 1929, he was the attention of many women including Princess Ingrid, later Queen of Denmark.
37. 93, rue Perronet, subsequently, but no longer, the site of the Centre Fondation Louis de Broglie.
38. And blacked out the final line on p 236. In these passages, de Broglie mentioned his abandonment in 1928 of his theory of the double soultion, his conversion to the indeterminist thesis of Bohr and Heisenberg, and his taking up again of his old ideas along with Bohm and Vigier.
39. Anatole Abragam, “Louis de Broglie: La grandeur et la solitude,” Recherche **23**, 918-23 (1992)
40. M. de Broglie, *Les premiers congrès de physique Solvay et l’orientation de la physique depuis 1911* (Michel, Paris 1951)
41. L. de Broglie, “Allocutions pronocées le 18 Octobre 1972,” *Louis de Broglie, sa conception du monde physique* (Gauthier, Paris 1973), p 384
42. L. de Broglie, “Mon itinéraire scientifique,” *Un itinéraire scientifique* (La découverte, Paris 1987), pp 33–8. Anon., “La mort du physicien Louis de Broglie: Un penseur de la matière,” Le Monde, 13107 (20 mars, 1987), 1, 15
43. This was unusual. For the usual, see Alphonse Berget, “La sciences,” *L’Avenir de la France* (Alcan, Paris 1918), 490–508. Terry Shinn, “The French science faculty system, 1808–1914: Institutional change and research potential in mathematics and physical science,” Historical Studies in the Physical Sciences **10**, 271–332 (1979). Craig Zwerling, “The emergence of the Ecole Normale Supérieur as a centre of scientific education in the 19th century,” in Robert Fox and George Weisz, *The organization*

of science and technology in France, 1808–1914 (Cambridge University Press, Cambridge, 1980), 31–60

44. One of the Poulsen magnets from I. T. & T. later became part of Lawrence's 27-inch cyclotron. See J. Heilbron, R. Seidel, and B. Wheaton, *Lawrence and his laboratory* (Lawrence Berkeley Laboratory, Berkeley, 1981), pp 12–14.
45. Providing the most dramatic photographic evidence of the armistice ever, since the signals were recorded photographically and ceased abruptly according to the time marker at 11 am on 11 November 1918. With thanks to Roy MacCleod.
46. Details in correspondence with F. A. Lindemann (later Viscount Cherwell) at Nuffield College, Oxford. For specifics, see Nye, "Aristocratic culture" (Ref 5) and Wheaton, *Tiger* (Ref 2).
47. Picture on p 236 of *Louis de Broglie que nous avons connu* (Fondation L. de Broglie, Paris 1988)
48. Louis de Broglie, "L'Oeuvre scientifique du Général Ferrié," *Savants et découvertes* (Michel, Paris 1951), 71–88
49. Since torn down to build the headquarters of, appropriately, Electricité de France.
50. P. de Pange, *1900* (Ref 22), vol 1
51. His student André Georges, quoted by Louis's successor as secrétaire perpetuelle at the Académie, Paul Germain, "Louis de Broglie ou la passion de la 'vraie' physique," in *Louis de Broglie que nous avons connu*, (Ref 47), iii–xviii, on p xi
52. Wheaton, *Tiger* (Ref 2), pp 275–283
53. Trillat, "Réminescences sur l'âge héroque de la diffraction électronique," *Louis de Broglie que nous avons connu* (Ref 47), pp 231–6, on p 232. Every wednesday at Byron the group discussed current literature and their own researches. He refers always to "les fréres de Broglie."
54. I am much indebted to Louis Leprince-Ringuet for granting me a most useful interview in 1983. I learned much also from Louis Michel (1983-1992) and from Abner Shimony (2000).
55. For as complete a list as I could find in 1992 see Wheaton, "Laboratory" (Ref 5), note 24, to which I add now Blas Cabrera, Maurice d'Ocagne, and possibly Paul Janet, Joseph Béthenod, Gabriel Hanotaux, Paul Hazard, Jules Haag. I encourage research on all of these names, particularly in French industrial archives.
56. "the affliction of a father seeing one of his children, for whose education he has made the greatest sacrifices, voluntarily ruining the magnificent situation that he has made for him and dishonoring a respected name by escapades that the principles or the prejudices of the family cannot admit." The convoluted prose is intentional by Marcel Proust, *Sodome et Gomorrhe. À la recherche du temps perdu*, vol 5 (Ed. nouvelle rev., Paris 1922), pp 70–1.

57. Hardly in France: Pierre Duhem, “Usines et laboratoires,” Revue philomatique de Bordeaux et du sud ouest **9** (1899)
58. Why this is so important in France is clear from D. Pestre, “La physique en France, 1900-1930, un panorama,” in Germain, *La découverte des ondes de matière* (Ref 5), pp 1–10, in particular his section 3.
59. Louis Leprince-Ringuet, “La vie et l’oeuvre de Jean-Jacques Trillat,” La vie des sciences **5**, 473–7 (1987)
60. Terry Shinn, “Division du savoir et specificité organisationelle: Les laboratoires de recherche industrielle en France,” Revue française de sociologie **21**, 3–34 (1980); “The genesis of French industrial research 1880 1940,” Social science information **19**, 60–40 (1980); “Progress and paradoxes in French science and technology, 1900 1930,” Social science information **28**, 659–83 (1989). See also Fox and Weisz, *Organization* (Ref 43).
61. I would very much like to arrange with national historians comparative studies of electronic laboratory genesis in Italy, Japan, and the Soviet Union to mirror those we have now for Britain, France, Germany and the U.S. This is, for me, the signal scientific advance of that century.
62. David Landes, “French entrepreneurship and industrial growth in the nineteenth century,” Journal of economic history **9**, 45–61 (1949); Harry W. Paul, “The issue of decline in nineteenth century French science,” French historical studies **7**, 416–50 (1972); Terry Shinn, “Progress and paradoxes in French science and technology, 1900–1930,” Social science information **28**, 659–683 (1989); Maurice Crosland, *Science under control: The French Academy of Sciences, 1795–1914* (Cambridge University Press, Cambridge 1992)
63. Landes, “French entrepreneurship and industrial growth,” (Ref 62); Henry Guerlac, “Science and French national strength,” *Modern France: Problems of the third and fourth republics* (Princeton University Press, Princeton 1951), pp 81–105; Paul, “The issue of decline” (Ref 62); and “Apollo courts the vulcans: The applied science institute in the nineteenth century French science faculties,” in Fox and Weisz, *Organization* (Ref 43), 155–81
64. Leprince-Ringuet, “Louis, Maurice et le laboratoire,” (Ref 1)
65. Mary Jo Nye, “N rays: An episode in the history and psychology of science,” Historical studies in the physical sciences **9**, 125–56 (1980); and her “Gustave LeBon’s black light: A study in physics and philosophy in France at the turn of the century,” Historical studies in the physical sciences **4**163–95 (1972). Wheaton, *Tiger* (Ref 2), pp 15–16
66. Charles Moureu, “La science dans la vie moderne et les conditions générales de la recherches scientifiques en France,” *Dix ans d’efforts scientifiques et industriels, 1914–1924*, 2 vols. (Chimique et industrie, Paris 1926), vol 1, pp 37–48. C. R Day, “Education for the industrial world: Technology and modern instruction in France under the Third Republic, 1870 1914,” in Fox and Weisz, *Organization* (Ref 43), pp 127–53

67. M. Moscovici, "La recherche scientifique dans l'industrie," Analyse et prévision **2**, 792–800 (1966); and "Le laboratoire dans l'industrie: Pour une sociologie de la recherche organisée," Sociologie du travail **9**, 438–47 (1967)
68. Terry Shinn, "The genesis of French industrial research, 1880–1940," Social science information **19**, 607–40 (1980), on p 621
69. Harry W. Paul, "The crucible and the crucifix: Catholic scientists in the third republic," Catholic historical review **58**, 195–219 (1972)
70. Edmond Bauer, "Souveniers sur Paul Langevin," Courrier rationaliste **2** (1956); "Interview by T. Kuhn and T. Kahan, 8–14 Jan 1963," *Sources for history of quantum physics: Inventory and report* (American Philosophical Society, Philadelphia 1967)
71. Maurice d'Ocagne, "Une grande famille académique," Echo de Paris (31 Jan. 1935)
72. Pestre, *Physique et physiciens en France* (Ref 9)
73. Wheaton, *Inventory of sources for history of twentieth century physics: Report and microfiche index to 700,000 letters* (GNT Verlag, Stuttgart 1993), pp 146–8
74. Charles Fabry, "La physique Paris," Annales de l'université de Paris **6**, 551–75 (1928)
75. Pestre, *Physique et physiciens en France* (Ref 9) discusses the striking growth afterwards.
76. Alexandre Dauvillier, *Récherches spectrométriques sur les rayons x* (Masson, Paris 1920). René Ledoux Lebard and A. Dauvillier, *La physique des rayons x* (Gauthier, Paris 1921)
77. For background see Wheaton, "Was the photoelectric effect discovered? A spatial nomograph of micro-ontology," British journal for the history of science, in press since 2003.
78. Alexandre Dauvillier, *La technique des rayons x* (Blanchard, Paris 1924)
79. Ernest Rutherford, H. Robinson, and W. Rawlinson, "Spectrum of the beta rays excited by gamma rays," Philosophical magazine **28**, 281–286 (1914)
80. M. de Broglie, "La portée de découvertes nouvelles dans la région des rayons de très haute fréquence," Scientia **27**, 102–11 (1920) and "Les aspects récent de la physique des rayons x," Société des électriciens: Bulletin (Janvier 1920)
81. M. de Broglie "La relation $h\nu = \epsilon$ dans les phénomènes photoélectriques," *Atomes et électrons* (Gauthier, Paris 1923), 80–100, p. 89 and "The photoelectric effect: The phenomena of high frequency radiation," Physical society of London: Proceedings **36**, 423–8 (1924)
82. Recall Guitton's emphasis on Louis's *pudeur*. Some of this Maurice attributed to Louis's failure in 1912 to pass a university exam in general physics (including wave motion!): "Le prince Louis de Broglie enfant" *Grandes souvenirs, belles actualités: Le recueil du jeunes* **2** (Julliet, 1948),

4–6. “La jeunesse et les orientations intellectuelles de Louis de Broglie,” *Louis de Broglie: Physicien et penseur* (Michel, Paris 1953), 423–9
83. M. and L. de Broglie, “Remarques sur les spectres corpusculaires et l’effet photo électrique,” *Comptes rendus* **175**, 1139–41 (1922)
84. Darrigol, “Strangeness” (Ref. 5) provides the most detailed analysis of how misapprehensions, misinterpretations, and outright but canceling errors encouraged the de Broglies’ inspiration, harking back at least to 1911. For even more, plow through Darrigol, *From c numbers to q numbers: The classical analogy in the history of quantum theory* (University of California Press, Berkeley 1992)
85. Wheaton, *Tiger* (Ref 2), p 301
86. Louis Michel, “Louis de Broglie: Le savant,” La vie des sciences **9**, 335–40 (1992), p 340
87. Helge Kragh, “Anatomy of a priority conflict: The case of element 72,” Centaurus **23**, 275–301 (1980). On the state of relativistic doublets see Bruce Wheaton, “T. S. Kuhn as pedagogue,” Archives internationales d’histoire des sciences **52**, 306–54 (2002).
88. Paul Forman and V. V. Raman, “Why was it Schrödinger who developed de Broglie’s ideas?” Historical studies in the physical sciences **1**, 291–314 (1969). Paul Hanle, “The Schrödinger–Einstein correspondence and the sources of wave mechanics,” American journal of physics **47**, 644-8 (1979). Silvio Bergia, “Who discovered the Bose–Einstein statistics?” in *Symmetries in physics (1600–1980)* (U. Autònoma Barcelona, Barcelona 1987). Helge Kragh, “The heritage of Louis de Broglie in the works of Schrödinger and other theoreticians,” in Germain, *La découverte des ondes de matière* (Ref. 5), pp 65–78
89. *Atomes et électrons* (Gauthier, Paris 1923), the acts of Solvay congress III (1921)
90. Maurice de Broglie, *Les rayons x* (Presses U, 1922), pp 17, 156–159. The idea was so revolutionary that a subsequent presentation to the British Association for the Advancement of Science on “X-rays and beta-rays” was listed in Maurice’s official vita as “X-rays and gamma-rays,” and is still frequently so mis-cited. See M. de Broglie, “X rays and beta rays,” British Association for the Advancement of Science: *Report* (Murray, London 1922), pp 352–353.
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92. L. de Broglie, “Allocution prononcée au jubilé de M. Maurice de Broglie le 13 juin 1946,” *Savants et découvertes* (Michel, Paris 1951), pp 298–305, on p 302
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104. L. de Broglie, *Recherches* (Ref 98), pp 127–128 (conclusion)
105. O. Darrigol, "Les premiers travaux de Louis de Broglie," in Germain, *La découverte des ondes de matière* (Ref 5), 41–51, p 48. Recall that "at age 20" was when Louis failed his general physics exams, and turned *réservé.*
106. This success was really the crux of the matter for physicists. Compare Langevin's draft critique of de Broglie's thesis, in Wheaton, *Tiger* (Ref 2), pp 295–7, with the final version presented to the Sorbonne by Perrin, in facsimile in *Louis de Broglie que nous avons connu* (Ref 47), un-numbered pages following Lochak, "Une certaine idée de la science," xix-xxxii.
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4

A Complementary Opposition: Louis de Broglie and Werner Heisenberg

Georges Lochak
(Translated by James Evans)

To make a great man of science, scientific genius does not suffice: one must also be *un grand fauve* – a great and savage beast. And all the great ones are just that, whatever their character; for all, in their manner, are great predators, capable of stalking their prey, of pursuing it, whether in full voice or in silence, of tracking it over the most torturous and unexpected ground, of seeking it in the deepest shadow or swooping on it in full light, of seizing it with vigor, of defending it against their rivals and of dismembering it, while imposing themselves, one alone, against all.

Some are powerful and imperious, such as Newton or Pasteur, however good and affable in their private lives. Others are giants with the visages of angels, such as Maxwell, or somewhat hidden behind an infinite reserve, such as Lorentz. Others emerge a little late, such as Planck, who was considered just another old professor, before he was recognized as a doughty knight holding the holy grail at the end of an interminable quest, having vanquished all obstacles in order to pronounce one day a cabalistic and incomprehensible word: *quanta*.

Let's not deceive ourselves: behind the profound and meditative visage of Bohr there was a *fauve*, for everyone knows the pugnacity with which he imposed his views on his partners in discussion. In the same way, the good, the biblical Einstein, with the hair of driven snow, was a lion. One day, according to Cornelius Lanczos (who was a coworker of Einstein), someone reported to Einstein a compliment of Nernst (who maintained a curious love-hate relationship with Einstein): " . . . if somebody made a discovery as great as $E = mc^2$, he could retire . . . ." Then "Einstein laughed and said, 'He would love that, the old rascal, but I am not going to oblige'." [1]

Well, then, Heisenberg and de Broglie were also excellent *fauves*, though very different from one another [2]. Heisenberg, of bourgeois origin, had been raised in a spirit of competition with a brother as brilliant as he, who became a chemist. I believe that, for Heisenberg, to live was to conquer, which did not prevent his spirit of a poet from flitting over the mountains of Bavaria, in the course of interminable walks, and from dreaming of a world made of atoms and quanta. One day, while he was still in high school, he said to his inseparable friend, Karl Friedrich von Weizsäcker: “You know, I believe that man will never be able to know exactly and at the same time the position and the speed of an electron.” [3] This was then without any solid physical basis, but it brings to mind the youthful dream of Einstein who, at sixteen years, saw a gull fly above the sea, in the same direction and at the same speed as the swell. The gull therefore saw the swell immobile. Einstein wondered whether one could thus “fly” at the same speed as a wave of light, and he thought that this wasn't possible! . . . so there are these premonitions [4].

De Broglie, in his early youth, did not dream of science, and I don't believe that he ever thought that to live is to conquer – for the simple reason that he descended from a family which, in the two and half centuries since it had left Piedmont to install itself in France, had produced three marshals, several ministers, including first ministers, historians of renown, diplomats, and so on. It was enough for him to believe that he would be at the level of his family, which he could not doubt, to be certain of being one day in the highest spheres of the state. Raised in chateaux or in grand houses furnished with tens of thousands of books, he saw literary, historical or diplomatic careers all open to him. But fate did not choose it so – it was to be science.

To tell the truth, it was no more evident that Heisenberg would be a physicist. Of course, he did not appear very influenced by history, certainly not in comparison with de Broglie, for whom history was a family tradition and whose first studies were historical. But Heisenberg, in keeping with an old German tradition, was steeped in philosophy and this, in the fashion of the period, was irrationalist. He frequented circles in which “one apologized” for doing physics [5]. That is to say, Heisenberg, from the beginning, was a stranger to the determinist ideas of Einstein or de Broglie.

De Broglie's family, although of Catholic ancestry, was attracted rather to the philosophy of “enlightenment” and, moreover, de Broglie felt himself to be the intellectual son of Henri Poincaré and, more distantly, of Descartes, Fermat and Fresnel. For him, his great contemporaries were Planck, Einstein, Boltzmann and Lorentz. Maxwell, too, to go back farther, had his influence on him. He had been introduced to all these thinkers by his brother Maurice de Broglie,Broglie, Maurice de his elder by seventeen years. It was his brother, too, who attracted him to modern science, and who introduced him to it first-hand in 1911 upon his return from the celebrated Solvay Congress, in which he was a participant, and for which he had with Langevin the responsibility of publishing the proceedings. He gave these proceedings for reading to his younger brother Louis, who saw the light and abandoned everything else for science.

Thus de Broglie – by family tradition, by the influence of his brother, who was already a physicist of great renown, and above all by his reading – was from the start in the line of Einstein, Planck and Lorentz. To this one should add that, having been surprised by the war of 1914 in the course of his military service, de Broglie was assigned to the radio transmission service, by virtue of his scientific knowledge, and spent five years at the radio post of the Eiffel Tower. This was for him a technical and experimental education that played a fundamental role. He said to me one day, "When one has gotten one's hands dirty starting the big alternators used for radio transmission, it is no longer so easy to believe that a wave can be only a probability of presence."

However, this is exactly what Heisenberg thought. For him, statistics were "an essential part of the foundation of quantum mechanics." [6] Determinism and causality were in his eyes only a macroscopic appearance of the microscopic haziness of a quantum world in perpetual agitation, certain aspects of which can definitively escape science. Uncertainty, for Heisenberg, as for Bohr and the Copenhagen School, was a new and intangible part of science, which found there an insurmountable barrier and a new human dimension. Here there was, without doubt, a fundamental opposition between Heisenberg and de Broglie.

In a sense, one may say that Heisenberg did not search behind the observed facts for a deeper truth, a hypothetical world susceptible of explanation, in the way that the atomic hypothesis had explained the phenomena of heat. His goal was *to rationally organize the observed facts*, which one may sum up with an aphorism of Goethe: "Search for nothing behind the facts – they are themselves the doctrine." This is why Heisenberg gladly put in the foreground an abstract representation, more eloquent in his eyes than the observed experimental object. Thus he said,"What should replace the concept of fundamental particle? I believe that we must replace it by that of fundamental symmetry." [7] This is, literally, to substitute the map for the territory.

It is for this reason that in 1925 he proposed as the basis of the future quantum mechanics an abstract algorithm destined to formalize the Correspondence Principle of Bohr, by associating matrices with the quantum states of the atom (thus the name *matrix mechanics*), with rules of calculation that physicists did not yet know. The idea was strange but genial, for it gave the theory an algebraic structure, and it corresponded well with the principle of giving a structure to the observed facts.

This is the reverse of the route that de Broglie had followed two years earlier, in 1923. For him, it was necessary to search behind the facts for the deterministic, dynamical processes capable of explaining the statistical appearance of the observed phenomena. Thus, speaking of the atomic discoveries of Boltzmann, he wrote, “That day, the veil was torn and we finally perceived with relief the physical reality that was hidden behind the abstract form of classical thermodynamics.” [8] A propos of Boltzmann’s theory, Planck also has written, weighing the difficulty of the enterprise and tending in the same sense as de Broglie, “Here it is a question of deducing a dynamical law, that is, a causal relation, from particular phenomena, starting from a statistical law.” [9]

It was with these same *a priori* theoretical predilections that de Broglie discovered the foundation of the future *undulatory mechanics*. He refused to admit the *whole numbers* of Bohr’s atom simply as given. He had to explain them. Now, the only known phenomenon giving rise to such numbers is that of *resonance*. From this came the idea that every material particle is at the same time a *wave*, which he defined by making use of the relativistic and quantum laws of Einstein and Planck, and the analogy between optics and mechanics based on the principles of Fermat and Maupertuis. We know what followed: the diffraction of electrons and of other particles, quantum statistics and the coherence of induced emission (foreseen in the famous thesis) [10].

The harvest was more than impressive on both sides and was to be crowned – two years after de Broglie, one year after Heisenberg – by Schrödinger’s equation. Moreover, Schrödinger, in a genial effort, demonstrated that the two rival theories were but one. Quantum mechanics was from then on to speak with the *voice* of Schrödinger, the *vocabulary* of waves of de Broglie, and the *grammar* of Heisenberg.

But the founders of the new theory were to remain divided into two rival groups separated by radically different conceptions of science. On the one side, Einstein, Planck, Lorentz, de Broglie, Schrödinger, Brillouin and Langevin remained the defenders of a causal and descriptive physics after the old manner, despite the recent discoveries in which some of them had played a decisive part. The other group – which is often called the Copenhagen School in homage to Bohr, and which included, besides Bohr himself, Heisenberg, Pauli, Dirac (who is actually more difficult to classify), Kramers and many others – were the champions of a formal and indeterministic theory.

The two groups collided in 1927 at Brussels on the occasion of the memorable Solvay Congress. It was the second group that triumphed, imposing a mark on science that still remains in our minds and that perpetuates itself, thanks to the textbooks. This second group, strong in its unity, presented quantum mechanics as a finished and definitive whole, which had no alternative. The other group arrived in dispersed order, each one seeking in his own direction for a response to the questions raised by the new mechanics, questions that have remained unanswered up till now.

De Broglie came back from Brussels very discouraged. However, on the platform of the station, Einstein approached him to encourage him on the path that he had marked out for himself, of searching for a representation of particles as singularities of the wave. Einstein had just independently proposed the same idea in relativity, with the famous theorem of Einstein and Grommer [11]. But de Broglie saw, above all, the mathematical difficulties in his theory that he had vainly tried for two years to surmount – and that, it must be said, have never been surmounted since. He thought that, in persisting, he ran the risk of marginalizing himself and of letting physics pass him by. This is why, against his will, he rejoined the School of Copenhagen.

I would like to add a word on the subject of the atmosphere of the Congress of 1927. I have heard it said that the "ferocity" of Heisenberg in the discussion was responsible for de Broglie's sudden change. But this is completely incorrect. It is true that, in the fever of the accession of a new theory, rivalries were exacerbated, and that, in the confrontation between those who judged that the theory was complete and those who still posed questions, the atmosphere was that of a coup d'état. It is equally true that the young Heisenberg, aged 26 years, genial and ambitious, was very aggressive. But – and what I say here was the opinion of Louis de Broglie himself – one doesn't change theories because a contradictor "isn't nice": he changed because he did not know how to respond to the objections. And he was to change again, taking up his old ideas once more, because science had evolved, as well as his own thinking. On this subject, de Broglie cited Voltaire: "It is only a stupid man who doesn't change."

Thus there was a "next time." For the three musketeers of Alexandre Dumas, there was a "twenty years later"; for quantum mechanics it was about the same. The great protagonists of the theory had preserved their force of mind, while all (except Bohr, I think) losing the ideological pugnacity of their youth. Their value was recognized, their place in history was assured, but all were, despite that, treated more or less lightly by the "young hoodlums" of physics, while they, on the contrary, knowing full well what "the others" were worth, including those to whom they were formerly opposed, more and more showed toward one another a mutual admiration, while each continued to follow his own path.

And so Dirac finished by wondering whether Einstein had not been right in his criticisms. As for de Broglie, Heisenberg and Pauli, they spoke of one another with more and more esteem. (Between Heisenberg and Pauli, this is of course obvious, but a shadow crept between them in connection with the *nonlinear theory* of particles disowned by Pauli, and then developed by Heisenberg and his students).

In the 1950s, de Broglie, who (against his own heart) had long supported the ideas of the Copenhagen School, parted company with them after having, one last time, set forth in a course the governing ideas of this school. Thirty years later, he authorized me to publish this course with a preface and notes in which I set out his new position (certain notes are his own). But it is important to emphasize that there is no aggressiveness in this work, and that one can find there the best exposition that exists of Heisenberg's uncertainty relations [12].

It was at about this time that David Bohm [13] published in the *Physical Review* the articles in which he rediscovered the pilot wave theory that de Broglie had abandoned in 1927. De Broglie was not always in agreement with his old theory, but Bohm's articles incontestably influenced him. The reason for this is that the ideas of each thinker are always influenced by the atmosphere of the time. In 1927, the atmosphere of the time was against de Broglie, whence (in part) his abandonment of the theory. In the 1950s, questions began to be raised on the subject of quantum mechanics, notably because of certain difficulties in the quantum theory of fields, as well as in nuclear physics. Quantum mechanics began to be more and more complicated, without yielding satisfactory resolutions of the new problems posed by particle physics: it was to this that Dirac and Heisenberg reacted, each in his own manner. And it was also to this that Bohm had reacted; but if he had an influence on de Broglie, it is because the latter had begun a profound change of course as early as 1946, when he recognized that he was not going to surmount certain difficulties in nuclear physics and he set himself to re-examining the foundations of quantum theory.

It was at the end of the 50s, if my memories are correct, that Heisenberg came to Paris, and I had the honor of attending his lectures on his nonlinear theory – an audacious theory which, like de Broglie, put back at issue the foundations of quantum theory, even if one did not recognize this at the time. As for me, I was very impressed by Heisenberg, I listened to him with a passionate interest, and I discovered a great savant who, like Einstein, like de Broglie, like Dirac, visibly wondered whether what he was saying was true. His intelligence was intact, but he was not brilliant. He was only profound.

When all was said and done, Heisenberg had as high an opinion of de Broglie as de Broglie had of him, despite their divergences. In a book printed in celebration of de Broglie, Heisenberg remarked that de Broglie's theory of light "raised questions as important as the discovery of matter waves." There was a happy and recent epilogue to that: Harald Stumpf (a friend and former student of Heisenberg), I myself (friend and former student of de Broglie) and Thomas Borne (former student of Stumpf), have published together a book on de Broglie's theory of light, developed by starting from some ideas of Heisenberg [14].

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5

Schrödinger Against Particles and Quantum Jumps

Michel Bitbol

Introduction

It may seem paradoxical that Erwin Schrödinger, a major creator of quantum physics, did not believe in particles or in quantum jumps [1]. For, at first sight, the behavior of atoms and elementary particles is precisely the object of quantum mechanics. Moreover, it seems that particles bump into each other in particle accelerators, that they are observed in bubble chambers, and that therefore one would just flatly ignore an experimental fact by denying them. As for the criticism of quantum jumps, it appears to be tantamount to a rejection of one the most salient and characteristic concepts of quantum mechanics.

However, it will soon be clear that all these arguments against Schrödinger's views are based on prejudice. On the one hand, one must not forget that, except perhaps in the framework of hidden variable theories, speaking in terms of elementary particles can only be done either at the cost of alleviating the corpuscularian connotation of the word particle, or by restricting the relevance of this word to a certain class of experimental situations. In modern physics, "particle" is a word that is carefully redefined in such a way that it does not really mean what it so strongly evokes. On the other hand, the quantization algorithm that is typical of quantum mechanics does not fit with the usual pictures according to which each object is in a given eigenstate at a certain moment, and then jumps from one eigenstate to another one. Quantization does not entail quantum jumps, and therefore denying quantum jumps does not mean rejection of quantization.

Let me add a further remark. Two major advances in quantum theories have led some prominent authors to wonder whether the concepts of particles and quantum jumps are not mere remnants of past stages of physics. In 1984, Paul Davies published a paper entitled "Particles do not exist" [2], basing his dismissal on quantum field theories. Paul Teller [3] later argued convincingly against the usual notion of quasi-individual labeled particles, and he advocated a radical switch from the quasi-classical concept of n particles present in a certain volume, to the quantum field theoretical concept of *propensity to display n-quanta states* in this volume. As for H.-Dieter Zeh, he published in 1993 a paper entitled: "There are no quantum jumps, nor are there particles!" [4] The title of his article was overtly aimed at answering Schrödinger's well-known questions: "Are there quantum jumps?" [5] and "What is an elementary particle?" [6] Interestingly, H.-D. Zeh drew his arguments from the decoherence theories of which he is one of the leading specialists. More recently, E. Joos, Zeh's closest collaborator, undertook to explain in detail this Schrödinger-like statement in his website, and also in a textbook [7]. According to him, since decoherence (of a global state vector) is enough to explain why certain appearances seem to be localized in space, this means that the concept of particle is no longer needed. Similarly, since decoherence is enough to explain "why microscopic systems are usually *found* in their energy eigenstates (and therefore *seem* to jump between them)" [8], this means that the concept of quantum jump is no longer needed. Joos and Zeh thus gave Schrödinger's questions about particles and quantum jumps the negative answer Schrödinger would have approved, by grounding this negative answer on the essentially wave-mechanical calculations Schrödinger would have recommended.

In view of this growing consensus, it will prove quite instructive to analyze the reasons Schrödinger gave for his disbelieving in particles and in quantum jumps.

5.1 Dispensing with Particles

What exactly is a particle, in the primarily classical sense which was retained by Schrödinger? It is a small localized material body whose three indispensable features are the following:

(i) it can be ascribed permanent *properties*,

(ii) it has individuality,

(iii) it can be re-identified through time.

These three basic features can be taken :

A – As basic components of the *ontological* concept of a particle.

B – As expressions of functionally distinct elements of *speech*. Indeed, ascription of properties corresponds to predication. Individuality corresponds to indexical reference by demonstrative pronouns like "this." And reidentifiability corresponds to reference by names, for, as Kripke rightly pointed out, naming involves implicit reliance on an initial act of baptism, and on the possibility of monitoring the trajectory of the body from this act of baptism on.

C – As *epistemological* requirements for ascribing a certain complex of phenomena the status of a "particle." Thus, saying that something has a "property" means that one can rely confidently on certain *virtual* observations expressed by *counterfactual* empirical propositions (such as "if I had performed such and such experiment, I would have obtained such and such outcome"). Saying that a certain body has individuality means that certain experimental criteria of individuation (or mutual discrimination) have been or can be successfully applied to the phenomena that "manifest" it. And claiming that it has trans-temporal identity means either that these criteria of individuation are permanent, or that there exists a spatio-temporal world line that establishes the continuity between a phenomenon at one location and a phenomenon at another location.

Interestingly, Kant, who developed this kind of epistemological reading of (classical) bodies, and who was one of the major philosophical sources of Schrödinger, only retained features (i) and (iii) for his concept of "substance." According to him, a substance has to be a property bearer and be permanent. Being able to ascribe permanence to a certain complex of phenomena is a basic condition of possibility of knowledge, since without it, an enduring base for defining time itself would be missing. Moreover, this permanent aspect is bound to correspond to the substrate of properties [9]. The reason why feature (ii) was not retained by Kant as constitutive of the concept of substance, is that he believed "substance" can ultimately be assimilated to a mass-term, like the quantity of matter, and therefore is not individualizable. A body, which is in principle ascribed individuality, can be treated as a "substance" for practical sake, but not in principle.

Let me now review in turn how the three basic features of a particle were considered by Schrödinger as *absent* in the microscopic world.

The concept of virtuality (which would provide epistemological ground for the formal concept of "property") was very soon recognized as a cornerstone in the debate on the interpretation of quantum mechanics. As early as 1926, Einstein challenged Heisenberg's positivist-like strict adherence to effectively performed experiments. He believed that one could not dispense with introducing some version of the modal category of the *possible* in the reasoning, and only retain the *actual* [10], lest one lose the very content of the notion of a real object *on which* experiments are performed. In a conference of 1928, Schrödinger went even farther than Einstein in stressing the decisive importance of virtualities as a basic ontological constituent. Whereas Einstein considered the "virtual" or the "foreseeable" as a *component* of reality, Schrödinger *defined* reality as a construct made out of a proper combination of actual and virtual material: "That is the *reality* which surrounds us: some actual perceptions and sensations become automatically supplemented by a number of virtual perceptions and appear connected in independent complexes, which we call existing *objects*." [11]

This sentence defines Schrödinger's peculiar use of modalities in these circumstances. Firstly, the "virtual" perceptions, observations, or experimental results that constitute a real object are not *exclusive* of one another. They are associated in "complexes"; they are construed as co-existent. Secondly, the justification of their being linked in such a way is that they are experimentally *accessible* at any moment. The virtualities are conceived by Schrödinger as the modal expression of expectations [12]. A virtual observation is not only an observation which could have been made, but an observation which can be made in the future provided the appropriate experimental conditions are fulfilled.

Of course, one has to qualify this condition of permanent accessibility to the virtual observations, in order to make it applicable to the most familiar situations of daily life. An ideal accessibility presupposes that no change whatsoever happens between the instant when the actual observation is made and the instant when the conditions of the expected observation are fulfilled. However, immutability usually does not obtain. Some disturbances may occur, or the system may be submitted to an evolution law which modifies its state in the interval. It is thus indispensable to *modulate* the condition of accessibility. Let us consider, for instance, a classical material point. Its position having been measured at time t, one has the right to say that the momentum value is virtually coexistent with it provided a measurement of the momentum can be performed at any later time, and the result of the two measurements are strictly connected through an appropriate operator of evolution.

From this condition, it becomes clear that the condition of accessibility of virtual observations is deeply connected with the possibility of *interpolating* between actual observations. Now, the fact that any interpolation of the trajectory of microscopic bodies is submitted to Heisenberg's uncertainty relations gave Schrödinger a good reason to be pessimistic about "...whether in this case, in principle, virtual observations are at all conceivable, on which the real existence of these objects can be based." [13] True, the idea that particles have some underlying properties of the usual sort, even though they are disturbed [14] in an *uncontrollable* way by the measurement, could still be sustained at this early stage of the debate about the meaning of quantum mechanics. And such a possibility would have been sufficient to maintain, at least formally, the concept of virtuality in spite of the uncertainty relations: the value of any observable could have been considered as virtually coexistent with the effectively measured value of another incompatible observable, *modulo* an evolution factor involving appropriate (but uncontrollable) disturbance terms. But Schrödinger found it increasingly difficult to accept this artificial conception. He had formulated his own version of no-hidden-variable theorems in 1935, and he claimed in the 1950's that the "belief" according to which the particles possess virtual values of every observable (as hidden variable theorists would contend) is not justified.

Of course, we know nowadays that such von-Neumann-like no-hidden-variable theorems do not rule out just *any* hidden variable theory, but only a very restricted class of such theories. We also know that further theorems about hidden variables, such as Bell's, only rule out certain classes of theories: especially the *local* hidden variable theories. Some hidden variable theories, such as Bohm's, belong to the class of those theories that are ruled out by *none* of the listed theorems. Could then Schrödinger have changed his mind about the possibility of ascribing simultaneously values to all observables to particles, in view of Bohm's theory? I guess he would not have been convinced. For on the one hand, even though he was perfectly aware of the ancestor of Bohm's theory, i.e. de Broglie's pilot wave theory, he showed no sign of attraction towards it. On the other hand one may also guess that Schrödinger would have shown very little enthusiasm for Bohm's type of hidden variable theories, due to the fact that this theory stands quite far from the epistemological standards he was eager to maintain. Completion in thought could not mean for him completion by something which is *in principle* out of reach of any kind of experimental assessment. But the contextuality of Bohm's theory prevents one *in principle* from testing experimentally the claim it makes, namely that the value of all the relevant properties of a particle are simultaneously determined at any time. There is thus no reason to think that Schrödinger would not have applied the same kind of Ockham's razor to this theory as the one he was ready to apply to his own wave mechanics [15] in 1926:

do not add empirically empty "clothing" to the structure of quantum mechanics just for the sake of satisfying the desire for pictures.

So, when specifically directed to the observables position and momentum, Schrödinger's remarks about uncertainty relations and his rejection of hidden variable theories led him to the conclusion that the particles *cannot even be ascribed anything like a continuous trajectory*: "Observations are to be regarded as discrete, disconnected events. Between them there are gaps which we cannot fill in." [16] More precisely, we cannot fill them in *according to a trajectory pattern.*

At this point, the over-revolutionary attitude of Schrödinger arises. Is it coherent to keep on speaking of "particles" if they have nothing like a trajectory? Schrödinger's answer is a definite *no*. When he asked "what is a particle which has no trajectory or no path?" [17], it was just a somewhat ironical way of emphasizing that "...the particles, in the naive sense of the old days, *do not exist*." [18] Some years later, he confirmed most clearly this equivalence between *no trajectory* and *no particle at all* in a letter to Henry Margenau: "To me, giving up the path seems giving up the particle." [19] The reason for this strict implication is to be found in Schrödinger's combined meditation about individuality and trans-temporal identity. The "individual sameness" of the macroscopic bodies which surround us is ascertained, according to him, by their "*form* or *shape* [German: *Gestalt*]" [20], including some imperceptible details that distinguish them permanently from other bodies of the same kind. The elementary particles can also be ascribed a form (at least in the most abstract sense of *determinant*), even though it is likely to be nonsense in their case to say that this is the form *of some material substratum.* But the said form can but define their species; it does not help to single out each one of them and to identify it through time. Instead, one must revert to another criterion in order to ascertain the individuality and identity of the particles.

The alternative criterion, in classical mechanics, is merely their having distinct positions at a given instant, these positions being connected to distinct past histories through different continuous trajectories. This criterion was called "genidentity" by H. Reichenbach after K. Lewin [21]. As Schrödinger himself noted in his letter to Margenau, it was already proposed by Boltzmann [22] in 1897: "The discontinuity removes the univocal identification. Would you believe it, that Boltzmann, in his *Principe der Mechanik*, right in the beginning, underlines this point in what he calls his *Ertes kinematisches Grundgesetz.* This was a few years before Planck's great discovery, I think about in 1897." [23]

In quantum mechanics, however, we already know that the particles, if any, cannot be ascribed a trajectory. The ultimate criterion of permanent individuality thus collapses. True, there is still a possibility to rescue something of the old concept of individual and trans-temporally reidentifiable body. It is to say, as most contemporary physicists do, that two groups of circumstances are to be distinguished: the circumstances where the range of uncertainty of two trajectories overlap, and the circumstances where they do not overlap. In the first case, the particles have no definite individual identity, whereas in the second case, they have one [24]. But Schrödinger rejected this expedient from the outset. According to him, "Even if you observe a similar particle a very short time later at a spot very near to the first, and even if you have every reason to assume a causal *connection* between the first and the second observation, there is no true, unambiguous meaning in the assertion that it is the same particle you have observed in the two cases. The circumstances may be such that they render it highly convenient and desirable to express oneself so, but it is only an abbreviation of speech; for there are other cases where the 'sameness' becomes entirely meaningless; and there is no sharp boundary, no clear-cut distinction between (the two types of circumstances), there is a gradual transition over intermediate cases." [25] Even if two "particles" are experimentally located very far away from each other, even if the standard deviations on their positions do not overlap, there is still a small probability that an "exchange" has occurred between them. The distinction can thus be performed *in practice*, but its possibility is ruled out in *principle*: "I beg to emphasize this and I beg to believe it: It is not a question of our being able to ascertain the identity in some instances and not being able to do so in others. It is beyond doubt that the question of 'sameness', of identity, really and truly has no meaning." [26] In principle, there is nothing like two *distinct particles*. There is thus nothing like an individual and trans-temporally reidentifiable particle; and, Schrödinger concludes, there is thus nothing like a particle: "...I must warn of a misconception which the preceding sentences may suggest, viz. that crowding only prevents us from registering the identity of a particle, and that we mistake one for the other. The point is that there are not individuals which could be confused or mistaken one for another. Such statements are meaningless." [27]

The final claim that the difficulties in identifying a given particle and distinguishing particles from one another makes the very concept of individual particle *meaningless* is not explicitly justified, but it is not difficult to figure out a sound reason for it. Indeed, if one cannot ascribe with certainty a given droplet in a cloud chamber to a given particle, then, one cannot in general ascribe the droplet to *another* given particle either. The absence of a criterion for ascertaining the sameness of *one* "particle" is all-pervasive and challenges the very possibility of making sense of the concept of an individual particle. Each observation must eventually be considered as an isolated event, not to be related to any kind of spatio-temporal continuant; the particle itself accordingly dissolves in one or several scattered events: "When you observe a particle of a certain type, say an electron, now and here, this is to be regarded an isolated event." [28] It is only the superficial linear appearance of some gatherings of events (i.e., tracks in Wilson cloud chamber) that tends to remind one of the trajectory of a particle. But, according to Schrödinger this must be considered as an illusion: "…it is better to regard a particle not as a permanent entity but as an instantaneous event. Sometimes these events form chains that give the illusion of permanent beings." [29] Just the same type of illusion as the one which is widely know in psychology under the name "phi-effect," where *two static* spots of light being successively (and very quickly) switched on, they are seen as a *single moving* spot.

Quantum statistics and quantum field theories provided Schrödinger with an additional argument. In his paper "What is an elementary particle?", Schrödinger gave a very simple and very clear illustration of how the *new* (Bose–Einstein and Fermi–Dirac) statistics could be obtained [30]. Let us first suppose that we distribute a certain amount of money between several persons. Provided this amount of money is divided in finite quantities, the number of different distributions is given by the Bose–Einstein formula. Let us then suppose that we distribute "vacancies in a football team" between several persons. Once it has been noticed that one person cannot be offered more than *one* vacancy, it becomes clear that the number of different distributions is given by the Fermi–Dirac formula. The surprise comes when the metaphor is translated in terms of the relevant physical entities. The persons (individuals) stand for the *states*, not the particles; and the amounts of money or football club vacancies (non-individuals) stand for the particles. "The example may seem odd and inverted. One might think, 'why cannot the people be the electrons and various clubs their states? That would be so much more natural.' The physicist regrets, but he cannot oblige. And this is just the salient point: the actual statistical behavior of electrons cannot be represented by any simile that represents them by identifiable things." [31] With this illustration, one understands that quantum statistics strongly suggests a kind of ontological inversion. In the classical paradigm, the particles were ascribed the grammatical status of subjects of propositions and the states acted as predicates of the particles; but in the quantum paradigm, it is much more natural to consider states as subjects, and the numbers of each variety of quanta in these states (or their statistical distributions) as predicates. This inversion is in good agreement with the Fock-space formalism of quantum field theories when taken at face value.

So, here are the three basic reasons for which Schrödinger did not believe in particles: in general, they do not incorporate coexistent virtualities, they cannot be reidentified through time, and they do not play the role of individual substances bearing properties.

However, the consequences of this criticism of the concept of particles were not fully developed by Schrödinger himself. As J. S. Bell [32] pointed out, Schrödinger even recognized in his paper "Are there quantum jumps?" that he had no clear idea about how to account in detail for tracks in cloud (or bubble) chambers in his anti-corpuscularian framework of thought. As I mentioned earlier, Schrödinger had a definite idea about what the tracks do *not* show (namely the trajectory of a spatio-temporal continuant called a "particle"); but he remained quite hesitant about what they *do* express. Yet, two authors who quickly assimilated the spirit of Schrödinger's discovery in the late 1920's proposed a very satisfactory wave mechanical account of tracks. These authors were C. G. Darwin [33] and N. F. Mott. Let me state their ideas, before I discuss Schrödinger's nuanced views on tracks.

In 1929, Nevill Mott's published a celebrated paper entitled "The wave mechanics of α-ray tracks." [34] In this paper Mott followed quite closely the interpretation of wave mechanics that had been developed by Charles Galton Darwin [35] a few weeks earlier in 1929, but he made these ideas more precise.

The initial motivation of both Darwin's and Mott's papers was Gamow's theory of radioactive disintegration of 1928 [36]. In this theory, the emission of α-rays was explained wave-mechanically by means of potential-barrier penetration. Now, the problem is that as soon as they have emerged from the nucleus, the α-rays *appear to* have essentially corpuscle-like properties, for they give rise to tracks in cloud chambers. Charles Galton Darwin's project was then to restore conceptual homogeneity between the explanation of the radioactive emission (which is based on pure wave mechanics) and the account of detection (which apparently must involve corpuscularian categories). He wished to make sense of the α-ray tracks without resorting to the process that consists in imagining that at each observation "the wave [turns] into a particle and then back again [into a wave].” [37] He wanted “to show how a discussion only involving the wave function Ψ would give spontaneously the results which simple intuition would suggest could only be due to particles.” [38] As for Mott, he also insisted that “the wave mechanics unaided ought to be able to predict the possible results of any observation that we could make on a system.” [39] But how is it possible? According to Darwin, in order to account for the tracks, the relevant wave function must contain factors corresponding not only to the α-particle, but also to every ionizable atom in the cloud chamber. “Before the very first collision, (the wave function) can be represented as the product of a spherical wave for the α particle, by a set of more or less stationary waves for the atoms .... [The] first collision changes this product into a function in which the two types of coordinates are inextricably mixed.” This is a very clear early statement of what we now call the *entanglement* of wave functions after Schrödinger’s papers of 1935 [40]. As for Mott, he noticed that “... we are really dealing with wave functions in the multispace formed by the coordinates both of the α-particle and of every atom in the Wilson chamber.”

Now, what about the *interpretation* of the wave function? According to Mott and Darwin, the quantum mechanical account, including when it uses entangled wave functions, does not provide the slightest element of *description* of the putative processes underlying the phenomenon; it only enables us “to *predict* the possible results of any observation.” In other terms, “interpreting the wave function should give us simply the *probability* that such and such an atom is ionized.”

Then, Mott and Darwin insisted that the multidimensional wave-mechanical account must be pushed as far as possible, and that any reference to particles or to discontinuous processes must be delayed as much as possible. This procedure is fully coherent, insofar as it consists in developing continuously the predictive formalism until the stage where a probabilistic prediction is required, rather than mixing up continuous predictive elements with unwarranted discontinuous descriptive stories. In Mott’s terms, “Until this final [probabilistic] interpretation is made, *no mention should be made of the α-ray being a particle at all.*” As for Darwin, he took this delay as the pivotal concept of his interpretation of quantum mechanics. His major aim was to show “how it is possible to postpone speaking of particles,” for according to him, “there is no need to invoke particle-like properties in the unobserved parts of any occurrence, since the wave function Ψ will give all the necessary effects.” Each entangled wave function can be read as a disjunction of conditional probabilistic statements, relating one ionization to a series of other ionizations approximately located on the straight line joining the radioactive nucleus and the first ionization. While the probability of the first ionization is evenly distributed, the conditional probability of obtaining an approximately straight track following this first ionization is very high.

The interpretative strategy used by Heisenberg in his *Physical Principles of the Quantum Theory* to account for the tracks was quite different. Unlike Mott and Darwin (and owing to the influence that Bohr had exerted on him), Heisenberg had no reluctance to jump from corpuscle representation to wave representation and back again whenever it appeared convenient to do so. He considered that including the α particle and the ionizable hydrogen atoms of the cloud chamber within the same compound system, or taking the α particle as the only system and the ionizable atoms as part of the observation device, is a matter of free choice. A cut has to be introduced somewhere between the system and the observation device, but, says Heisenberg after Bohr, the location of this cut is almost arbitrary; it only depends on pragmatic considerations.

Now, even though Heisenberg's method on the one side and Mott's and Darwin's on the other side are equivalent from a purely pragmatic standpoint, they are not from an intellectual standpoint.

The method of successive wave-packet reductions is usually much simpler, for it consists in using the information afforded by each point-like observation to extract a new wave function for the α particle alone out of the compound wave function of the larger system consisting of the α particle and an ionized atom. The problem is that one usually forgets that successive reductions are by no means *changes* of the initial wave function, but rather *redefinitions* of it for practical reasons. As a consequence of this forgetfulness, the discontinuous evolution of the wave function is taken as a sort of *descriptive* account of the process that gives rise to the track, and this arouses spurious questions about the physical mechanism of the wave packet reduction. By contrast, the method of the entangled wave-functions has the merit of permanently maintaining a clear distinction between the predictive continuous model and the series of predicted discontinuous events.

There also is another significant intellectual difference between the two attitudes. Heisenberg's insistence that corpuscularian categories are good enough to explain tracks in cloud chambers may be taken as an incentive to forget in the long term Bohr's cogent statement according to which the corpuscular picture is *relative* to a certain class of experimental situations, or to a certain mode of analysis of experiments, and that one should therefore avoid taking it at ontological face value. By contrast, holding on to the wave-mechanical model until the very moment when the probability of a series of ionizations is to be calculated enables one to *bypass* completely the corpuscularian categories, and thus to avoid taking them too seriously.

Mott's calculation may thus be considered as a concrete development of Schrödinger's devastating criticism of the concept of particle. Elaborating on Schrödinger's denunciation of "the illusion" of permanent body-like beings in microphysics, Mott completely dispelled that illusion by using appropriate theoretical methods.

But why did Schrödinger himself not develop or advocate some Mott-like account of α ray tracks? Some hints about this question can be found in the last section of his paper "Are there quantum jumps?". This section is characteristically entitled "Observing single particles." To begin with, here as in other texts, Schrödinger apparently accepts several components of Mott's solution. He insists that the tracks in cloud chambers "... only represent a small section of all that we know about nature," where the expression "all that we know" obviously refers to the sum of information conveyed by the wave functions. He points towards the *entanglement* of the wave functions of the various elements which partake of the experiment, when he evokes "... wave parcels in more than three dimensions, actually three times the number of particles that come into play." He insists that accounting for the tracks is not a simple job to be performed by reading them out literally as the trajectory of a point-like particle, but a very difficult problem of mathematics: "This is witnessed by the pages and pages of intricate formalism that are devoted to account for even the simplest of them." Those "pages of intricate formalism" are likely to allude to Mott-like calculations involving some heavily entangled wave functions. Schrödinger even comes very close to Mott's minimalist conception of the wave function when he declares that "interpreting" it could mean nothing more than connecting it with some parameters that characterize a statistical distribution of experimental events: "... the interpretation can be stated in one sentence: the expectation value of an observable is the inner product of the actual wave function into the function that results from operating on the same by the operator associated with the observable in question." [41] Schrödinger then concludes that, even if it were true that wave mechanics has no proper *explanation* in store for the tracks in cloud chambers, but only a consistent method for predicting it, this deficiency would be nothing when compared to the defects of the orthodox view that a wave function only represents the probability of a *particle's* being here *or* there: "if you accept the current probability view ... in quantum mechanics, the single-event observation becomes comparatively easy to tackle, but all the rest of physics ... is lost to sight." In other terms, according to Schrödinger, one should pay more attention to the ability of the wave mechanical formalism to provide a general law-like connection between the phenomena, and less to its apparent failure in *explaining* the details of the phenomenon of tracks; for any such explanation is bound to be very local (not to say *ad hoc*), and to provide no clue for the complete network of experimental events.

To sum up, Schrödinger came very close to Mott's wave mechanical predictive account of tracks in cloud chambers, but he remained slightly uncomfortable about it. There are two reasons for his discomfort. Firstly, although Schrödinger was one of the main discoverers of the concept of entanglement in 1935, his writings still held the mark of his initial interpretation of the Ψ function as a representation of a real wave in 3-dimensional ordinary space. For instance, in the body of his 1952 paper "Are there quantum jumps?" he dealt *separately* with the characteristic frequencies of the vibrating processes associated with *each* subsystem. And even when he explicitly acknowledged the necessity of using $3n$-dimensional compound wave-functions, he called them a purely "auxiliary concept." [42] Schrödinger's enduring nostalgia for a descriptive value of (3-dimensional) wave functions here contrasts with Mott's unrestricted acceptance of the purely predictive use of their $3n$-dimensional counterparts. Secondly, unlike Mott, Schrödinger was quite reluctant to declare that wave functions are essentially tools for calculating *probabilities*. However, this does not mean that he rejected indeterminism [43], let alone that he denies the essentially stochastic connection between wave functions and experimental phenomena. This only means that Schrödinger could not accept the dominant *ignorance* connotations of the word "probability." To him, it was absurd to think that the wave function is only a statistical assessment of some (knowable or unknowable, but presently unknown) underlying process that concerns *particles*. Now, Mott did not speak of the probability of a particle's being here or there, but only of the probability of an atom's ionization (or, more generally, the probability of an *experimental phenomenon*). If such a restriction is put on the use of the term "probability" in quantum mechanics, one has no reason to bypass it, even from Schrödinger's standpoint. It is thus clear at this point that full acceptance of Mott's account of tracks in cloud chambers by Schrödinger would only have needed some clarifications about the status of $3n$-dimensional wave functions, and about the concept of probability. Defeating the strongest experimental argument in favor of a corpuscularian view of the world would then have proved quite easy.

5.2 Getting Rid of Quantum Jumps

Let's now come to the problem of quantum jumps. In order to understand Schrödinger's strongly negative attitude towards quantum jumps, one must keep in mind two aspects of his thought. The first one is metaphysical and the second one is methodological.

Schrödinger's metaphysical outlook started from a sharp criticism of the naive realist view that there are intrinsically existing objects out there that impinge on our bodily senses and that explain our inter-subjective agreement about them. To Schrödinger, this common sense conception is the result of our wrongly endowing with intrinsic existence those aspects of *phenomena* we had isolated at first from the continuum of *experience* during the so-called process of "objectivation." "Objectivation" only provides them with some *fake autonomy* with respect to individual perceptions and emotions. Accordingly, one could say, in good agreement with the so-called "theory of identity" Schrödinger overtly borrowed from the Indian *Advaita Vedânta*index*Advaita Vedânta* (a phrase that can be translated as "non-dualist outcome of the Vedas"), that there is no *true* duality between these objects and ourselves.

This version of non-duality is repeatedly expressed in Schrödinger's successive writings. In *Mind and Matter*, for instance, we read: "No single man can make a distinction between the realm of his perceptions and the realm of things that cause it, since however detailed the knowledge he may have acquired about the whole story, the story is occurring only once and not twice. The duplication is an allegory suggested mainly by communication with other beings and even with animals; which shows that their perceptions in the same situation seem to be very similar to his own, apart from insignificant differences in the point of view." [44] In the second part of *My View of the World*, which was written in 1960, one year before his death, Schrödinger develops the basics insight of his criticism of the dualist theory of knowledge, by relying on some arguments which have become classical in Western philosophy after Kant. To begin with, the idea that there exists an object beyond our representation of the world, which somehow causes this representation in us, appears superfluous to him. It does not even explain our inter-subjective agreement about the world, because it just duplicates the mystery of this agreement by adding another mystery to it: the mystery of a thing-in-itself which is inaccessible, except by means of the very representation it is supposed to cause in us. Even if we are not deterred by the strange assumption of something to which our representation conforms, but whose conformity to this representation we shall never be able to assess by comparing them directly, we must beware of the spurious use of the concept of *causality* when we refer to the relation between the thing-in-itself and the representation. For, as we know since Kant, says Schrödinger, causality is basically a category of understanding which only applies to relations between phenomena, namely to relations which are internal to our representation. It would be an abusive extension to apply it to the relation between this representation as a whole and something which completely transcends it.

Thus, according to Schrödinger, as well as Schopenhauer, the world is the experiential and/or theoretical representation itself; it is not an elusive something *beyond* the representation, which is supposed to be *re*-presented. Even objectivity has been reached by a process which is immanent to representation; it has nothing to do with reference to a thing which transcends representation. On the other hand, we can say that it is just the remarkable success of objectivation, and especially the efficient stabilization of the objective construal of the world by language and science which favors the illusion of a transcendent world of intrinsically existing objects. Something like a collective dream prompted by the social conventions of language.

This background metaphysics did not intrude into the contents of Schrödinger's physics, but it had a strong influence on his directions of research and on his favorite metaphors. This influence can especially be seen in his predominantly holistic views. From the very beginning of his reflection on quantum physics, in 1925, Schrödinger thus advocated a view according to which the particles and atoms should not be construed as individual little bodies isolated from one another, but as modes of vibration of a single background that he later identified with the universe as a whole (including ourselves). His paper of 1925, on the Bose–Einstein statistics, is full of beautiful sentences which say, in short, that particles are but *wave-crests*, or a sort of *froth* on the deep ocean of the Universe. These metaphors are strikingly similar to the Buddhist or Vedântic image of waves on the ocean, or of bubbles in the air (that Schrödinger knew and evoked), when the relation between the individuals and the absolute reality is to be evoked.

Now, one must realize that in such a metaphysics, where there is no true distinction between the representation and the represented world or, as expressed by Schopenhauer in the title of his most famous work, where the world is nothing else than our representation, any condition imposed on our experiential and then on our theoretical representation is *ipso facto* a condition imposed onto the world. Seeing that our experiential representation is a spatio-temporal continuum, and that our theoretical representation must also follow these standards of continuity (for reasons that are expounded in the paragraphs below), we have to accept that no discontinuity may occur in the world either.

But of course, this metaphysical outline only explains Schrödinger's *motivation*, not to mention his personal *bias*, when he criticized the picture of quantum jumps. It does not provide us with a true argument against quantum jumps. So, at this point, we have to turn to the second relevant aspect of Schrödinger's thought: the methodological one.

Quite apart from his metaphysics, Schrödinger imposed very high standards onto theory construction. One of these standards is *absolute precision* and perfect clarity. According to him, this standard can only be reached if the model goes well beyond the previously observed facts: "... the desire for having a *clear* picture necessarily led one to encumber it with unwarranted details." [45] One must integrate in the model not only the *actual* experimental results, but an infinity of *possible* results; one must perform a systematic "completion in thought" [46] of the recorded observations. And the only acceptable proof that this process of *completion in thought* has been completed is the disappearance of any *gap* in the picture, namely its *continuity*.

This continuity condition was already stated by Schrödinger in 1929 [47], when the prospect of forming a satisfactory continuous picture of atomic phenomena seemed quite remote: "... we are bound to supplement our immediate observations, in order not to be left with a patchwork of individual facts instead of reaching some sort of 'Weltbild'." [48] It was then reformulated more assertively in 1950: "... from an incomplete description – from a picture with gaps in space and time – one cannot draw clear and unambiguous conclusions; it leads to hazy, arbitrary, unclear thinking – and this is the thing we must avoid at all costs!" [49] According to Schrödinger, there was thus no doubt that the gaps in our pictures *had* to be filled in. The only problem was that, at the birth of quantum mechanics, nobody could figure out how this aim was to be reached.

As I noted in the previous section, the most natural way of filling the gaps, when what is observed consists, say, of a series of dots in a cloud chamber, is bare interpolation [50]. One is thus tempted to insert more and more imaginary dots between the actual dots, and to make them smaller and smaller until they form something like a continuous trajectory. But Heisenberg's uncertainty relations have demonstrated that this is just impossible [51]. The process of making the dots smaller and smaller, as well as closer and closer, would only result in increased dispersion. It would lead to a cloud of points which by no means looks like a corpuscular trajectory. In other terms, no deterministic link between the observed dots, in which the former infinitesimal fragment of trajectory (together with the local potential) fixes univocally the later infinitesimal fragment of trajectory, can be established. The only link between the observed dots is a probabilistic one.

The alternative solution is then to fill in the gaps by means of the very continuous theoretical entity which serves as a tool for calculating the probabilistic link between the observed dots, namely the Ψ-wave. But this procedure sounds utterly unnatural, due to the obvious heterogeneity between the extended wave-like filling material and the point-like observed facts. As Heisenberg first pointed out in his celebrated 1927 paper (see Sect. 5.1) if it is to fit with each observed dot, the Ψ-wave must be "reduced" to a wave packet whose size is of the order of magnitude of the precision with which the corresponding position measurements have been performed [52]. Thus, even if a kind of *spatial* continuity between the cloud chamber dots is established by means of the Ψ-wave, one still has to cope with the *temporal* discontinuity implied by the successive "reductions" of the Ψ-wave. Schrödinger's strategy thus consisted in doing much more than just filling in the gaps *between* the observed dots. It consisted in ascribing an *absolute priority* to the continuity of the Ψ-wave picture over the discontinuity of the dots. In short, his quite bold prescription could be formulated thus: *if the observed facts do not fit with the continuity of the picture, then just eliminate the facts from the picture.* And the consequence was that he pushed the concept of fact to the edges of the scientific thought, only relating the facts indirectly to the picture through probabilistic correspondence rules.

But, at this point, an embarrassing question arises: is this purely continuous theory, completely freed from the obligation of incorporating something of the experimental discontinuities in the course of the time-development of its entities, able to account for the experimental effects which prompted the introduction of the quantum of action by Planck at the turn of the century?

Here, we are reaching one of the most surprising chapters of the history of quantum mechanics, a chapter where the emotionally rooted convictions and the sociological predominance of the Göttingen–Copenhagen group managed to mould the opinion of the majority of physicists during a full half-century, even against the clearest theoretical evidence.

During the first half of 1926, Heisenberg's reaction to Schrödinger's wave mechanics had been extremely negative. After having attended Schrödinger's conference in Munich, in July 1926, he wrote his impressions to Pauli: "Schrödinger throws overboard everything which is 'quantum theoretical': namely, the photoelectric effect, the Franck[–Hertz] collisions, the Stern–Gerlach effect, etc. It is not then difficult to establish a theory. However, it does not agree with experience." [53] A few months later, the conviction that wave mechanics would prove unable to account for properly "quantum theoretical" effects was expressed directly by Bohr and Heisenberg to Schrödinger, during the latter's visit to Copenhagen [54]. According to Bohr, they managed to convince Schrödinger that "... a continuity theory in the form indicated in his last paper at a number of points leads to expectations fundamentally different from those of the usual discontinuity theory." [55] Actually, Schrödinger felt both quite embarrassed by Bohr's contentions and not fully convinced. In his letter to Bohr, written a few weeks after his stay in Copenhagen, he acknowledged "... the psychological effect of these objections – in particular the numerous specific cases in which for the present my views apparently can hardly be reconciled with experience – is probably even greater for me than for you." [56] However, he did not believe that this incompatibility was something that hindered the very possibility of using continuous pictures: "I do not consider it inconceivable to construct pictures that actually reproduce the above circumstances." [57] He even suspected that the difficulties which Bohr had indicated were really no more than *apparent*, and he did not therefore renounce finding a clue to reconcile pure wave mechanics with the most striking discontinuous aspects of atomic processes.

One of his priorities in the following years was then to formulate wave-mechanical accounts of all the known "quantum theoretical" effects. The task did not prove intractable.

In 1927, he provided a wave-mechanical demonstration of Planck's radiation law [58]. And he insisted at the end of paragraph 4 of his paper that this result was obtained *without* using the postulate of quanta properly speaking.

He also gave during the same year a wave-mechanical account of the Compton effect [59], by considering it as a phenomenon of diffraction of high frequency electromagnetic waves on a moving grating of electronic charge distribution. The resulting directions of propagation, together with the Doppler effect, proved equivalent to the values Compton was able to predict previously by using a corpuscular model. True, Schrödinger's calculation looks highly unsatisfactory by present days standards due to his reluctance to use $3n$-dimensional Ψ-functions for composite systems, and to his recurrent taste for 3-dimensional waves and charge density clouds. But at any rate, by providing a semi-classical wave alternative to Compton's original semi-classical calculation in terms of particles, Schrödinger had demonstrated that one has no reason to consider the Compton effect as a convincing *proof* that corpuscularian representations cannot be dispensed with at one stage or another of the account of microscopic phenomena.

The teaching of this series of papers is unambiguous: it is perfectly possible to account for typically "quantum theoretical" effects without introducing any *intermediate* temporal discontinuity. Two strategies are available to reach this aim. The first one is approximate and consists in using a model of interacting 3-dimensional waves. The second one, illustrated by Mott's calculation, is exact. According to it, one merely has to use extensively the multidimensional wave-mechanical formalism (with its eigenfunction scheme) *for a sufficiently large system*, and to restrict the probabilistic scheme to the connection between the *final* outcome of the calculation and the relevant experimental events. At no intermediate point between the preparation of the experiment and the experimental events do discontinuities and probabilistic considerations have to be introduced. Moreover, since the criteria which must be used to stop the time-development of the Ψ-function are purely practical, nothing prevents one from prolonging it indefinitely, and from taking into account more and more degrees of freedom. In other terms, the temporal discontinuities, namely wave-packet reductions, are by no means an integral part of the predictive power of quantum mechanics; *they may be* related *to the formalism, whenever it is suitable in practice* [60].

Schrödinger did not work out this latter idea in its full generality before the late forties and the beginning of the fifties. But as soon as he came to realize its perfect coherence and soundness, he resumed very actively his early attempt at giving a wave-mechanical account of the "quantum theoretical" effects. He focused his attention once more on Planck's radiation law, which he managed to demonstrate for his Dublin seminar lectures of 1949, in a section characteristically entitled "Planck-black-body-radiation (without discontinuity!)" [61]. At the end of the sub-section about the Bose–Einstein statistics, he pointed out that his derivation does not rely either on the idea that each system is always in an eigenstate of some observable, or on the related idea that systems jump from one eigenstate to another: "...on (the ordinary) photon view one implicitly admits that not only the whole body of radiation but every simple 'oscillator' (or proper mode) is always in a state of sharp energy. We have assumed nothing of the kind." *Atomicity*, namely discreteness of the level scheme of continuous wave processes, replaced *atomism*, namely discontinuity of the processes and entities themselves. Accordingly, in the 1952 edition of his book *Statistical Thermodynamics* (whose first edition dates back to 1944), Schrödinger inserted an important "Appendix" where he purported to demonstrate that "the thermodynamical functions depend on the quantum-mechanical level-scheme, not on the gratuitous allegation that these levels are the only allowed states," and that they do not depend either on the view that "...a physical process consists of continual jump-like transfers of energy parcels between microsystems." [62]

Now, are these results proper to Schrödinger's version of wave mechanics? Couldn't they be obtained by means of another version of quantum mechanics, such as Heisenberg's matrix mechanics [63]? Actually, this is not impossible. As Mara Beller cogently pointed out, it was not only wave mechanics, but *also* matrix mechanics which "undermined the fundamental role of *a priori* stationary states and 'irreducible' quantum jumps." [64] Matrix mechanics, as wave mechanics, incorporates a level scheme, not the necessity of considering that microscopic objects jump from one level to another. Quantum mechanics *in general*, not only wave mechanics, is alien to the concept of "quantum jump." If the "quantum jumps" were really indispensable in order to derive the Planck's radiation law, no *interpretation* of quantum mechanics could be sufficient *as such* to perform this derivation. One would not only have to *interpret* the formalism, but to *add* something to it, namely the idea that "quantum jumps" *really occur*. It is thus on this metaphysical issue of the "reality" of quantum jumps between two observations that the debate actually centered. According to Mara Beller, "...the whole controversy gains intelligibility only when we assume that not only Schrödinger but also Heisenberg (sincere or not) had some very strong opinions about the way unobservable processes *really* occur in nature." [65] Now, on this ontological issue, I believe Schrödinger's position was much more consistent than that of his opponents. Schrödinger could argue that, since the "quantum jumps" are not necessary in order to predict any observable effect, and since they are not even an integral component of the quantum mechanical formalism, they can be dispensed with by virtue of the rule of Ockham's razor. His own strategy of ontologizing the entities of the most economical (and at the same time adequate) physical theory, could by no means lead him to ontologize the "quantum jumps" (or to endow them with "reality"), for the said quantum jumps are just an additional "convenient metaphor" [66] serving to illustrate the level-scheme of quantum mechanics. Schrödinger's ontological elimination of "really occurring quantum jumps" thus reflects a sound version of the principle of economy of thought. His asserting the unreality of quantum jumps was not grounded on bare beliefs, but on a certain set of consciously manipulated criteria allowing one to endow certain theoretical entities with "reality" and to avoid postulating unnecessary levels of "reality."

By contrast, Heisenberg's insistence in 1927 on the essential character of the discontinuities for the theoretical description of fluctuation phenomena [67] had much weaker justifications. Firstly, as I have already noticed in previous paragraphs, it proved quite easy for Schrödinger to demonstrate that these discontinuities can perfectly be dispensed with, even when one has to account for the fluctuations of energy between two interacting atoms [68]. Secondly, as Schrödinger pointed out somewhat ironically, Heisenberg's underlying idea that systems occupy one level and then jump to another level (or that they undergo temporal discontinuities, from one eigenstate of the Hamiltonian to another), is definitely inconsistent with both the structure of quantum mechanics and the epistemological decision to limit physics to the description of "observable facts." It is "irreconciliable with the very foundations of quantum mechanics" [69], for it jettisons the principle of superposition which indicates that there are available states which are *not* eigenstates. And it is inconsistent with the decision to limit physics to the description of "observable facts" because it surreptitiously tells something about what the systems "really" do when no observing subject and no measuring apparatus interfere with them: "(The assumption that each gas-molecule is always in one of its stationary states) is in violation of that precious principle that the same school of physicists is so anxious to put across, namely that we must never admit anything to be except what we have measured." [70]

Conclusion

To recapitulate, Schrödinger had many precise reasons not to believe in particles and quantum jumps. But I think that his major reason is much more general than those I have enumerated. According to him, the view that quantum mechanics is a theory of motion applying to microscopic particles which just happen to have blurred properties and to undergo discontinuous transitions, was much too *cheap* and too *modest*, as a conception of the quantum revolution. He thus proposed nothing less than a complete change of the ontological furniture of the world in addition to the change of its laws.

Compared with this attitude, the current descriptions of the community of physicists in terms of particles look quite contrived and conservative. One may wonder why they do go on talking of entities endowed with such strange features as the so-called "particles" (i.e. being somehow ubiquitous, having no spatial or kinematical *properties* but only *observables*, adopting an extended wavy behavior besides their assumed localization, being created and annihilated, etc.). The reason of this persistence is likely to be exactly the converse of Schrödinger's strategy. Schrödinger was seeking continuity in the epistemological standards, at the cost of radical discontinuity in concepts. But the community of the physicists preferred to maintain a continuity in concepts by constantly referring to familiar entities such as the "particles," at the cost of altering their epistemological standards.

There might also be a difference in metaphysical orientations underpinning this difference in epistemological attitudes. Indeed, it is crucial to metaphysical realism to secure a certain historical stability for ontology. Any excess of instability would indeed trigger doubt as to whether science can ever reach (or asymptotically approach) a state where it can be said to represent faithfully the external world. As a result, a metaphysically realist philosophy of science is bound to *require* ontological stability. It is part of its culture, of its basic stance [71], to *impose* an unchanging set of entities even when the theoretical landscape has been turned upside down. By contrast, a strongly non-realist metaphysics such as Schrödinger's, articulated with a methodological brand of scientific realism, can be extremely flexible about the type of objects physics is concerned with.

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just as much or as little ‘reality’ as the very idea of individual particles. In both cases we are concerned with a demand of causality complementary to the space-time description ....”

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6

Aspects of Nonlocality in Quantum Mechanics

Abner Shimony

6.1 Introduction

In this chapter, the formalism of quantum mechanics is briefly reviewed in order to show that entanglement of quantum states of composite systems is a consequence of the superposition principle. A new version of the classic argument of Einstein–Podolsky–Rosen is presented, inferring from relativistic locality plus a moderate sufficient condition for the existence of elements of physical reality that the quantum mechanical description of certain physical systems is incomplete. A version of Bell's Theorem is presented, showing that any hidden variables theory that satisfies certain conditions of locality makes experimental predictions inconsistent with those of quantum mechanics. A brief review is given of relevant experiments, which strongly suggest that nature behaves quantum mechanically and nonlocally. Two loopholes in most experiments are discussed, and attempts to block the loopholes are discussed. Different senses of nonlocality suggested by Bell's Theorem and the experiments are distinguished. A deepening of current physics is needed in order to explain the nonlocality discovered on a phenomenological level, and the conjecture is stated that noncommutative geometry may be an essential ingredient in the new physics.

6.2 The Superposition Principle and Entangled States

Each physical system is associated with a vector space **H**, with vectors $|\phi\rangle$, $|\chi\rangle$, etc., and scalars c, d, etc., that are in general complex numbers. *Each non-null vector represents a state of the system.* If $|\phi\rangle$ belongs to **H** so does $c|\phi\rangle$, and if $c|\phi\rangle$ is non-null it represents a state, which is the same as that represented by $|\phi\rangle$. If $|\phi\rangle$ and $|\chi\rangle$ belong to **H**, then

$c|\phi\rangle + d|\chi\rangle$ also belongs to **H** and hence represents a state of the system if it is non-null. This is the mathematical statement of *the superposition principle.*

H is also endowed with an *inner product*, which is the assignment of a complex number to each ordered pair of vectors, designated by $\langle\phi|\chi\rangle$. It is convenient to work mostly with *unit vectors*: $\langle\phi|\phi\rangle = 1$.

Physical significance of the superposition principle: if $|a_i\rangle$ is a unit vector representing a state in which a physical quantity A of the system has the value a_i (and assume that $a_i \neq a_j$ if $i \neq j$), then $|\Psi\rangle = \sum_i c_i|a_i\rangle$ represents a state in which *A has no definite value* if more than one of the c_i is non-zero. Let the state vector be normalized so that $\sum_i |c_i|^2 = 1$. Then, furthermore, *if the quantity A is measured, the probability that the outcome will be a_k is $|c_k|^2$.*

This formula is the primary linkage of the formalism of quantum mechanics to experimental phenomena. Furthermore, it is philosophically significant: *if the quantum mechanical description of a physical system is complete – i.e., says everything to be said about it – then the quantity A is objectively indefinite in the state $|\Psi\rangle$ and the probabilities $|c_k|^2$ are objective probabilities.* Heisenberg summarizes these features as "potentiality" [1].

A composite system S consisting of two subsystems S' and S'', respectively associated with vector spaces $\mathbf{H}'$ and $\mathbf{H}''$, is associated with a product vector space $\mathbf{H} = \mathbf{H}' \otimes \mathbf{H}''$. Among the vectors in **H** are product vectors of the form $|\phi\rangle|\chi\rangle$, where $|\phi\rangle$ belongs to $\mathbf{H}'$ and $|\chi\rangle$ belongs to $\mathbf{H}''$. But the superposition principle implies that there are also vectors in **H** of the form

$$|\Psi\rangle = c_1|\phi_1\rangle|\chi_1\rangle + c_2|\phi_2\rangle|\chi_2\rangle. \qquad (6.1)$$

For convenience, suppose that all the vectors on the right hand side of (6.1) are unit vectors, that neither c_1 nor c_2 is zero and that $|c_1|^2 + |c_2|^2 = 1$, and finally that the inner products $\langle\phi_1|\phi_2\rangle$ and $\langle\chi_1|\chi_2\rangle$ are both 0. Then it is easy to show that there is no way in which $|\Psi\rangle$ can be written as

$$|\Psi\rangle = |\rho\rangle|\sigma\rangle,$$

with $|\rho\rangle$ belonging to $\mathbf{H}'$ and $|\sigma\rangle$ to $\mathbf{H}''$. Schrödinger used *the term "entangled" to characterize a state of a composite system that cannot be expressed in any way as product vector* [2]. If the quantum state is a complete description of the physical system, then an entangled state is indeed strange: *it is a complete description of the composite system, but neither of the two subsystems is in a definite complete state. The* *composite system has a holistic character, not specifiable by saying all that there is to say about each subsystem S' and S'' separately.*

Not surprisingly, an entangled state ensures correlations between certain properties of S' and S''. Specifically, if a property A of S' has definite values a_1 and a_2 in states $|\phi_1\rangle$ and $|\phi_2\rangle$ respectively, while property B of S'' has definite values b_1 and b_2 in states $|\chi_1\rangle$ and $|\chi_2\rangle$ respectively, then the probability of obtaining joint results a_1 and b_1 is $|c_1|^2$, the probability of obtaining joint results a_2 and b_2 is $|c_2|^2$, and the probabilities of obtaining any other joint results is zero. Furthermore, there is *conditional probability* unity of obtaining b_1 if the result of measuring A is a_1, *conditional probability* unity of obtaining b_2 if the result of measuring A is a_2, etc. If the systems S' and S'' are well separated when the measurements are made on them, *these correlations implied by entanglement have the appearance of nonlocality.* This appearance will be investigated below in Sects. 6.4 and 6.6.

6.3 Einstein–Podolsky–Rosen (EPR)

The famous argument of EPR (1935) aimed at showing that the quantum mechanical description of a physical system is incomplete [3], and if this conclusion is correct it incidentally exorcises some of the strangeness of entanglement. This argument will be reformulated to make its logic explicit [4].

First, consider an entangled state that can be expressed in two different ways:

$$\begin{aligned} |\Psi\rangle &= (1/\sqrt{2})[|a_1\rangle|b_1\rangle + |a_2\rangle|b_2\rangle] \\ &= (1/\sqrt{2})[|f_1\rangle|g_1\rangle + |f_2\rangle|g_2\rangle]. \end{aligned} \tag{6.2}$$

Here a_1 and a_2 are distinct values of a property A of S', f_1 and f_2 are distinct values of a property F of S', b_1 and b_2 are distinct values of a property B of S'', and g_1 and g_2 are distinct values of G of S'', where

$$|f_1\rangle = (1/\sqrt{2})[|a_1\rangle + |a_2\rangle], \tag{6.3a}$$

$$|f_2\rangle = (1/\sqrt{2})[-|a_1\rangle + |a_2\rangle], \tag{6.3b}$$

$$|g_1\rangle = (1/\sqrt{2})[|b_1\rangle + |b_2\rangle], \tag{6.3c}$$

$$|g_2\rangle = (1/\sqrt{2})[-|b_1\rangle + |b_2\rangle]. \tag{6.3d}$$

Clearly, F has an indefinite value when A is definite and conversely; and similarly concerning the properties B and G of S''.

Now consider an immense ensemble ϵ of composite systems, each in the state of (6.2), and for each composite system let a random choice be made in region $\mathcal{R}$ to measure either A or F and a random choice be made in region $\mathcal{L}$ to measure either B or G, with probability 1/4 for each of the four choices (A, B), (A, G), (F, B), (F, G). The two regions $\mathcal{R}$ and $\mathcal{L}$ have spacelike separation. (The letters $\mathcal{R}$ and $\mathcal{L}$ should be equipped with indices corresponding to the member of the ensemble being examined, but these cumbersome indices will be suppressed.) ϵ is thus partitioned into four mutually exclusive and exhaustive subensembles: ϵ_1 (in which A and B are measured), ϵ_2 (in which A and G are measured), ϵ_3 (in which F and B are measured), and ϵ_4 (in which F and G are measured), and of course with very high probability each of the four subensembles is immense.

EPR postulated a sufficient condition for the existence of an element of physical reality: "if, without in any way disturbing a system, we can predict with certainty ... the value of a physical quantity, then there exists an element of physical reality corresponding to this quantity." [5] For a reason to become apparent later, I prefer to substitute "infer" for "predict". Consider a composite system in the subensemble ϵ_1. An observer in $\mathcal{R}$ can note the outcome of the measurement of A and, in view of the first part of (6.2) and the interpretation of the entangled state given earlier, can infer with certainty the outcome of the measurement of B in $\mathcal{L}$. For example, if A has outcome a_i, then it is inferred that B has outcome b_i, where i is 1 or 2. (The inference is not a prediction because $\mathcal{R}$ and $\mathcal{L}$ have space-like separation, and therefore there is no relativistically invariant temporal ordering of an event in $\mathcal{R}$ and one in $\mathcal{L}$.) Because of the space-like separation of $\mathcal{R}$ and $\mathcal{L}$, an event in the former region, such as the outcome of the measurement of A, cannot disturb the system in $\mathcal{L}$ whose property B is measured. Consequently the sufficient condition of EPR is satisfied, and therefore there exists an element of physical reality in the system in $\mathcal{L}$ corresponding to property B, presumably in the backward light-cone of the measurement of B and determining its outcome. This element of physical reality cannot be attributed to the random choice made in $\mathcal{L}$ to measure B, since that choice cannot, without violating relativistic locality, implant the element correctly correlated with the outcome of measuring A in region $\mathcal{R}$. Symmetrically, there is an element of physical reality in $\mathcal{R}$ determining the outcome of A, and it cannot be attributed to the random choice to measure A.

So far, elements of physical reality corresponding to A and B have been established, using EPR's condition, only for subensemble ϵ_1. But since a random choice was made to measure A and B in the composite system under consideration and thereby to place it in this subensemble, ordinary inductive reasoning (or a refined Bayesian development thereof) leads to the conclusion with overwhelming probability that any arbitrary member of the entire ensemble ϵ possesses elements of physical reality in regions $\mathcal{R}$ and $\mathcal{L}$ which are exactly such as to determine outcomes of measurements of A and B if those are chosen to be measured in region $\mathcal{R}$ and region $\mathcal{L}$ respectively.

A parallel argument starting with subensemble ϵ_4 leads to the conclusion that elements of physical reality determining the outcomes of F and G, if those are chosen to be measured, are present in regions $\mathcal{R}$ and $\mathcal{L}$ for every composite system in the entire ensemble ϵ.

It follows that in the entire ensemble there are definite values assigned to both A and F in region $\mathcal{R}$, despite the quantum mechanical incompatibility of these two properties, and likewise for both B and G in region $\mathcal{L}$. Hence, from EPR's premises, we reach their conclusion that the quantum mechanical description of physical systems is incomplete.

EPR's conclusion is intended to remove the taint of nonlocality from entanglement: the quantum mechanical correlations are due to a detailed preparation (performed in the intersection of the backward light-cones of $\mathcal{R}$ and $\mathcal{L}$) of each system in the entire ensemble ϵ, such that the elements of physical reality implanted in S' and S'' ensure strict correlation of A and B, if they are measured, and of F and G, if they are measured.

Notes: (I) EPR's conclusion was reached here by a relativistic analysis of the two subsystems. This is not explicitly done in EPR's original paper of 1935, though it is in the spirit of that paper.

(II) No counterfactual reasoning was used in order to extend the inference of elements of physical reality from a subensemble to the entire ensemble, as has been claimed in some recent articles [6], but instead inductive reasoning was used for this purpose.

(III) Once the existence of EPR's "elements of physical reality" is established for the entire ensemble, there is a basis for counterfactual reasoning – i.e., what would have happened if properties, different from those actually measured, had been measured.

6.4 A Generalized Version of Bell's Theorem

Bell's original theorem of 1964 proved that the elements of physical reality deduced by EPR in situations governed by certain quantum mechanical entangled states $|\Psi\rangle$, combined with a locality assumption, are inconsistent with some of the quantum mechanical predictions of $|\Psi\rangle$ [7]. It will be useful to present a more general theorem of which Bell's original one is a corollary [8].

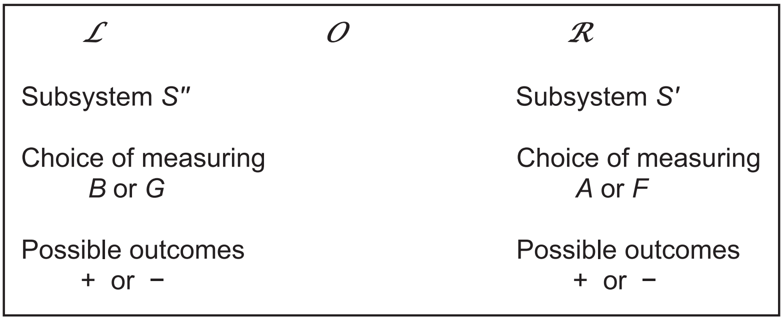

Fig. 6.1. The experimental arrangement of the relativistic EPR argument. λ is a complete state of $S' + S''$ at $\mathcal{O}$ at $t = 0$. Measurements are subsequently made on subsystems S' and S'' in regions $\mathcal{R}$ and $\mathcal{L}$ respectively.

Consider the experimental arrangement shown in Fig. 6.1. Regions $\mathcal{R}$ and $\mathcal{L}$ of space-like separation are schematically indicated, and a composite system governed by $|\Psi\rangle$ consists of a pair of particles S' and S'' emitted jointly from a point O in the intersection of the backward light-cones of $\mathcal{R}$ and $\mathcal{L}$. The complete state of the composite system $S = S' + S''$ is λ (belonging to a space of complete states $\mathbf{\Lambda}$). A probability distribution ρ over $\mathbf{\Lambda}$, with integral unity over $\mathbf{\Lambda}$, is assumed to be determined by the physical circumstances of the source of pairs of particles. A random choice is made (in $\mathcal{R}$ and $\mathcal{L}$ respectively) to measure either A or F in region $\mathcal{R}$, and either B or G in region $\mathcal{L}$. The only possible outcomes of these measurements – that is, the a_1 and a_2, the b_1 and b_2, f_1 and f_2, g_1 and g_2 – are $+1$ and -1. Fig. 1 indicates the possible choices and outcomes. For each complete state λ the following conditional probabilities are assumed to be well defined:

$$P_{\mathcal{R}}(A = +1|\lambda, B), \qquad P_{\mathcal{R}}(A = +1|\lambda, G), \tag{6.4a}$$

$$P_{\mathcal{R}}(A = +1|\lambda, B = +1), \quad P_{\mathcal{R}}(A = +1|\lambda, G = +1), \tag{6.4b}$$

and all other similar conditional probabilities of outcomes in region $\mathcal{R}$ obtained by substituting one or both occurrences of $+1$ by -1. (A word about the notation: $P_{\mathcal{R}}(A = +1|\lambda, B)$ denotes the conditional probability of obtaining the result $A = +1$ in a measurement made in region $\mathcal{R}$, given that the system is in state λ and that B has been measured in region $\mathcal{L}$. $P_{\mathcal{R}}(A = +1|\lambda, B = +1)$ is the conditional probability of obtaining the result $A = +1$ in a measurement made in region $\mathcal{R}$, given that the system is in state λ and that the result $B = +1$ has been obtained in a measurement made in region $\mathcal{L}$.) Similarly, there are well-defined conditional probabilities concerning outcomes in region $\mathcal{L}$:

$$P_{\mathcal{L}}(B = +1|\lambda, A), \qquad P_{\mathcal{L}}(B = +1|\lambda, F), \tag{6.5a}$$

$$P_{\mathcal{L}}(B = +1|\lambda, A = +1), \quad P_{\mathcal{L}}(B = +1|\lambda, F = +1), \tag{6.5b}$$

and likewise the conditional probabilities of outcomes in region $\mathcal{L}$ obtained by substituting one or both occurrences of $+1$ by -1. All of these probabilities are conditional on λ; those in (6.4a) and (6.5a) are also conditional upon which measurement is performed in the other region; those in (6.4b) and (6.5b) are conditional upon the outcome of a specified measurement in the other region.

Now make the following locality assumptions, which are suggested by the space-like separation of $\mathcal{R}$ and $\mathcal{L}$:

I. *Parameter Independence* (regarding the choice of measurement in the farther region as a parameter):

$$P_{\mathcal{R}}(A = +1|\lambda, B) = P_{\mathcal{R}}(A = +1|\lambda, G) \equiv P_{\mathcal{R}}(A = +1|\lambda), \tag{6.6}$$

and similar equalities. The idea is that the probability of an outcome of a measurement in one region is independent of which measurement is made in the other region, once the complete state of the composite system at birth is specified.

II. *Outcome Independence*:

$$\begin{aligned} P_{\mathcal{R}}(A = +1|\lambda, B = +1) &= P_{\mathcal{R}}(A = +1|\lambda, B = -1) \\ &= P_{\mathcal{R}}(A = +1|\lambda, B), \end{aligned} \tag{6.7}$$

and similar equalities. The idea is that the probability of an outcome of a measurement in one region is independent of the outcome of a measurement made in the other region, though of course dependent upon λ and possibly upon the choice of the quantity measured in the farther region.

It follows from (6.6) and (6.7) and their variants that

$$P(A = +1, B = +1|\lambda) = P_{\mathcal{R}}(A = +1|\lambda)P_{\mathcal{L}}(B = +1|\lambda), \tag{6.8}$$

where the two factors on the right hand side are probabilities conditional upon the complete state λ but independent of what measurement is made in the farther region and what its outcome is; and of course other similar equalities hold. Equation (6.8) is essentially Bell's locality condition in his paper of 1971, but the explicit recognition of Parameter Independence (6.6), and Outcome Independence (6.7), and the proof that the conjunction of these two independence conditions are equivalent to Bell's locality condition is due to J. Jarrett [9].

An immediate corollary of the factorization of probabilities in (6.8) is a factorization of the expectation value of the product of quantities AB:

$$\exp(AB|\lambda) = \exp_{\mathcal{R}}(A|\lambda)\exp_{\mathcal{L}}(B|\lambda), \tag{6.9}$$

and similar factorizations for the expectation values of the products AG, FB, and FG conditional on λ.

Because the only outcomes of measurements of A, B, F and G are $+1$ and -1,

$$1 \geq \exp_{\mathcal{R}}(A|\lambda) \geq -1, \tag{6.10}$$

and likewise for the expectation values of B, F, and G. It is a straightforward algebraic exercise to derive from (6.9) and (6.10) that

$$2 \geq \exp(AB|\lambda) + \exp(AG|\lambda) + \exp(FB|\lambda) - \exp(FG|\lambda) \geq -2. \tag{6.11}$$

If we now integrate (6.11) over the space $\mathbf{\Lambda}$ of complete states, using the probability distribution ρ (which was assumed to integrate to unity over $\mathbf{\Lambda}$) and use the notation

$$\exp(AB) \equiv \int_{\mathbf{\Lambda}} \exp(AB|\lambda)d\rho, \tag{6.12}$$

etc., we obtain an inequality governing the experimental expectation values of the four products of quantities AB, AG, FB, and FG:

$$2 \geq \exp(AB) + \exp(AG) + \exp(FB) - \exp(FG) \geq -2. \tag{6.13}$$

Inequality (6.13) is the generalized version of Bell's Inequality that was promised. No use has been made of quantum mechanics in the derivation of the generalized Bell's Inequality, but we can compare the predictions of quantum mechanics with that Inequality. Let the composite system be in the state of (6.2) – that is, let

$$\begin{aligned}|\Psi\rangle &= (1/\sqrt{2})[|a_1\rangle|b_1\rangle + |a_2\rangle|b_2\rangle] \\ &= (1/\sqrt{2})[|f_1\rangle|g_1\rangle + |f_2\rangle|g_2\rangle],\end{aligned}$$

where a_1 and a_2 are distinct values of a property A of S', f_1 and f_2 are distinct values of a property F of S', etc., and where the relations given by (6.3a, 6.3b, 6.3c, 6.3d) hold among the vectors $|a_i\rangle, |b_i\rangle, |f_i\rangle, |g_i\rangle$. Then $\exp_\Psi(AB)$ is the quantum mechanical expectation value of the product AB in that state, and it is straightforward to calculate that

$$\exp_\Psi(AB) + \exp_\Psi(AG) + \exp_\Psi(BF) - \exp_\Psi(BG) = 2\sqrt{2}. \quad (6.14)$$

There is obviously a conflict between the generalized Bell's Inequality (6.13) and the quantum mechanical prediction (6.14). This conflict is a "generalized Bell's Theorem."

6.5 Experimental Tests of Bell's Inequality

Several dozen tests of the generalized Bell's Inequality have been performed in circumstances in which the quantum predictions for a nearly ideally performed experiment would violate the Inequality. Nearly all tests have been performed with pairs of photons, and that fact provides an opportunity to acknowledge a special debt to Max Planck: without the granularity of light it is hard to see how one could carry out optical tests of Bell's Inequality. The first test was reported by Freedman and Clauser [10] in 1972. Their experiment used pairs of photons produced in a cascade in excited calcium from an initial zero angular momentum state to a final zero angular momentum state, and it agreed with quantum mechanics. Almost all subsequent experiments have agreed with the quantum predictions and have exceeded the limits of Bell's Inequality by many times the experimental error, in some cases by a factor of the order of 100. In the first experiment that did not violate Bell's Inequality, that of Holt and Pipkin [11] there was suspicion of a systematic error, and when their experimental arrangement was repeated [12] with care to avoid the suspected systematic error, agreement with quantum mechanics and violation of Bell's Inequality were obtained.

Nevertheless, there are possible loopholes in the experiments so far. One is the "communication loophole," that the region $\mathcal{R}$, in which a choice is made between measuring A and F and then completing the measurement, and the region $\mathcal{L}$, in which a choice is made between measuring B and G and then completing that measurement, do not have space-like separation. If so, it is imaginable that a subluminal communication takes place between $\mathcal{R}$ and $\mathcal{L}$ to effect the correlations predicted by quantum mechanics, and hence no violation of relativistic locality is entailed by the violation of Bell's Inequality. I know of three experiments in which ingenious efforts were made to block this loophole by making a sufficiently rapid choice between A and F and between B and G. The first was performed by Aspect, Dalibard, and Roger [13], but had the shortcoming that the choices on each side were made by periodically varying processes – and the clever hidden variables could conceivably become aware of the periodicity and adjust their choices accordingly. More recently Tittel, Brendel, Zbinden, and Gisin of Geneva performed an experiment in which the choices were governed not by a periodic device but by a beam splitter, which is a quantum mechanical randomizer [14]. The two photons that constitute the composite system are analyzed and detected at loci 10.9 km apart, showing the quantum mechanical entanglement endures despite great spatial separation! Weihs, Jennewein, Simon, Weinfurter, and Zeilinger of Innsbruck performed an experiment in which each of the two choices between quantities to be measured was made by a pseudo-random-number-generator and the measurements were completed with ultrafast circuitry; the two photons of the composite system were analyzed and measured 400 m apart [15]. In all three of the experiments just noted, the results agreed with the quantum mechanical predictions and violated Bell's Inequality.

The "detection loophole," however, has not yet been blocked in any experiment. It stems from the fact that the inefficiency of the detectors entails that only a fraction of the ensemble of interest is detected. Consequently, it is logically possible that Bell's Inequality is obeyed in the ensemble, but the selection from the ensemble by inefficient detectors (whose selection procedure is conceivably governed by clever hidden variables) would produce data in agreement with the quantum mechanical predictions and in disagreement with the Inequality. Explicit models by Clauser and Horne [16] and by Maudlin [17] show that this implausible scenario is logically possible, provided that the detectors are sufficiently inefficient. However, there are calculations by Mermin and others [18] showing that this scenario is not logically consistent if the individual detectors have efficiency of more than 82.8%. But photons in the experimentally feasible frequency range can at present be detected with no more than 50% efficiency, far below the threshold calculated by Mermin et al. Almost all the Bell tests so far have been performed with photons, because of the relative ease of producing strongly entangled pairs of them, but there is no barrier in principle to using other pairs of particles.

A promising experiment is now in process. Fry and Walther [19] are far advanced in a test of Bell's Inequality using correlated atoms produced by the dissociation of dimers. After dissociation of the dimer each atom has total angular momentum (including the contribution from the nuclear spin) of $F = 1/2$, but the angular momenta of the two atoms in any given direction are strongly correlated (i.e., the state of the pair of separated atoms is quantum mechanically entangled). The quantities A and F measured for one of the atoms are the components of angular momentum F_n along two different choices of direction n, and quantities B and G measured for the other atom are F_m along two choices of direction m. In each case, the measurement consists of selective ionization of the atom by a laser beam of precisely selected frequency – i.e., ionization if $F_n = +1/2$ and no ionization if $F_n = -1/2$. Since high efficiency is achievable in the detection of ions, the threshold calculated by Mermin et al. for blocking the detection loophole should be surpassed. The experiment is expected to be completed soon, and will be recognized as a classic experiment, whether the results agree with the quantum mechanical predictions or with Bell's Inequality. Fry [20] has also considered the possibility of combining procedures in a single experiment which would block both the communication and the detection loopholes, but that experiment is remote.

6.6 What Kind of Nonlocality is Implied by a Violation of Bell's Inequality?

Recall that Bell's Inequality was derived from Bell's locality condition – our (6.8), also called the "factorization condition":

$$P(A = +1, B = +1|\lambda) = P_{\mathcal{R}}(A = +1|\lambda)P_{\mathcal{L}}(B = +1|\lambda), \text{ etc.},$$

and that was shown by Jarrett to be equivalent to the conjunction of Parameter Independence and Outcome Independence:

I. *Parameter Independence* (treating the choice of measurement in the farther region as a parameter):

$$P_{\mathcal{R}}(A = +1|\lambda, B) = P_{\mathcal{R}}(A = +1|\lambda, G) = P_{\mathcal{R}}(A = +1|\lambda),$$

as expressed in (6.6), and similar equalities. The idea is that the probability of an outcome of a measurement in one region is independent of which measurement is made in the other region, once the complete state of the composite system at birth is specified.

II. *Outcome Independence*:

$$P_{\mathcal{R}}(A=+1|\lambda, B=+1) = P_{\mathcal{R}}(A=+1|\lambda, B=-1) \\ = P_{\mathcal{R}}(A=+1|\lambda, B), \quad (6.15)$$

as in (6.7), and similar inequalities. The idea is that the probability of an outcome of a measurement on one side of the experiment is independent of the outcome of the measurement on the other side, once the complete state λ is specified.

Another premise used in the proof is the independence of the probability distribution ρ from the choice of the measured quantities.

The violation of the generalized Bell's Inequality implies the violation of one of these three independence conditions. What is the consequence of each of these possible violations?

Violation of Parameter Independence: a bit of information can, with probability as close to unity as desired, be transmitted between regions $\mathcal{R}$ and $\mathcal{L}$ (hence superluminally) by using an ensemble of composite systems all in the same complete state λ, each of the systems S' in the same region $\mathcal{R}$ and each of S'' in the same region $\mathcal{L}$ and making the same choice between A and F for each of the S'. This superluminal transmission of information would be a clear violation of Special Relativity. It should be noted, however, that quantum mechanics does not violate Parameter Independence [21].

Violation of Independence from Choice of Quantities Measured: Since the space-time region $\mathcal{O}$ in which a number of replicas of $S' + S''$ are generated is in the backward light-cones of both $\mathcal{R}$ and $\mathcal{L}$, this violation would imply causal influence into the past, which is a highly undesirable kind of nonlocality.

Quantum mechanics obviously violates Outcome Independence, as shown by the correlations discussed in Sect. 6.4. But this violation does not permit the superluminal transmission of information, essentially because the *outcome* of measurement of A in region $\mathcal{R}$ is not under the experimenter's control, even though the *choice* of measuring A rather than F is controlled by the experimenter. Nevertheless, the violation of Outcome Independence is contrary to the spirit of Special Relativity and in tension with it. One cannot properly say that the first of the measurements made in $\mathcal{R}$ and $\mathcal{L}$ influences the probability distribution of the outcome of the second, where this influence is a kind of causation unprecedented in classical physics, because the temporal ordering of the two events is not relativistically invariant when $\mathcal{R}$ and $\mathcal{L}$ have space-like separation. In the special case (considered in our discussion of EPR), where there is perfect correlation of the outcomes of A and B and of

F and G, there seems to be a kind of causation and yet no invariant assignment of "cause" to one event and "effect" to the other. One is tempted to say that the classical concepts of event and causation have to be modified: that the outcome in $\mathcal{R}$ and the outcome in $\mathcal{L}$ constitute a single inseparable non-localized event. Then the space-time structure of Special Relativity would be salvaged, but at the price of generalizing the concept of event. This strategy is suspect, however. Do we know what is meant by this generalization of "event"? And is the conceptual tension with Special Relativity theory resolved?

6.7 The Need for a Deeper Physics

The violation of Bell's Inequality is not the only locus of tension between quantum mechanics and space-time theory. Other sources of tension are the difficulty of quantizing general relativity and the difficulty of maintaining the very concept of a space–time continuum at the Planck level (around 10^{-33} cm), where uncertainties of the metric structure undermine the meaningfulness of the metrical and even the topological structure of space–time. Consequently, many students (including myself) believe that a solution to the nonlocality problem created by Bell must be a deep solution. I believe that nonlocality is here to stay, but so far we only have a phenomenological account of it. What is needed is a deep theory underlying the phenomenology, in the way that Boltzmann's statistical account of thermodynamic processes provided the conceptual underpinning of the second law of thermodynamics.

Is there a deep theory on the horizon? Maybe. I am impressed and attracted by the non-commutative geometry of A. Connes [22] and its application to quantum mechanics and space–time theory by M. Heller [23]. Their idea is to approach differential geometry through algebra, and then generalize the approach. All information about a differentiable manifold is contained in the algebra of smooth functions on the manifold: a point in the manifold is identified with the set of smooth functions which vanish at it. The algebra of smooth functions is a commutative algebra, taking ordinary multiplication of functions as the multiplication operation of the algebra. Now suppose a non-commutative algebra is given (a familiar example being the algebra of bounded operators on a Hilbert space). Can one work backwards to a generalization of the differentiable manifold? At first it seems unpromising, since the identification of a point with a subset of the algebra breaks down. The noncommutative geometry associated with the noncommutative algebra has no points, and the concept of neighborhood is not defined. What comes closest to the concept of a point in the usual differentiable manifold is something like a state in quantum mechanics, which is intrinsically global, in that it says something about all observables. The possibility of doing something like differential geometry on such a space stems from the fact that the concept of a derivation is definable in the noncommutative algebra, satisfying linearity and Leibniz's rule.

Heller and his collaborators propose that the "pregeometry" of the universe is just such a noncommutative geometry, having no local coordinate systems that could be identified with space and time because there is nothing local, and they claim that this is just what one needs to penetrate below the Planck threshold. How then does ordinary physics, at a grosser scale than the Planck threshold, emerge? Mathematically, the emergence consists of a restriction of the noncommutative algebra to its center – the set of elements commuting with all elements of the algebra. The physical counterpart of this mathematical restriction is a phase transition, from which emerge space, time, and individuality of particles. I confess bafflement about the physical circumstances and conditions of this phase transition, which has no clear analogue to the familiar phase transitions of condensed matter physics or of elementary particle theory. Nevertheless, I find appealing the proposal that even after this phase transition there remain residues of the nonlocal pregeometry–EPR correlations, the nonlocality revealed in violations of Bell's Inequality, and the horizon problem in relativistic cosmology. They present a model in which there are two "limiting cases": to general relativity and to quantum mechanics. In the correspondence limit of quantum mechanics the noncommutative dynamics leads to the standard unitary evolution described by the Schrödinger equation. In the correspondence limit of general relativity, the noncommutative dynamics "projects down" to processes occurring in space–time. The reduction of the state vector in a measurement is an example of such a process. Thus, the approach of non-commutative geometry promises not only a deep explanation of the phenomenological nonlocality found in violations of Bell's Inequality but also a solution to the other great problem (discussed by Roland Omnès in Chap. 12 of this volume) in the foundations of quantum mechanics – the measurement problem.

The question was raised whether the speculative deep physics built upon non-commutative geometry would be any less mysterious than quantum nonlocality is at present. The answer seems to me to be no. The fundamental laws of nature are almost certain to seem strange to creatures like us, whose cognitive faculties have been shaped by evolution to deal practically with the macroscopic world. And furthermore, it is hard to see how fundamental laws of physics could be "rational" in the sense of being derivable *ex nihilo* by pure logic and mathematics [24]. Nevertheless, a deep physics which provides a satisfactory explanation of phenomenological nonlocality would be gratifying, since it would incorporate that nonlocality into a systematic world picture, instead of regarding it as an anomalous frontier of fundamental physics.

Perhaps the next centenary of Planck will celebrate his discovery of the Planck threshold in space–time as much as his discovery of quanta of radiation!

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7

Decoherence and the Foundations of Quantum Mechanics

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7.1 Introduction

Over the past quarter-century, decoherence has become an omnipresent term in the literature on quantum mechanics. Even named part of the “new orthodoxy” [1] in understanding quantum mechanics, it has attracted widespread attention among experimental and theoretical physicists as well as philosophers of physics. The burgeoning field of quantum computing [2] and research into the realization of mesoscopic and macroscopic superposition states [3] has made decoherence a more widely studied field than ever. Although decoherence per se does not introduce anything particularly new into the formalism of standard quantum mechanics, it is capable of yielding surprising results that, when properly interpreted, can contribute crucially to a proper understanding of the connection between the quantum-mechanical formalism and the world of our perception. Anyone working in the field of quantum mechanics today needs to know the basics of decoherence and its conceptual implications. This article is intended as a primer that reviews those basics [4].

Decoherence studies the ubiquitious interactions between a system and its surrounding environment. These interactions lead to a rapid and strong entanglement between the two partners that has crucial consequences for what we can observe at the level of the system. Studies have shown that even the microwave background radiation can have a significant impact on systems of sizes as small as a dust particle [5]. The decoherence program describes such environmental interactions and evaluates their formal, experimental, and conceptual consequences for the quantum-mechanical description of physical systems. In the following, we will introduce the main concepts of decoherence and discuss some of their implications for foundational aspects and interpretations of quantum mechanics.

7.2 Basics of Decoherence

The key idea promoted by decoherence is rather simple, although its consequences are far-reaching and seem to have been overlooked for a surprisingly long time: To give a correct quantum-mechanical account of the behavior and properties of a physical system, we must include the interactions of this system with its omnipresent environment, which generally involves a large number of degrees of freedom.

Classical physics typically studies systems that are thought of as being separated from their surroundings. The environment is generally viewed as a "disturbance" or "noise." In many cases, the influence of the environment is neglected, usually in accordance with the relative sizes of system and environment. For instance, the scattering of air molecules on a bowling ball is ignored when the motion of the ball is studied, while surrounding molecules have a crucial influence on the path of a small particle in Brownian motion.

By contrast, in quantum mechanics, environmental interactions amount to more than a simple delivery of "kicks" to the system. They lead to the formation of a nonlocal entangled state for the system–environment combination. Consequently, no individual quantum state can be attributed to the system anymore. Such entanglement corresponds to establishing correlations that imply properties for the system–environment combination that are not derivable from features of the individual parts themselves and that change the properties that we can "assign" to the individual system. Thus interactions between a given system and its large, ubiquitous environment, must not be neglected if the system is to be described properly in quantum-mechanical terms.

The theory of decoherence typically involves two distinct steps: a dynamical step, namely, the interaction of the system with its environment and the resulting entanglement, and a coarse-graining step in form of a restriction to observations of the system only. The latter step can be motivated by the (nontrivial) empirical insight that all observers, measuring devices, and interactions are intrinsically local [6]. In any realistic measurement performed on the system, it is practically impossible to include all degrees of freedom of the system and those of the environment that have interacted with the system at some point. In other words, inclusion of the environment is needed to arrive at a complete description of the time evolution of the system, but we subsequently "ignore" at least a part of the environment by not observing it. For example, light that scatters off a particle will influence the behavior of the particle, but we will intercept (i.e., observe) only a tiny part of the scattered photons with our visual apparatus; the rest will escape our observation. The key question that decoherence investigates can then be put as follows: What are the consequences of *nonlocal* environmental entanglement for *local* measurements?

To formalize matters, let us assume that the system $\mathcal{S}$ can be described by state vectors $|s_k\rangle$, and that the interaction with the environment $\mathcal{E}$ leads to a formation of product states of the form $|s_k\rangle \otimes |e_k(t)\rangle$, where the $|e_k(t)\rangle$ are the corresponding "relative" states of $\mathcal{E}$ (representing a typically very large number of environmental degrees of freedom). If the initial state of the system at $t = 0$ is given by the pure-state superposition $|\Psi_\mathcal{S}\rangle = \sum_k \lambda_k |s_k\rangle$, and that of the environment by $|e_0\rangle$, the initial state of the system–environment combination has the separable form

$$|\Psi\rangle = |\Psi_\mathcal{S}\rangle \otimes |e_0\rangle = \left(\sum_k \lambda_k |s_k\rangle \right) \otimes |e_0\rangle. \tag{7.1}$$

Here, the system has the well-defined individual quantum state $|\Psi_\mathcal{S}\rangle$. However, the interaction between $\mathcal{S}$ and $\mathcal{E}$ evolves $|\Psi\rangle$ into the nonseparable entangled state

$$|\Psi(t)\rangle = \sum_k \lambda_k(t) |s_k\rangle \otimes |e_k(t)\rangle. \tag{7.2}$$

In essence, the dynamical evolution $|\Psi\rangle \to |\Psi(t)\rangle$ corresponds to von Neumann's account of quantum measurement [7] that models the measurement process within unitary (no-collapse) quantum mechanics as the formation of appropriate quantum correlations between the system and the measuring apparatus (where the latter is here represented by the environment). Accordingly, decoherence was initially only referred to as "continuous measurement by the environment."

Since the state $|\Psi(t)\rangle$ in general can not be expressed anymore in a separable product form $|\Psi_\mathcal{S}(t)\rangle \otimes |\Psi_\mathcal{E}(t)\rangle$, no individual state vector can be attributed to $\mathcal{S}$. The phase relations λ_k, describing the coherent superposition of $\mathcal{S}$-states $|s_k\rangle$ in the initial state, have been "dislocalized" into the combined state $|\Psi(t)\rangle$ through the interaction; i.e., coherence has been "distributed" over the many degrees of freedom of the system–environment combination and has become unobservable at the level of the system. To paraphrase Joos and Zeh [8], the superposition still *exists* (in fact, it now even pertains to the environment), but it is not *there* (at the individual system). In this sense, we can speak of the decoherence process as describing a local suppression (or rather: inaccessibility) of interference.

Since the interaction is strictly unitary, decoherence can in principle always be reversed. However, due to the large number of degrees of freedom of the environment (that are typically not controlled and/or controllable), decoherence can be considered irreversible for all practical purposes. It also turns out that, for the same reason, the states $|e_k(t)\rangle$ rapidly approach orthogonality (i.e., macroscopic distinguishability) as t increases,

$$\langle e_k(t)|e_{k'}(t)\rangle \longrightarrow 0 \qquad \text{if } k \neq k'. \tag{7.3}$$

To see more directly the phenomenological consequences of the processes described thus far in the context of actual measurements, let us consider the density matrix $\rho_{\mathcal{SE}}(t)$ corresponding to the state $|\Psi(t)\rangle$:

$$\rho_{\mathcal{SE}}(t) = |\Psi(t)\rangle\langle\Psi(t)| = \sum_{k,k'} \lambda_k(t)\lambda^*_{k'}(t)|s_k\rangle|e_k(t)\rangle\langle s_{k'}|\langle e_{k'}(t)|. \tag{7.4}$$

(We shall from here on omit the tensor-product symbol "⊗" to simplify our notation.) The presence of terms $k \neq k'$ represents interference (quantum coherence) between different product states $|s_k\rangle|e_k(t)\rangle$ of the system–environment combination $\mathcal{SE}$. By contrast, if we dealt with a classical ensemble of these states, our density matrix would read

$$\rho^{\text{class}}_{\mathcal{SE}}(t) = \sum_k |\lambda_k(t)|^2|s_k\rangle|e_k(t)\rangle\langle s_k|\langle e_k(t)|. \tag{7.5}$$

Such an ensemble is interpreted as describing a state of affairs in which $\mathcal{SE}$ is in one of the states $|s_k\rangle|e_k(t)\rangle$ with (ignorance-based) probability $|\lambda_k(t)|^2$.

Let us now include the coarse-graining component, i.e., we assume that we do not (cannot, do not need to) have full observational access to all the many degrees of freedom of the environment interacting with the system. The restriction to the system can be represented by forming the so-called reduced density matrix, obtained by averaging over the degrees of freedom of the environment via the trace operation:

$$\begin{aligned}\rho_{\mathcal{S}}(t) &= \text{Tr}_{\mathcal{E}}(\rho_{\mathcal{SE}}(t)) \\ &= \sum_l \langle e_l|\rho_{\mathcal{SE}}(t)|e_l\rangle \\ &= \sum_{k,k'} \lambda_k(t)\lambda^*_{k'}(t)\langle e_{k'}(t)|e_k(t)\rangle|s_k\rangle\langle s_{k'}|,\end{aligned} \tag{7.6}$$

where the $\{|e_l\rangle\}$ forms a basis of the Hilbert space of $\mathcal{E}$. Density matrix $\rho_{\mathcal{S}}$ suffices to compute probabilities and expectation values for all local observables $\widehat{O}_{\mathcal{S}}$ that take into account only the degrees of freedom of $\mathcal{S}$. In this sense, it contains all the relevant information about the "state" of $\mathcal{S}$ that can be found out by measuring $\mathcal{S}$ (while, of course, no individual quantum state vector can be attributed to $\mathcal{S}$).

Now, since the decoherence process makes the environmental states $|e_k(t)\rangle$ approximately mutually orthogonal, as in (7.3), the reduced density matrix approaches the diagonal limit

$$\rho_{\mathcal{S}}(t) \longrightarrow \sum_k |\lambda_k(t)|^2 |s_k\rangle\langle s_k|. \tag{7.7}$$

Since this density matrix *looks like* that for a classical ensemble of $\mathcal{S}$-states $|s_k\rangle$ [cf. (7.5)], it is often referred to as describing an "apparent ensemble." As a consequence, the expectation value of observables $\widehat{O}_{\mathcal{S}} = \sum_{k,k'} O_{kk'} |s_k\rangle\langle s_{k'}|$ computed via the trace rule $\langle \widehat{O}_{\mathcal{S}} \rangle = \mathrm{Tr}_{\mathcal{S}} \left[\rho_{\mathcal{S}}(t) \widehat{O}_{\mathcal{S}} \right]$ approaches that of a classical average, i.e., the contribution from interference terms $k \neq k'$ becomes vanishingly small.

While the dislocalization of phases can be fully described in terms of unitarily evolving, interacting wavefunctions [see (7.2)], the reduced density matrix has been obtained by a nonunitary trace operation. The formalism and interpretation of the trace presuppose the probabilistic interpretation of the wave function and ultimately rely on the assumption of the occurence of an (if only apparent) "collapse" of the wave function at some stage. We must therefore be very careful in interpreting the precise meaning of the reduced density matrix, especially if we would like to evaluate the implications of decoherence for the measurement problem and for no-collapse interpretations of quantum mechanics. It is probably fair to say that early misconceptions in this matter have contributed to the confusion and criticism that has surrounded the decoherence program over the decades. So we will discuss this point in some detail in the next section.

7.3 Decoherence and the Measurement Problem

The measurement problem relates to the difficulty of accounting for our perception (if not the objective existence) of definite outcomes at the conclusion of a measurement. It follows from the linearity of the Schrödinger equation that when the (usually microscopic) system $\mathcal{S}$ is described by a superposition of states $|s_k\rangle$ which the (typically macroscopic) apparatus $\mathcal{A}$ (with corresponding states $|a_k\rangle$) is designed to measure, the final composite state of the system–apparatus combination $\mathcal{SA}$ will be a superposition of product states $|s_k\rangle|a_k\rangle$. This is basically the state of affairs described by (7.1) and (7.2) (representing the von Neumann-type measurement scheme), with the environment $\mathcal{E}$ now replaced by the measuring device $\mathcal{A}$.

The usual rules of quantum mechanics then imply that no single, definite state can be attributed to the apparatus, and that in general we have (1) a multitude of possible outcomes (not just one), and (2) interference between these multiple outcomes. That a superposition must not be interpreted as an ensemble has also been widely confirmed in numerous experiments, in which superpositions are observed as individual physical states where all components of the superposition are simultaneously present [9].

So how is it, then, that at the conclusion of a measurement we always observe the pointer of the apparatus to be in a single definite position, but never in a superposition of positions? This "measurement problem" actually contains of two separate questions: (A) Why is it that always a particular quantity (usually position) is selected as the determinate variable (the "preferred-basis problem")? And (B), why do we perceive a single "value" (outcome) for the determinate variable (the "problem of outcomes")? We shall discuss these questions and their connection with decoherence in the following.

7.3.1 The Preferred-Basis Problem

As a simple example for the preferred-basis problem, consider a system $\mathcal{S}$ consisting of a spin-1/2 particle, with spin states $|\uparrow_z\rangle_\mathcal{S}$ and $|\downarrow_z\rangle_\mathcal{S}$ corresponding to the eigenstates of an observable σ_z that measures whether the spin points up or down along the z axis. Now, let $\mathcal{S}$ be measured by an apparatus $\mathcal{A}$ in the following way: If the system is in state $|\uparrow_z\rangle_\mathcal{S}$, the apparatus ends up in the state $|\uparrow_z\rangle_\mathcal{A}$ at the conclusion of the measurement, i.e., the final system–apparatus combination can be described by the product state $|\uparrow_z\rangle_\mathcal{S}|\uparrow_z\rangle_\mathcal{A}$ (and similarly for $|\downarrow_z\rangle_\mathcal{S}$). Since we may think of the $|\uparrow_z\rangle_\mathcal{A}$ and $|\downarrow_z\rangle_\mathcal{A}$ as representing different pointer positions on a dial (say "pointer up" and "pointer down"), the $|\uparrow_z\rangle_\mathcal{A}$ and $|\downarrow_z\rangle_\mathcal{A}$ are often referred to as the "pointer states" of the apparatus.

Suppose now that the state of $\mathcal{S}$ before the measurement is given by the superposition $\frac{1}{\sqrt{2}}(|\uparrow_z\rangle_\mathcal{S} - |\downarrow_z\rangle_\mathcal{S})$. Then, at the conclusion of the measurement, the combined (entangled) state of $\mathcal{S}$ and $\mathcal{A}$ is

$$|\Psi\rangle_{\mathcal{SA}} = \frac{1}{\sqrt{2}}(|\uparrow_z\rangle_{\mathcal{S}}|\uparrow_z\rangle_{\mathcal{A}} - |\downarrow_z\rangle_{\mathcal{S}}|\downarrow_z\rangle_{\mathcal{A}}). \tag{7.8}$$

We note that this again represents the final state of a typical von Neumann measurement [cf. (7.1) and (7.2)]. Looking at the state $|\Psi\rangle_{\mathcal{SA}}$, the answer to the question "what observable has been measured by $\mathcal{A}$?" seems obvious: σ_z, of course, i.e., the spin in z direction. But as the reader may easily verify, $|\Psi\rangle_{\mathcal{SA}}$ can in fact be rewritten using any other basis vectors $\{|\uparrow_{\hat{n}}\rangle_{\mathcal{S}}, |\downarrow_{\hat{n}}\rangle_{\mathcal{S}}\}$ of $\mathcal{S}$, where now $\hat{n}$ is a unit vector that can point into any arbitrary direction in space, and still $|\Psi\rangle_{\mathcal{SA}}$ will maintain its initial form. For example, if we choose $\hat{n}$ to point along the x axis, (7.8) becomes

$$|\Psi\rangle_{\mathcal{SA}} = \frac{1}{\sqrt{2}}(|\uparrow_x\rangle_{\mathcal{S}}|\uparrow_x\rangle_{\mathcal{A}} - |\downarrow_x\rangle_{\mathcal{S}}|\downarrow_x\rangle_{\mathcal{A}}). \tag{7.9}$$

What would we now deduce from this form of $|\Psi\rangle_{\mathcal{SA}}$ as the measured observable? Apparently σ_x, i.e., a measurement of the spin in x direction. So it appears that once we have measured the spin in *one* direction (again, interpreting the formation of correlations between $\mathcal{S}$ and $\mathcal{A}$ as a measurement), we seem to also have measured the spin in *all* directions. But wait, the reader may now object, σ_z and σ_x do not commute, so they can't be measured simultaneously!

The conclusion to be drawn is that quantum mechanics, in the form of the von Neumann measurement scheme applied to the isolated system–apparatus combination, does not automatically specify the observable that has been measured. This is certainly hard to reconcile with our experience of the workings of measuring devices that seem to be designed to measure highly specific physical quantities. We can generalize this problem by asking why (especially macroscopic) objects are usually found in a very small set of eigenstates, most prominently in position eigenstates. In fact, the observation that "things around us" always seem to be in definite spatial locations, whereas the linearity of the Hilbert space of the quantum mechanical formalism would in principle allow for arbitrary superposition of positions, is maybe the most intuitive and direct illustration of the preferred-basis problem.

The inclusion of interactions with an environment suggests a solution to this problem. The system $\mathcal{S}$ and the apparatus $\mathcal{A}$ will, in all realistic situations, never be fully isolated from their surrounding environment $\mathcal{E}$. Thus, in addition to the desired measurement interaction between $\mathcal{S}$ and $\mathcal{A}$, there will also be an interaction between $\mathcal{A}$ (and $\mathcal{S}$) and $\mathcal{E}$, leading to the formation of further correlations. Many such $\mathcal{A}$–$\mathcal{E}$ interactions will, however, result in a disturbance of the initial correlations between $\mathcal{S}$ and $\mathcal{A}$, thus altering, or even destroying, the measurement record, which would render it impossible for an observer to perceive the outcome of the measurement.

Zurek therefore proposed the definition of a "preferred pointer basis" of the apparatus as the basis that "contains a reliable record of the state of the system $\mathcal{S}$" [10], that is, the basis $\{|a_k\rangle\}$ of $\mathcal{A}$ in which the correlations $|s_k\rangle|a_k\rangle$ are least affected by the interaction between $\mathcal{A}$ and $\mathcal{E}$ (for simplicity, we shall assume here that $\mathcal{S}$ interacts directly only with $\mathcal{A}$ but not with $\mathcal{E}$). A sufficient (but not necessary) criterion for such a pointer basis would then be given by requiring all the projectors $|a_k\rangle\langle a_k|$ to commute with the apparatus–environment interaction Hamiltonian $H_{\mathcal{AE}}$ (the so-called "commutativity criterion"), that is,

$$[\,|a_k\rangle\langle a_k|,\, H_{\mathcal{AE}}\,] = 0 \qquad \text{for all } k. \tag{7.10}$$

In other words, the apparatus would be able to measure (i.e., be designed to measure) observables reliably that are linear combinations of the $|a_k\rangle\langle a_k|$, but not necessarily certain other observables. Thus, the environment – or more precisely, the form of the apparatus–environment interaction Hamiltonian – determines the preferred basis of the apparatus, and in turn also the preferred basis of the system ("environment-induced superselection").

Of course, we can generalize these findings from a setup explicitly containing measuring devices to the more general situation of entanglement between arbitrary systems and their environment. The fact that physical systems are usually observed to have determinate values only with respect to a small number of quantities (typically position for macroscopic objects) can then be explained by the fact that the system–environment interactions depend on precisely these quantities, e.g., distance (relative position). The commutativity criterion then implies that the system will preferably be found in (approximate) eigenstates of observables corresponding to those quantities. Since this selection mechanism is based on standard unitary quantum mechanics, it avoids the necessity to postulate ad hoc basis selection criteria, and it can therefore also be expected to be in agreement with our observations.

Apart from the most simple toy model cases, the commutativity criterion holds usually only approximately [11], and general operational methods have therefore been proposed to determine (at least in principle) the preferred basis in more complex situations [12]. One remaining conceptual problem concerns the question of what counts as the "system" and what as the "environment," and where to place the cut (see the discussion in Sect. 7.4 below). Nonetheless, environment-induced selection can be considered as the most promising approach toward explaining the emergence and stability of preferred states.

7.3.2 The Problem of Outcomes

Let us again consider the situation of von Neumann quantum measurement in form of an interaction that entangles the state of the system with the state of the measuring apparatus. We now also include the environment into the chain of interactions. That is, the apparatus $\mathcal{A}$ interacts with the system $\mathcal{S}$; in turn, the $\mathcal{SA}$ combination then interacts with the environment $\mathcal{E}$. The linearity of the Schrödinger equation yields the following time evolution of the entire system $\mathcal{SAE}$:

$$\begin{aligned}\left(\sum_n \lambda_n |s_n\rangle\right)|a_0\rangle|e_0\rangle &\longrightarrow \left(\sum_n \lambda_n |s_n\rangle|a_n\rangle\right)|e_0\rangle \\ &\longrightarrow \sum_n \lambda_n |s_n\rangle|a_n\rangle|e_n\rangle.\end{aligned} \tag{7.11}$$

Here $|a_0\rangle$ and $|e_0\rangle$ are the initial states of the apparatus and the environment, respectively. Evidently, after the interaction has taken place, the combined system $\mathcal{SAE}$ is described by a coherent pure-state superposition at all times. While the dislocalization of the phases λ_n into the $\mathcal{SAE}$ combination resulting from the interaction between $\mathcal{S}$, $\mathcal{A}$, and $\mathcal{E}$ "dissolves" local interference into the global system (see Sect. 7.2), this decoherence process by itself does not automatically explain why definite outcomes are perceived. Since superpositions represent individual quantum states in which all components of the superposition "exist" simultaneously, we cannot (and must not) isolate a single apparatus state $|a_m\rangle$ that would indicate an actual outcome of the measurement.

We can break free from the persistence of coherence in the $\mathcal{SAE}$ combination only when the dynamics of the open subsystem $\mathcal{SA}$ in terms of its reduced density matrix is considered. And, of course, all that we really need is the ability to ascribe a definite value to $\mathcal{A}$ (to be precise, to the $\mathcal{SA}$ combination, if the measurement is to be considered faithful), rather than to the total system $\mathcal{SAE}$. The time evolution of the reduced density matrix will in general be nonunitary, since it is not only influenced by the Hamiltonian of $\mathcal{SA}$, but also by the interacting (but averaged-out) environment. As indicated before, decoherence leads to the formation of "classical-looking" density matrices for $\mathcal{SA}$: The reduced density matrix $\rho_{\mathcal{SA}}$ becomes rapidly diagonal in a set of stable, environment-selected basis states. In other words, the decohered density matrix of the local system–apparatus combination becomes *operationally* indistinguishable from that of an ensemble of states, and it correctly describes the time evolution of the open system $\mathcal{SA}$.

It would then seem that decoherence could account for the existence of a local ensemble of potential measurement outcomes with definite probabilities (that in turn could then be related to the occurence of single outcomes in individual measurements). The problem with this argument has already been briefly touched upon earlier: The averaging-out of environmental degrees of freedom by means of the trace operation needed to arrive at the reduced density matrix relies on the probabilistic interpretation of the state vector (i.e., on the interpretation of $|\langle\varphi_k|\Psi\rangle|^2$ as the probability for the system described by the state vector $|\Psi\rangle$ to be found in the state $|\varphi_k\rangle$ upon measurement). In turn, this is related to the assumption of some form of wavefunction "collapse" at a certain stage of the observational chain. In this sense, taking the trace essentially "amounts to the statistical version of the projection postulate" [13]. Of course we do not want to presuppose some sort of collapse that would solve the measurement problem trivially without even necessarily having to worry about the role of decoherence.

We therefore conclude that, by itself, decoherence does not directly solve the measurement problem. After all, this might not come as a surprise, as decoherence simply describes unitary entanglement of wavefunctions – and since the resulting entangled superpositions are precisely the source of the measurement problem, we cannot expect the solution to this problem to be provided by decoherence. However, the fact that the reduced density matrices obtained from decoherence describe observed open-system dynamics and the emergence of quasiclassical properties for these systems perfectly well, decoherence is extremely useful in motivating solutions to the measurement problem. This holds especially when the physical role of the observer is correctly taken into account in quantum-mechanical terms of system–observer correlations, making more precise what the "perception of definite outcomes" and the related measurement problem actually *mean* in terms of physical observations.

Accordingly, we shall describe in Sec. 7.5 how decoherence can be put to use in various interpretations of quantum mechanics, especially with respect to a resolution of the measurement problem. Before that, however, we shall discuss in the next section a couple of conceptual issues related to decoherence.

7.4 Resolution into Subsystems and the Closed-Universe Objection

The application of the theory of decoherence requires a decomposition of the total Hilbert space into subsystems. As long as we consider the universe as a whole, it is fully described by its state vector $|\Psi\rangle$ that evolves strictly deterministically according to the Schrödinger equation, and no interpretive problem seems to arise here. The notorious measurement problem only comes into play once we decompose the universe into subsystems (thus forming the joint product state $|\Psi\rangle = |\psi_1\rangle \otimes |\psi_2\rangle \otimes \cdots$), and attempt to attribute *individual* states to the subsystems.

However, there exists no general criterion that would determine where the splitting cuts are supposed to be placed. Of course, in a standard laboratory-like measurement situation, the physical setup might lead to an easy identification of "the system of interest," "the measuring device," and "the external environment." But this is a rather special and subjective rule for the splitting, and confronted with a more complex state space (encompassing, say, larger contiguous parts of the universe), there is neither a general rule for decomposition (given, for example, a total Hilbert space and its Hamiltonian) nor a definition for what counts as a "system." This issue becomes particularly important if one would like to use decoherence to define "objective macrofacts" of the universe as a whole. On the other hand, one might of course adopt the view that all correlations (and the resulting properties) should be considered as intrinsically relative to a given local observer, and that therefore a general rule for "objective" state-space decompositions need not be required.

Also, the ignorance-based coarse-graining procedure required by decoherence to obtain the reduced density matrix requires the openness of the system. But what about if we take this system to be the universe as a whole? (Quantum cosmology, for example, is all about studying the evolution of the universe in its entirety.) By definition, the universe is a closed system, and thus no external environment exists whose "unobserved" degrees of freedom could be averaged over. This has become known as the "closed-universe problem." From the point of view of talking about "events" or "facts" as the result of observations, this does however not necessarily constitute a problem, since every observation is inherently local and presupposes the ignorance of certain other parts. As Landsman [14] put it, "the essence of a 'measurement', 'fact' or 'event' in quantum mechanics lies in the non-observation, or irrelevance, of a certain part of the system in question.... A world without parts declared or forced to be irrelevant is a world without facts."

7.5 Decoherence and Interpretations of Quantum Mechanics

There are numerous interpretive approaches to quantum mechanics. On the "standard" (textbook) side, we have the "orthodox" interpretation with its infamous collapse postulate, together with the similar (and often not distinguished) Copenhagen interpretation. As "alternative" interpretations, we can name several main categories: the relative-state interpretation, introduced by Everett [15] (and further developed as "many-worlds" and "many-minds" interpretations); the class of modal interpretations, first suggested by van Fraassen [16]; physical collapse theories like the Ghirardi–Rimini–Weber (GRW) approach [17]; the consistent-histories approach introduced by Griffiths [18]; and the de Broglie–Bohm pilot-wave theory, a highly non-local hidden-variable interpretation [19]. Common to all of the alternative approaches is their attempt to dispose of the collapse postulate of the orthodox (and Copenhagen) interpretation. Some of them are just alternative readings of the formalism of standard quantum mechanics (Everett), others modify the rules that connect the formalism to the actual physical properties (modal interpretations), postulate new physical mechanisms (GRW), and introduce additional governing equations (de Broglie–Bohm).

The necessity to include environmental interactions for a realistic description of the behavior of physical systems is an objective one, independent of any interpretive framework. But the effects (and their proper interpretation) arising from such interactions have much to do with conceptual and interpretive stances. For instance, we might ask whether decoherence effects alone can already solve some of the foundational problems without the need for certain interpretive "additives," or whether decoherence can motivate (or falsify) some approaches – or even lead to a unification of different interpretations. In the following, we discuss some of the connections between decoherence and the main interpretations of quantum mechanics [20].

7.5.1 The Orthodox and the Copenhagen Interpretations

A central element of orthodox interpretations is the well-known collapse (or projection) postulate which prescribes that every measurement, represented by some suitably chosen observable, leads to nonunitary reduction of the total state vector to an eigenstate of the measured observable. To avoid the preferred basis problem, measurements are assumed to be carried out by an "observer" that can freely "choose" an observable before the measurement, and thus determine what properties can be ascribed to the system after the measurement (a strongly positivist, observer-dependent viewpoint).

A major problem with this approach is that it is not clearly defined what counts as a "measurement," and that the measuring process has a strong "black box" character. It does not explain why measuring devices seem to be designed to measure certain quantities but not others. Taking into account environmental interactions can provide the missing physical description of measurements. According to the stability criterion of the decoherence program, for a measurement to count as such, it must lead to the formation of stable records in spite of immersion into the environment. Therefore, the structure of the interaction between the apparatus and its environment singles out the preferred observables of the apparatus (and thereby also determines what properties can be assigned to the measured system). In this sense, decoherence and environment-induced selection can augment, if not replace, the formal and vague concept of measurement employed by the orthodox interpretation with general observer-independent criteria that specify what observables can actually be measured by a given apparatus.

The most distinctive feature of the Copenhagen interpretation (compared to the orthodox interpretation) is its postulate of the necessity for classical concepts to describe quantum phenomena. Instead of deriving classicality from the quantum world, e.g., by considering some macroscopic limit, the requirement for a classical description of the "phenomena," which comprise the whole experimental arrangement, is taken to be a fundamental and irreducible element of a complete quantum theory. Specifically, the Copenhagen interpretation postulates the existence of intrinsically classical measuring devices that are not to be treated quantum mechanically. This introduces a quantum–classical dualism into the description of nature and requires the assumption of an essentially nonmovable boundary (the famous "Heisenberg cut") between the "microworld," containing the objects that are to be treated as quantum systems, and the "macroworld" that has to be described by classical physics.

However, the studies of decoherence phenomena demonstrate that quasiclassical properties, across a broad range from microscopic to macroscopic sizes, can emerge directly from the quantum substrate through environmental interactions. This makes the postulate of an a priori existence of classicality seem unnecessary, if not mistaken, and it renders unjustifiable the placement of a fixed boundary to separate the quantum from the classical realm on a fundamental level.

7.5.2 Relative-State Interpretations

The core idea of Everett's original relative-state proposal, and of its interpretive extensions into a many-worlds or many-minds framework, is to assume that the physical state of an isolated system (in particular, that of the entire universe) is described by a state vector $|\Psi\rangle$, whose time evolution is given by the Schrödinger equation that is assumed to be universally valid. All terms in the superposition of the total state correspond in some way to individual physical states (realized, for instance, in different "branches" of the universe or "minds" of an observer). One major difficulty of this approach is the preferred basis problem, which is here particularly acute since each term in the state vector expansion is supposed to correspond to some "real state of affairs." Thus, it is crucial to be able to define uniquely a particular basis in which to expand the continuously branching (since new quantum correlations are formed constantly and everywhere) state vector at each instant of time.

It has frequently been suggested to use the environment-selected basis to define the preferred branches. This has several advantages. Instead of having simply to postulate what the preferred basis is, the basis arises through the interaction with the environment and the natural criterion of "robustness." Clashes with empirical evidence are essentially excluded, since the selection mechanism is based on well-confirmed Schrödinger dynamics. Finally, and maybe most importantly, the environment-preferred components of the decohered wavefunction can be reidentified over time, which yields stable, temporally extended branches.

There have been several criticisms of this idea. First, as we have pointed out before, there exists no objective rule for what counts as a system and what can be considered as the environment. Therefore, decoherence-induced selection of branches is often promoted in the context of an observer-based (subjective) interpretation [21]. Typically this includes the observer's neuronal (perceptional) apparatus in the full description of observations, instead of assuming the existence of "external" observers that are not treated as interacting quantum systems. Each neuronal state then becomes correlated with the states corresponding to the individual terms in the superposition of the observed system, and decoherence between these different brain states [22] is assumed to prevent the different "outcome records" from interfering and thus to lead to a perception of individual outcomes.

Second, decoherence typically yields only an approximate ("for all practical purposes" [23]) definition of a preferred basis and therefore does not provide an "exact" specification of branches [24]. Responses to this criticism suggest that it is fully sufficient for a physical theory to account for our experiences, which does not entail the necessity for exact rules as long as the emerging theory is empirically adequate [25].

7.5.3 Modal Interpretations

The main characteristic feature of modal interpretations is to abandon the rule of standard quantum mechanics that a system must be in an eigenstate of an observable in order for that observable to have a definite value. In its place, new rules are introduced that specify lists of possible properties (definite values) that can be ascribed to a system given, for example, its density matrix $\rho(t)$. The results of the theory of decoherence have frequently been used to motivate and define such rules of property ascription. Some [26] have even suggested that one of the main goals of modal interpretations is to provide an interpretation of decoherence. The basic approach consists of using environment-selected preferred bases (in which the decohered reduced density matrix is approximately diagonal) to specify sets of possible quasiclassical properties associated with the correct probabilities. This provides a very general and entirely physical rule for property ascriptions that can be expected to be empirically adequate. The rule could also be used to yield property states with quasiclassical, continuous "trajectory-like" time evolution (since the decohered components of the wavefunction are stable and can thus be reidentified at over time) that is in accordance with unitary quantum mechanics [27].

The difficulty with this approach lies in the fact that determining the environment-selected robust basis states explicitly is nontrivial in more complex systems. The aim of modal interpretations, however, has been to formulate a general rule from which the set of possible properties can be directly and straightforwardly derived. Frequently, instead of explicitly finding preferred states on the basis of the stability criterion (or a similiar measure), the orthogonal decomposition of the decohered density matrix has been used to determine the property states directly. When applied to discrete models of decoherence (that is, for systems described by a finite-dimensional Hilbert space), this method has in most cases been found to yield states with the desired quasiclassical properties, similar to those obtained from the stability criterion, at least when the final composite state was sufficiently nondegenerate [28]. In the continuous case, however, it has been demonstrated that the predictions of decoherence (e.g., as measured by the coherence length of the density matrix) and the properties of the states determined from the orthogonal decomposition do not mesh [29]. Thus decoherence can here be used to indicate that certain methods of property ascription might be physically inadequate [30].

7.5.4 Physical Collapse Theories

These are theories that modify the unitary Schrödinger dynamics to induce an actual collapse of the wavefunction based on a physical mechanism. The most popular version has probably been the one proposed by Ghirardi, Rimini and Weber (GRW) [31] which postulates the existence of instantaneously and spontaneously occurring "hits" that lead to a spatial localization of the wavefunction. The frequency of the hits is chosen such that macroscopic objects are localized faster than any observation could resolve, while preserving an effectively unitary time evolution on microscopic scales.

Decoherence provides a physical motivation for the a priori choice of position as the universal preferred basis in the GRW theory. Many physical interactions are described by distance-dependent terms, which according to the stability criterion of the decoherence program leads to the selection of (at least approximate) eigenstates of the position operator as the preferred basis. On the other hand, however, decoherence also demonstrates that in many situations position will *not* be the preferred basis. This occurs most commonly on microscopic scales, where systems are typically found in energy rather than position eigenstates [32], but also for instance in superconducting quantum interference devices [33] that exhibit superpositions of macroscopic currents. As far as microscopic systems are concerned, the GRW theory avoids running into empirical inadequacies by having the spatial localization hits occur so rarely that state vector reduction in the position basis is effectively suppressed. However, this has certainly an ad hoc character in comparison with the more sensitive, general, and physically motivated basis selection mechanism of the decoherence program. Furthermore, since decoherence will always be present in any realistic system, the assumption that the GRW theory holds means that we can expect to have two selection mechanisms that either act in the same direction (if decoherence also leads to a spatial localization) or compete with each other (in cases where decoherence predicts a different preferred basis than position).

It also has been found that the governing equations for the time evolution of the density matrix of a system in the GRW theory bear remarkable similarity to the evolution equations obtained from an inclusion of environmental interactions. This has raised the question whether it is necessary to postulate an explicit collapse mechanism, or whether at least the free parameters in the equations of the GRW approach could be directly derived from the study of environmental interactions [34]. (Of course, the GRW theory achieves true state vector reduction, whereas decoherence only leads to improper ensembles, so they are not on the same interpretive footing.) Assuming the simultaneous presence of decoherence and GRW effects, one could imagine an experimental falsification of the GRW theory by means of a system for which GRW predicts a collapse, but decoherence leads to no significant loss of coherence [35]. However, since any realistic system is extremely hard to shield from decoherence effects, such an experiment would presumably be very difficult to carry out [36].

7.5.5 Consistent-Histories Interpretations

The central idea of this approach is to dispose of the fundamental role of measurements (that assume the existence of external observers) in quantum mechanics and instead study quantum "histories," i.e., sequences of quantum events represented by sets of time-ordered projection operators, and to attribute probabilities to such histories. A set of histories is called consistent (judged by an appropriate mathematical criterion) when all its members are independent, that is, when they do not interfere and the classical probability calculus can be applied.

One major problem of this approach has been that the consistency criterion appears to be insufficient to single out the quasiclassical histories that would correspond to the world of our experience – in fact, most consistent histories turn out to be highly nonclassical [37]. To overcome this difficulty, decoherence has frequently been employed in proposals that would lead to a selection of quasiclassial histories, and also in attempts to provide a physical motivation for the consistency criterion [38]. Interestingly, this move has also introduced a conceptual shift. While the original aim of the consistent-histories program had been to define the time evolution of a single, closed system (often the entire universe, where standard quantum mechanics runs into problems as no external observers can be present), wedding decoherence to the consistent-histories formalism requires a division of the total Hilbert space into subsystems and the openness of the local subsystems.

The decoherence-based approach commonly consists of using the environment-selected pointer states that (approximately) diagonalize the reduced density matrix as the projectors of histories. This leads typically to the emergence of histories that are stable and exhibit quasi-classical properties, since the pointer basis is "robust" and corresponds well to the determinate quantities of our experience. Moreover, such histories defined by projectors corresponding to the pointer basis also turn out to fulfill the consistency criterion automatically, at least approximately. This has led to the argument that the consistency criterion is both insufficient and overly restrictive in singling out histories with quasiclassical properties, and to a questioning of the fundamental role and relevance of this criterion in consistent-histories interpretations in general [39].

7.5.6 Bohmian Mechanics

Bohm's approach describes the deterministic evolution of a system of particles, where the system is described both by a wavefunction $\psi(t)$, evolving according to the standard Schrödinger equation, and by the particle positions $\mathbf{q}_k(t)$, whose dynamics are determined by a simple "guiding equation" for the velocity field, essentially the gradient of $\psi(t)$. Particles then follow well-defined trajectories in configuration space represented by the configuration $\mathcal{Q}(t) = (\mathbf{q}_k(t), \ldots, \mathbf{q}_N(t))$, whose distribution is $|\psi(t)|^2$.

Bohm's theory has been criticized for attributing fundamental ontological status to particles. It has been argued that, since decoherence typically leads to ensembles of wavepackets that are narrowly peaked in position space, one can identify these wavepackets with our (subjective) perception of particles, i.e., spatially localized objects [40]. This suggests that the explicit assumption of the existence of actual particles at a fundamental level of the theory might be rendered superfluous (modulo the basic question of how to go from an apparent to a proper ensemble of wavepackets).

Another problem is how to relate the Bohmian particle trajectories to quasiclassical trajectories that emerge on a macroscopic scale. Going back to studies of Bohm himself [41], it has been suggested that the inclusion of environmental interactions could provide the missing ingredient to arrive at quasiclassical trajectories. Typically the idea has been to identify the Bohmian trajectories $\mathcal{Q}(t)$ with the temporally extended, spatially localized wavepackets of the decohered density matrix that describe macroscopic objects. While this approach is highly intuitive and has been demonstrated to yield promising results in some of the explicitly studied examples, in other cases this identification turns out to be insufficient to sustain the classical limit [42].

7.6 Outlook

The key idea of the decoherence program relies on the insight that, in order to properly describe the behavior of a physical system in quantum-mechanical terms, the omnipresent interactions of the system with the degrees of freedom of its environment must be taken into account. The application of the formalism of decoherence to numerous model systems has led to many experimentally verified results, so the idea has proven to be very successful. Interestingly, however, the rather straightforward and well-studied approach of decoherence, both experimentally and theoretically, has led to several fundamental interpretive and conceptual questions.

By itself, decoherence simply describes environmental entanglement and the resulting practically irreversible dislocalization of local phase relations (i.e., of quantum-mechanical superpositions). Since the entangled pure state makes it impossible to assign an individual state vector to the system, the dynamics of the system must be described by a nonunitarily evolving reduced density matrix. While decoherence transforms such density matrices into apparent ensembles of quasiclassical states (which, when properly interpreted, may be used to obtain a physically motivated resolution of the measurement problem), the formalism and interpretation of reduced density matrices *presume* the probabilistic interpretation of the wavefunction. Thus decoherence alone (i.e., without being augmented by some additional interpretive elements) cannot solve the measurement problem. Furthermore, the requirement for a division of the universe into "systems" and "environments" introduces a strong flavor of subjectivity, since no general and objective rule exists for how and where to place the cuts. Also, the necessity for an "external" environment leads to difficulties when one would like to apply the theory to the universe as a whole, as in quantum cosmology.

This situation requires and motivates interpretive frameworks beyond the "orthodox" interpretation, frameworks that might provide some of the missing steps toward a conceptually complete and consistent interpretation of the decoherence program, and of quantum mechanics as a whole. Conversely, the assumptions made by an interpretation must be consistent with the results obtained from decoherence, thus narrowing down the spectrum of possible (empirically adequate) interpretations – maybe even making the choice between different such interpretations “purely a matter of taste, roughly equivalent to whether one believes mathematical language or human language to be more fundamental,” as Tegmark [43] put it in a comparison between the orthodox interpretation and decoherence-based relative-state interpretations. Clearly, the rather simple idea of including environmental interactions as promoted by decoherence has an extremely important impact on the foundations of quantum mechanics, suggesting solutions to fundamental problems as well as posing new conceptual questions.

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8

What Are Consistent Histories?

Alan Thorndike

8.1 Introduction

In the standard interpretation, quantum mechanics answers questions about the probabilities of obtaining such and such a value for such and such a measurement in such and such an experiment. A particle released from location a at a certain time has a certain probability of being detected at location b at some later time. This is a rather restricted view. It has nothing to say about quantities that aren't measured as part of the experiment, such as locations at intermediate times. And it is quiet about plausible physical processes that are not accessible to measurement, events that happened long ago or far away, for example. Finally, in the standard interpretation, the act of measurement forces a system to assume one of its several possible states, and thus interrupts the natural evolution of the system.

Over the last twenty years, a new interpretation of quantum mechanics has been developed that addresses these restrictions. The new interpretation – consistent histories – allows one to assign probabilities to sequences of states without actually perturbing the system with measurements at each time in the sequence. But this is only possible if certain conditions are satisfied. Consistent histories play an important role in current discussions of the foundations and interpretation of quantum mechanics, not the least by helping us sharpen our thinking about what constitutes a meaningful question and what does not. In this chapter, I provide an introduction to the notion of consistent histories and show how the mathematics works in a few examples.

8.2 How Does a System Get from *a* to *b*?

Suppose the state of a system is known at some initial time and at some final time. What can be inferred about the state at an intermediate time? In particular, if the initial state was $|a\rangle$ and the final state was $|b\rangle$, what is the probability that the intermediate state was $|r\rangle$? By this is meant that at the initial time a measurement of the observable A was made, resulting in the eigenvalue a, associated with the state $|a\rangle$. At the final time, B was measured with the result b. At the intermediate time, R was not measured, but we'd like to know what the result would have been, or at least to know the probabilities associated with the possible results of that measurement. Perhaps such questions arise in cosmology when one seeks to make inferences about the past, conditioned on present observations.

In the standard interpretation of quantum mechanics there is a "don't ask, don't tell" policy toward such questions. Quantum mechanics answers questions about quantities measured in experiments, and is silent about quantities that are not measured. Yet our classical experience includes many situations where we don't hesitate to make statements about things we didn't measure based on those we did. The consistent histories interpretation of quantum mechanics, developed by Griffiths [1], provides a way to make statistical statements about unmeasured quantities if certain conditions are met [2].

To begin, it is enough to consider observables that have only two possible outcomes, such as a component of spin of a spin 1/2 particle. Suppose, for example, a spin 1/2 particle, free of any interactions, is found to have $s_z = +1/2$ at the initial and final times. ($A = B = s_z$, $a = b = +1/2$.) If we ask about $R = s_z$, it seems likely that the probability Prob(r=+1/2) should be unity and Prob(r=−1/2) should be zero. This intuition relies on the assumption that the particle is free of interactions. Or we might ask about $Q = s_x$ at the intermediate time. If this makes you nervous, perhaps the following notes will help. Of course a better strategy is to refer to the original papers and subsequent texts [3].

First consider the classical situation. Denote the conditional probability for observing b at t_f, given that the state was a at time t_o, by the symbol $\{ba\}$. This abbreviated notation does not display the times explicitly. Because we have only two possible outcomes for each observable, we can denote them as a and $\bar{a}$ (= "not a"), b and $\bar{b}$, r and $\bar{r}$. Then

$$\{ba\} = \{br\}\{ra\} + \{b\bar{r}\}\{\bar{r}a\} = \{bra\} + \{b\bar{r}a\} \qquad (8.1)$$

and the conditional probability for finding r at the intermediate time is

$$\mathrm{Prob}(r|ab) = \frac{\{bra\}}{\{ba\}}. \tag{8.2}$$

The three-state symbols in (8.1) and (8.2) are defined as products of the conditional probabilities. These statements use the facts that the probability of a sequence of independent transitions is the product of the probabilities of the individual transitions, and that the probabilities associated with alternative distinct paths are additive.

In quantum mechanics the probability amplitudes are similar to, but not identical to, the conditional probabilities of classical probability theory. Associated with the transition from a to b is the amplitude

$$[ba] = \langle b|P(t_f, t_o)|a\rangle \tag{8.3}$$

where $P(t_f, t_o)$ denotes the propagator from the initial to the final time. The propagator can be expressed in terms of the Hamiltonian as $\exp(-iH(t_f - t_o))$, provided the Hamiltonian H does not depend explicitly on time. The probabilities in quantum mechanics are the complex squares of the amplitudes, so

$$\begin{aligned} \mathrm{Prob}(b|a) &= [ba]^*[ba] = [aba] \\ &= \langle a|P(t_o, t_f)|b\rangle\langle b|P(t_f, t_o)|a\rangle \\ &= \ \mathrm{Trace}\ (\Pi_a P_{of} \Pi_b P_{fo} \Pi_a) \end{aligned} \tag{8.4}$$

where Π_a denotes the projection operator $|a\rangle\langle a|$, and similarly for b. The final equality in (8.4) makes use of the fact that $\langle a|Q|a\rangle = \mathrm{Trace}(\Pi_a Q \Pi_a)$ for any operator Q. The square bracket notation is not to be confused with the commutator. It reminds us that we need to square the amplitudes to get probabilities. When more than two states are included in the square brackets, it is implied that the appropriate propagator is inserted between each pair of states.

The magic of the Dirac notation is that it can be parsed in different ways. We can read $[aba]$ in(8.4), as the product of two matrix elements $\langle a|P^*|b\rangle$ and $\langle b|P|a\rangle$ or as the (a, a) matrix element of the operator $P^* \Pi_b P$. Taking the latter interpretation, the quantum mechanical probabilities are calculated as the trace of a product of the operators

project → propagate → project → propagate ...

... around a closed circuit as in Fig. 8.1.

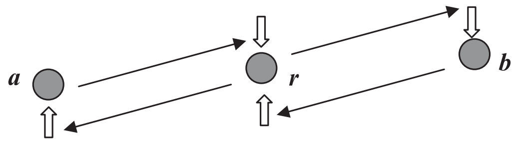

Fig. 8.1. The squared amplitude $[arbra]$ is found by propagating (solid arrow) the state a from t_o to t, projecting (open arrow) onto the state r, propagating from t to t_f, projecting onto b, propagating back in time to t, projecting onto r, propagating back to the initial time t_o, and finally projecting onto a.

Including the intermediate state produces the squared amplitude

$$[arbra] = \text{ Trace } (\Pi_a P \Pi_r P \Pi_b P \Pi_r P \Pi_a). \tag{8.5}$$

This quantity cannot be interpreted as the conditional probability of r given a and b, however. For suppose we calculate the squared amplitude for the intermediate state $|r \text{ or } \bar{r}\rangle$. We have

$$\begin{aligned} [aba] &= [a(r \text{ or } \bar{r})b(r \text{ or } \bar{r})a] \\ &= ([arb] + [a\bar{r}b])([bra] + [b\bar{r}a]) \\ &= [arbra] + [a\bar{r}b\bar{r}a] + 2 \text{ Real } [arb\bar{r}a]. \end{aligned} \tag{8.6}$$

The last term is a sort of interference term. Unless it vanishes we cannot interpret $[arbra]$ and $[a\bar{r}b\bar{r}a]$ as conditional probabilities, in analogy to (8.1). If the last term does vanish [4], we can write

$$\left.\begin{aligned} Prob(r|ab) &= [arbra]/[aba] \\ Prob(\bar{r}|ab) &= [a\bar{r}b\bar{r}a]/[aba] \\ Prob(r|ab) + Prob(\bar{r}|ab) &= 1 \end{aligned}\right\} \text{ if Real } [arb\bar{r}a] = 0. \tag{8.7}$$

The conclusion is that if the Griffiths condition, Real $[arb\bar{r}a] = 0$, is satisfied, it is possible to assign conditional probabilities to the intermediate states. Otherwise, not. Griffiths's condition involves calculating the amplitudes for closed circuits that pass through the initial and final states and through one intermediate state going forward in time and a different intermediate state on the return trip. (See Fig. 8.2).

8.2.1 Example 1

For the spin 1/2 example, the initial state $|a\rangle$ and final state $|b\rangle$ are spin up along the z axis, and the particle is free of any interactions, so

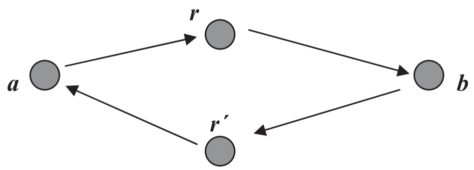

Fig. 8.2. Griffiths's condition examines the amplitudes for circuits of the form $[arbr'a]$ where r and r' are taken from different subspaces in the partition of R. For simplicity, the propagation and projection operators are shown together as a single arrow.

$H = 0$. We want to investigate whether it makes sense to speak of the particle as having passed through intermediate states $|r\rangle$ or $|\bar{r}\rangle$, which are spin up or spin down along the same z axis. Working in the s_z basis,

$$|a\rangle = |b\rangle = |r\rangle = \begin{bmatrix} 1 \\ 0 \end{bmatrix}, \quad |\bar{r}\rangle = \begin{bmatrix} 0 \\ 1 \end{bmatrix}, \tag{8.8}$$

$$\Pi_a = \Pi_b = \Pi_r = \begin{bmatrix} 1 & 0 \\ 0 & 0 \end{bmatrix}, \quad \Pi_{\bar{r}} = \begin{bmatrix} 0 & 0 \\ 0 & 1 \end{bmatrix},$$

$$P(t_2, t_1) = \mathrm{e}^{-iH(t_2 - t_1)} = 1,$$

$$[arb\bar{r}a] = \text{ Trace } \Pi_a P \Pi_r P \Pi_b P \Pi_{\bar{r}} P \Pi_a = 0.$$

This allows us to assign the conditional probabilities

$$\begin{aligned} \text{Prob}(r|ba) &= [arbra]/[aba] = 1, \\ \text{Prob}(\bar{r}|ba) &= [a\bar{r}\ b\bar{r}a]/[aba] = 0. \end{aligned} \tag{8.9}$$

This is what we expected, so the Griffiths formalism leads to results that correspond to our intuition in the case where our intuition is strong.

On the other hand, if we inquire about the x component of spin at the intermediate time, we find, still working in the s_z basis,

$$|r\rangle = \begin{bmatrix} 2^{-1/2} \\ 2^{-1/2} \end{bmatrix}, \quad |\bar{r}\rangle = \begin{bmatrix} 2^{-1/2} \\ -2^{-1/2} \end{bmatrix}, \tag{8.10}$$

$$\Pi_r = \begin{bmatrix} 1/2 & 1/2 \\ 1/2 & 1/2 \end{bmatrix}, \quad \Pi_{\bar{r}} = \begin{bmatrix} 1/2 & -1/2 \\ -1/2 & 1/2 \end{bmatrix}.$$

It follows that

$$[arb\bar{r}a] = 1/4,$$

which means we cannot assign conditional probabilities.

Perhaps you think, as I did, that the conditional probabilities of the intermediate states must be Prob(s_x=1/2) = Prob(s_x= −1/2) = 1/2, a plausible but wrong conclusion (call it PBW). To get a glimpse of the difficulties this PBW logic would lead to, consider a sequence of times $t_1, \ldots, t_5$, with initial and final states $s_z = 1/2$. If we ask about s_z at time t_3, we know Prob(s_z=1/2) = 1 and Prob(s_z=−1/2) = 0. At time t_2, using PBW, we would have Prob(s_x=1/2) = 1/2, and Prob(s_x=−1/2) = 1/2, and similarly at t_4. But if we now regard t_2 and t_4 as initial and final times, and ask about the s_z state at the intermediate time t_3, given the s_x states at t_2 and t_4, the same PBW logic will lead us to assign equal probabilities to $s_z = 1/2$ and $s_z = -1/2$, which conflicts with the earlier conclusion that at time t_3 the s_z state was certainly spin up.

8.3 Testing for Consistent Histories

In the language of consistent histories, the sequence of states arb ($= |a\rangle \to |r\rangle \to |b\rangle$) is called a history, and a family of histories arb and $a\bar{r}b$ is called a framework. Our example shows that for one framework ($R = s_z$) it is possible to assign probabilities to the two histories, whereas for a second framework ($R = s_x$) it is not.

These ideas extend to include longer histories $a \to q \to r \to s \to b$. It is easy to relax the specification of the initial, final, and intermediate states. We may be given only that the initial state is one of several eigenstates $\{a_j\}$ of A. We might ask: given that the state belonged to $\{a_j\}$ at the initial time, and $\{b_j\}$ at the final time, can we assign probabilities to the intermediate states $\{r_j\}$, $\{r'_j\}$,...? As in the single-state case, the answer is provided by Griffiths's consistency test,

$$\text{Real Trace } \Pi_{\{a\}} P \Pi_{\{r\}} P \Pi_{\{b\}} P \Pi_{\{r'\}} P \Pi_{\{a\}} = 0. \tag{8.11}$$

Here the projection operators are the sums of the single-state projectors:

$$\Pi_{\{a\}} = \sum_{a_j} |a_j\rangle\langle a_j|. \tag{8.12}$$

We can interpret the Griffiths test by listing all sequences of eigenstates $a_k \to r_m \to b_n$ and evaluating their amplitudes $[b_n r_m a_k]$. Eliminate from further consideration any history that doesn't go through $\{a_j\}$ and $\{b_j\}$. Griffiths's condition is equivalent to there being no two histories, one through $\{r_j\}$ and one through $\{r'_j\}$, and linking the same initial and final states. We can express this idea most simply by saying:

It is possible to assign conditional probabilities to the r subspaces if every path beginning at one of the $\{a_j\}$ and ending at one of the $\{b_j\}$ can be assigned uniquely to one of the r subspaces.

We could dub this statement the "condition of the unique intermediate." It suggests the following algorithm for constructing consistent histories. Given the initial and final states $\{a_j\}$ and $\{b_j\}$, and an observable R, we seek a partition of the Hilbert space into subspaces spanned by subsets of the eigenstates of R that satisfies the Griffiths condition. Begin with the fine-grained partition in which a subspace is associated with each eigenstates: $R = \{r_1\} \oplus \{r_2\} \oplus \ldots$. Choose particular initial and final states $|a_n\rangle$ and $|b_m\rangle$. By the condition of the unique intermediate, group together all $|r_k\rangle$states for which $[a_n r_k b_m]$ is non-zero, leading to a somewhat more coarsely grained partition. As we cycle through all pairs of initial and final states, the partition can only get coarser.

8.3.1 Example 1, Continued

In the first case ($R = s_z$), there are two paths linking the initial and final states $\uparrow\uparrow\uparrow$ and $\uparrow\downarrow\uparrow$. Only the first has non-zero amplitude. So it is true that every path beginning in one of the $\{a_j\}$ and ending at one of the $\{b_j\}$(i.e., every path of the form $\uparrow ? \uparrow$) can be assigned uniquely to one of the r subspaces (i.e., to either $r =\uparrow$ or $\bar{r} =\downarrow$), namely to the first.

In the second case ($R = s_x$), we still have only the single initial and final states to consider, $\uparrow ? \uparrow$, but the intermediate state cannot be assigned uniquely to $|r\rangle = |s_x = 1/2\rangle$ or to $|\bar{r}\rangle = |s_x = -1/2\rangle$ because the amplitudes for $\uparrow r \uparrow$ and $\uparrow \bar{r} \uparrow$ both are non-zero. Applying the algorithm, beginning with the initial state $|\uparrow\rangle$ and ending with the final state $|\uparrow\rangle$, we note that every r_j gives a non-zero amplitude $[a_1 r_j b_1]$, so all the r states must be grouped together, giving only the trivial, all-or-nothing partition.

8.3.2 Example 2

If $\{a\}$ and $\{b\}$ are single states, let $\{r\}$ consist of all r_j for which $[b r_j a] \neq 0$. It has probability one.

8.3.3 Example 3

For a not quite trivial example, let the eigenstates of A be

$$\begin{bmatrix}1\\0\\0\\0\end{bmatrix}, \begin{bmatrix}0\\1\\0\\0\end{bmatrix}, \begin{bmatrix}0\\0\\1\\0\end{bmatrix}, \begin{bmatrix}0\\0\\0\\1\end{bmatrix}.$$

Let those of B be

$$\begin{bmatrix}1\\1\\0\\0\end{bmatrix}, \begin{bmatrix}1\\-1\\0\\0\end{bmatrix}, \begin{bmatrix}0\\0\\1\\1\end{bmatrix}, \begin{bmatrix}0\\0\\1\\-1\end{bmatrix}.$$

And let those of R be

$$\begin{bmatrix}1\\1\\1\\0\end{bmatrix}, \begin{bmatrix}1\\-1\\0\\0\end{bmatrix}, \begin{bmatrix}-1\\-1\\2\\0\end{bmatrix}, \begin{bmatrix}0\\0\\0\\1\end{bmatrix},$$

all suitably normalized. Ignore dynamics by taking the propagator to be the identity operator. Assume initial condition $\{a_3\}$ and the final state $\{b_3, b_4\}$. Find the finest consistent partition of R.

To do so, we consider first the path $a_3?b_3$. (See Fig. 8.3.) The only paths with non-zero amplitude go through r_1 and r_3, so we must group these intermediate states together. Next, consider $a_3?b_4$. The only two paths with non-zero amplitude are $a_3r_1b_4$ and $a_3r_3b_4$, which would be another reason to group r_1 and r_3 together. The desired partition of R is then $\{r_1, r_3\}$, $\{r_2\}$, and $\{r_4\}$. The conditional probabilities for these properties can be calculated using (8.8). The first has probability one, and the others probability zero. The result is easily verified from the figure, which shows there is no path from a_3 to r_4 and no path from r_2 to $\{b_3, b_4\}$.

8.4 Conclusion

The question is whether one can use the results of measurements of certain observables to support conclusions about other observables that were not measured. The answer that Griffiths has given is “sometimes.” It depends on what you measure (the A and B) and how narrow a question you want to ask about the quantity you didn’t measure (i.e., how

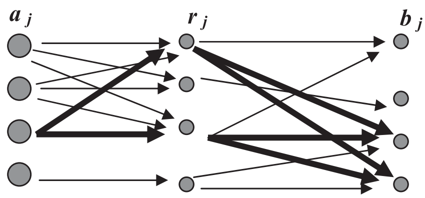

Fig. 8.3. The non-zero amplitudes linking a and r states and r and b states are shown. The bold arrows indicate the paths through the given initial state a_3 and the final states b_3, b_4. There are two paths linking a_3 and b_4, one through r_1, the other through r_3, so these states must belong to the same subspace in the partition of R. Otherwise there will be non-zero interference and probabilities that are not additive.

fine a partition of the R space you wish to resolve.) The general result is that it will not be possible to assign conditional probabilities if there are any paths with finite amplitude of the form $[bra]$ and $[br'a]$ linking the same initial state to the same final state and passing through intermediate states belonging to the disjoint subspaces $\{r\}$ and $\{r'\}$. The argument made no use of the order of the times t_o, t, t_f. The terms initial, intermediate, and final were superfluous. As is often the case, the root cause of the difficulty is that in quantum mechanics it is the probability amplitudes that are additive, not the probabilities themselves.

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4. Griffiths, "Consistent histories" (Ref 1)

9

Bose–Einstein Condensation: Identity Crisis for Indistinguishable Particles

Wolfgang Ketterle

9.1 The Phenomenon of Bose–Einstein Condensation

The phenomenon of Bose–Einstein condensation (BEC) is the most dramatic consequence of the quantum statistics that arise from the indistinguishability of particles. You are already aware that quantum mechanics started with Plancks law of black-body radiation. Let me familiarize you with the phenomenon of Bose–Einstein condensation on an intuitive level, emphasizing the root of the phenomenon, which is the indistinguishability of particles.

If we have a gas of ideal gas particles at high temperature, we may imagine those particles to be billiard balls (Fig. 9.1). They race around in the container and occasionally collide. This is a classical picture. However, if we use the hypothesis of de Broglie that particles are matter waves, then we have to think of particles as wave packets. The size of a wave packet is approximately given by the de Broglie wavelength λ_{dB}, which is related to the thermal velocity v of the particles as $\lambda_{\mathrm{dB}} = h/mv$. Here m is the mass of the particles and h Plancks constant. Now, as long as the temperature is high, the wavepacket is very small and the concept of indistinguishability is irrelevant, because we can still follow the trajectory of each wavepacket and use classical concepts. However, a real crisis comes when the gas is cooled down: the colder the gas, the lower the velocity, and the longer the de Broglie wavelength. When individual wave packets overlap, then we have an identity crisis, because we can no longer follow trajectories and say which particle is which. At that point, quantum indistinguishability becomes important and we need quantum statistics.

Based on work by Bose (Fig. 9.2), Einstein (Fig. 9.3) predicted in 1925 that when the de Broglie wavelength is comparable to the spacing

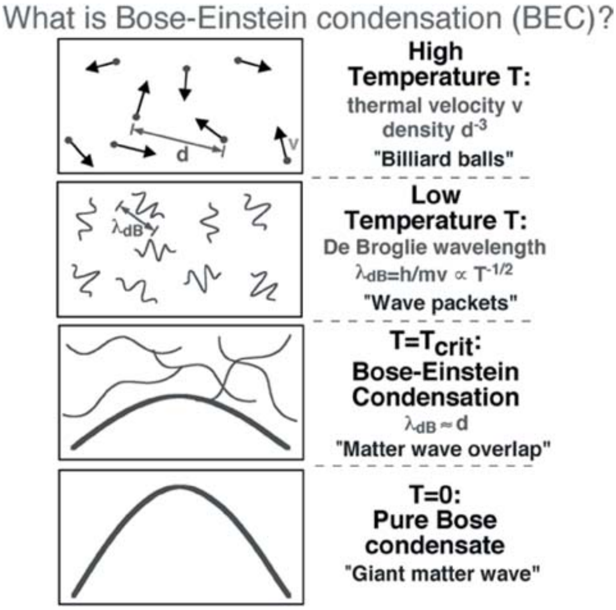

Fig. 9.1. Bose–Einstein condensation occurs when the de Broglie wavelength of the atoms becomes larger than their average spacing.

of particles – when quantum indistinguishability becomes important – there is a transition to a new phase of matter. What suddenly happens is that the particles come together in a single quantum state: they behave as one big matter wave. Intuitively one can say particles are wave packets, and when those wave packets overlap, then all the particles start to oscillate in concert or march in lockstep, and form one giant matter wave. They have lost their identity. If you could see them, they would all look the same. (You might also see few thermally excited atoms mixed in with the condensate.) Actually, what I have just said is valid for bosons only. For fermions, we have to apply the Pauli exclusion principle, and the gas will behave very differently.

The prediction of Bose–Einstein condensation, as we call it now, is more than 70 years old. It seemed for a long time that its only manifestation might be in condensed-matter physics, in liquid helium. But liquid helium is a liquid, not a gas, so it required strong modifications of those simple concepts. For twenty years there was an effort using atomic hydrogen to achieve conditions for which this condensation phenomenon could be observed. Indeed it was observed in hydrogen in 1998 [1], but the big excitement started three years earlier when alkali atoms were Bose condensed.

Fig. 9.2. Satyendra Nath Bose, in Paris, 1925. In 1924, from Dacca, India, Bose sent to Einstein a manuscript in which he applied a new kind of particle statistics to derive the coefficient of Planck's blackbody law quantum-theoretically, without recourse to classical concepts. ©Falguni Sarkar. SN Bose Biography Project www.snbose.org

Those observations in 1995, first at Boulder [2] and a few months later by my group at MIT [3], triggered a flurry of experimental and theoretical activities. The number of papers published each year with the terms "Bose" and "Einstein" in their title, abstract, or keywords exceeds now 400. Up till now, approximately 2000 papers have been published on the subject.

9.2 How to Count Classical and Quantum Particles

Let me now remind you what was the revolutionary step that was introduced by Bose and Einstein. You can disguise it in mathematical language, but I dont want to ask more of you than simply to count to three. Lets take three particles with colors black (B), gray (G) and white (W), and let's say their total energy is three. If we have four quantum levels (or single-particle states) with energies 0, 1, 2, 3, one way of arranging the three particles according to their total energy is to put all three particles in the level with energy one. We might denote this (multi-particle) state as $|\mathrm{BGW}\rangle = |111\rangle$. Of course we could also promote the gray particle to the level with energy two and demote the

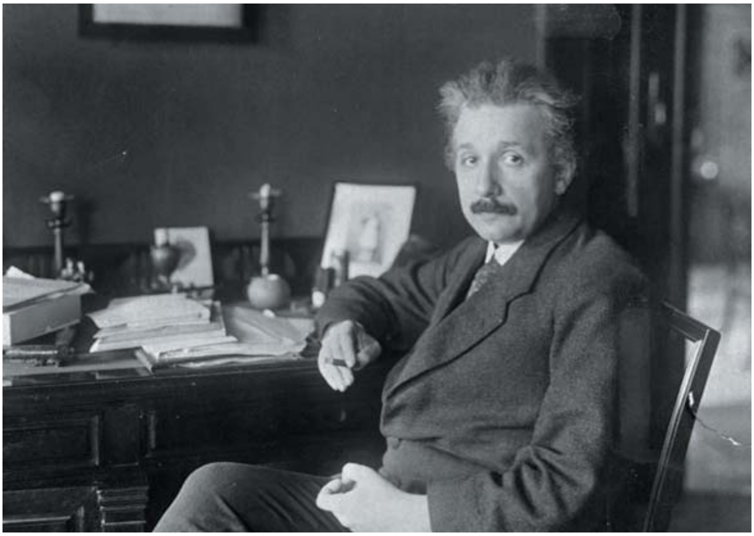

Fig. 9.3. Albert Einstein, photographed in 1929. In 1924, Einstein translated Bose's paper into German and arranged for its publication in *Zeitschrift für Physik*, then almost immediately extended Bose's method to massive particles. The following year, Einstein predicted the possibility of what is now called Bose–Einstein condensation. ©Bettmann/Corbis

black particle to the level with energy zero (this would be state $|021\rangle$), and still we do have a total energy of three. Or instead of promoting the gray particle, we could have promoted the white particle, obtaining $|012\rangle$, and so on. There are ten possible states with energy three (Fig. 9.4):

$$\begin{aligned}&|111\rangle,\\&|012\rangle, |021\rangle, |102\rangle, |201\rangle, |120\rangle, |210\rangle,\\&|300\rangle, |030\rangle, |003\rangle.\end{aligned}$$

Now let's do statistics and ask, "What is the probability that we find a particle in the level with zero energy?" A simple count shows that zero occurs 12 times in our list. Energy one occurs 9 times, energy two 6 times, and energy three 3 times. We can say that the probability of occupation is distributed over the energy levels 0, 1, 2, 3 in the ratios 4:3:2:1.

This is the situation for classical particles. They have different colors. They are distinguishable. If they had the same color, they would be indistinguishable. For indistinguishable particles, we have to be careful because some configurations are now the same, e.g., $|012\rangle, |021\rangle, |102\rangle, |201\rangle, |120\rangle, |210\rangle$. When we do statistics, we are not

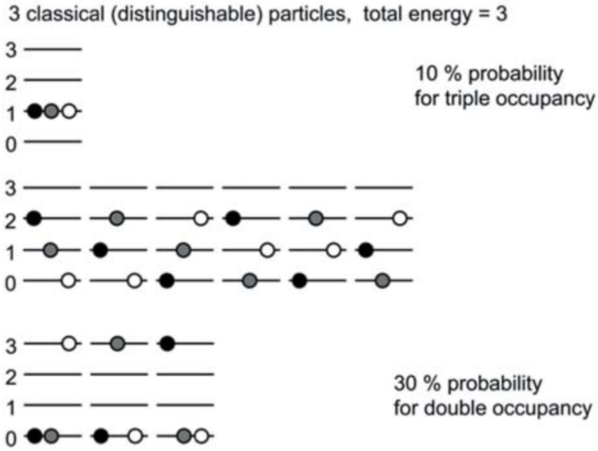

Fig. 9.4. All possible configurations to distribute three distinguishable particles with total energy three over four energy levels with energy 0, 1, 2, 3

allowed to count those configurations separately. Therefore, for bosons, there are only three possibilities left (Fig. 9.5):

$$|111\rangle, |012\rangle, \text{ and } |003\rangle.$$

For fermions two particles cannot occupy the same state, and therefore there is only one configuration allowed: $|012\rangle$. For bosons, the ratio of the populations of the energy levels zero to three is 3:4:1:1. For fermions it is 1:1:1:0. Therefore the assumptions we make about the particles (whether they are distinguishable or indistinguishable, and whether two particles can be in the same state or not) have dramatic consequences for how probable the various (single-particle) energies are.

Getting closer to the phenomenon of Bose–Einstein condensation, we ask now, "What is the probability for several particles to be in one state?" For classical particles, there is a 10 percent probability that three particles are in the same state (the $|111\rangle$ configuration) and 30 percent probability for double occupancy (the $|300\rangle$, $|030\rangle$, and $|003\rangle$ configurations). If you look at the boson system, those probabilities are higher, and if you look at fermions, the probability that two particles occupy the same quantum level is exactly zero. Therefore, just from this simple counting game we find that bosons are gregarious: they are social, they like to be together – whereas fermions are loners.

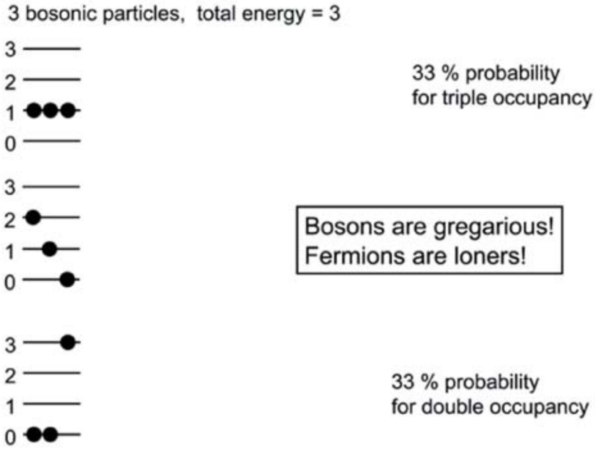

Fig. 9.5. All possibilities to distribute three indistinguishable bosonic particles with total energy three

Now, let's fast-forward to Bose–Einstein statistics. If we take many more than three particles in many more than four levels, we find the famous distribution laws associated with the names of Maxwell–Boltzmann, Bose–Einstein, and Fermi–Dirac:

$$P(\epsilon) \propto \frac{1}{\mathrm{e}^{(\epsilon-\mu)/kT}} \qquad \text{Maxwell–Boltzmann}$$

$$\frac{1}{\mathrm{e}^{(\epsilon-\mu)/kT}-1} \qquad \text{Bose–Einstein}$$

$$\frac{1}{\mathrm{e}^{(\epsilon-\mu)/kT}+1} \qquad \text{Fermi–Dirac}$$

$P(\epsilon)$ is the probability to find particles in an energy level with energy ϵ, T is the temperature and k Boltzmann's constant.

Those distribution functions look very similar, the only difference being plus or minus one, or the absence of the one in the denominator. But they are profoundly different because in the Bose–Einstein case, if the energy ϵ of the particle is close to a certain value, μ (which is the chemical potential, which I will explain below), the denominator can be zero and the probability of being in that particular state becomes infinitely large, leading to Bose–Einstein condensation.

9.3 Bose–Einstein Statistics and Planck's Black-Body Spectrum

Now it is fitting to relate the energy distribution of Bose–Einstein to Planck's blackbody spectrum for electromagnetic radiation. This is the celebrated formula

$$P(\epsilon) \propto \frac{1}{\mathrm{e}^{h\nu/kT} - 1} \quad \text{Planck}$$

where the photon energy $\epsilon = h\nu$ is expressed by the frequency ν of the radiation and Planck's constant h. I have left out the number of modes per unit energy interval. What I am comparing here is simply the population in one mode, or in one quantum state. Then you see the striking similarity between Planck's black-body spectrum and Bose–Einstein statistics. The difference is only that the value of μ is set to zero. This expresses the fact that the number of photons is not conserved: photons can appear and disappear; they can be emitted and absorbed. Bose, in his famous 1924 paper [4], derived Planck's black body spectrum using these counting statistics, thus combining the wave nature and the photon, or particle, nature of light. Subsequently, Einstein used de Broglie's hypothesis that all particles are waves. He immediately generalized Bose's treatment to massive particles [5]. But the number of massive particles is conserved (they cannot be created and destroyed), and that simply means that he had to introduce this additional parameter μ. It is called the chemical potential and ensures the conservation of particles.

9.4 Why Photons Don't Condense

I have explained the mathematical difference between Planck's black-body radiation law and the Bose–Einstein distribution. Now I will try to explain more intuitively why photons do not Bose condense. Why is there no Bose–Einstein condensate of photons? First let me explain how it is possible for us to get Bose–Einstein condensation for massive particles. We start with a gas at a certain temperature, T, then add more and more particles. Now, I have told you earlier that Bose–Einstein condensation occurs when the temperature is lowered. However, one can also simply add particles (at constant temperature) to reach the point where the de Broglie wave packets overlap. What this means for us is that once we exceed this critical number when we add particles, all the extra particles have to go into the Bose–Einstein condensate. The distribution of energies at the critical temperature (or at the critical number of atoms) is the Bose–Einstein distribution, with this chemical potential μ equal to zero. Therefore when the Bose–Einstein condensate is just about to form, the distribution is identical to the blackbody spectrum.

This makes it easy to see why the photons do not form a Bose–Einstein condensate (Fig. 9.6). We can do the same *gedanken* experiment with photons: what will happen if we take a cavity containing a blackbody radiation field and add extra photons? We know that if we have massive particles, they form a Bose–Einstein condensate. But in case of photons, there is a better way for Nature to accommodate those photons, not by forming a Bose condensate of photons but instead by simply absorbing the extra photons on the surface of the cavity! This will maximize entropy because it heats the walls of the cavity. If the walls were very shiny and the photons could equilibrate among themselves, then the photons could form a Bose condensate, but then we wouldn't have equilibrium between the blackbody radiation and the walls.

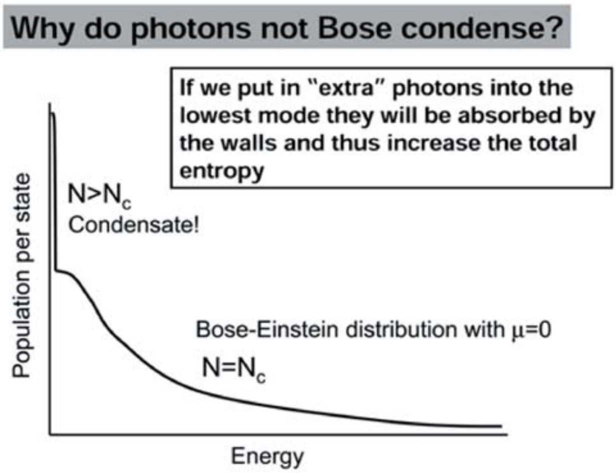

Fig. 9.6. There is no Bose–Einstein condensation of photons. Note that the graph shows the photon occupation number and therefore looks different from the more familiar black-body graph that shows energy density.

9.5 Bose–Einstein Condensation and Entropy

Now I want to make a few comments about randomness and entropy to avoid confusion. We simply counted the number of particles in the various energy levels, and obtained the distribution laws of quantum statistics as the most probable distribution. If you had 30 percent population in one state, then the value 30 percent reflects the number of microstates, or configurations, where a particle is in this particular state. Therefore we have maximized entropy once we have found the most probable configuration. I know that some of my colleagues initially were really puzzled by the fact that the Bose–Einstein condensate does not look like a random state. All the particles are marching in lockstep – it looks like regular motion! Nevertheless, if you put many atoms in your system and lower the temperature, the most random state of nature is to form this Bose–Einstein condensate, even if it appears counter-intuitive at first (Fig. 9.7). The randomness does not reside in the motion or location of the condensed particles. Instead, it is the numbers of atoms in the condensate and in the other states which fluctuate. When we say there is a certain number in the condensate, we mean on average. So the system still has fluctuation, and this is how Nature is trying out all possible microstates.

9.6 Absolute Indistinguishability

The counting argument, which is at the heart of Bose–Einstein condensation, assumed that all the little balls in our discussion were absolutely identical. If they were not, we could exchange two of them and get a different quantum state. In turns out that the assumption of indistinguishability can in fact be justified theoretically, but only in quantum field theory, where we can regard particles as excitations of a quantum field. Electrons everywhere in the world are excitations of the same field and therefore they are absolutely identical. But I should say, although we know that electrons are indistinguishable, physicists are always willing to spend great effort to test such assumptions. In the last few years, experimentalists have tried to do very stringent tests of questions like: what would happen if the bosons had a little *fermionic* content – if there were a small anti-symmetric contamination of the symmetric state? People have pushed those experiments to impressive limits, and I am sure that with every technological advance, experimentalists will push those limits even further.

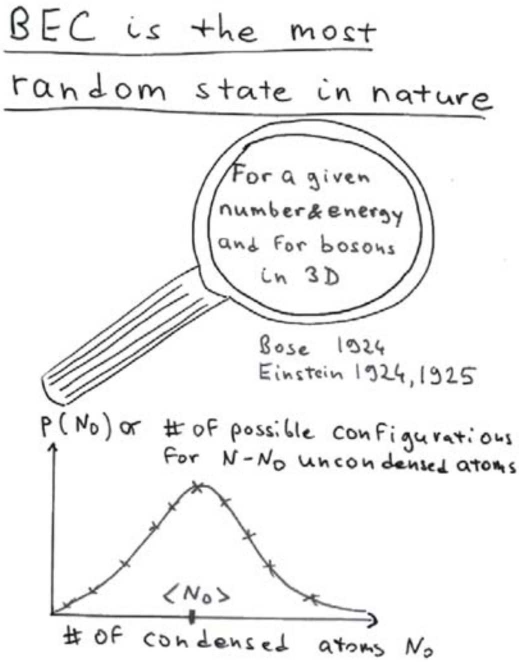

Fig. 9.7. The Bose–Einstein condensate maximizes entropy.

9.7 Some Historical Remarks

It is truly remarkable how quantum statistics has developed. Let me illustrate this with some examples. I have already mentioned that Bose's paper contained a derivation of Planck's radiation law, but he used photons and counted the number of states for photons. This was the beginning of modern quantum statistics (Fig. 9.8). Einstein, in three papers, applied the same concept to particles, the major difference being that the number of particles is conserved [6]. This has the spectacular consequence of the Bose–Einstein condensation. Bose's paper (though Bose was not aware of it) was radical in the sense that he broke with the statistical independence of particles [7]. What that means is that, if you have one particle in your system, and you add a second, then the second particle is not independent from the first. Its behavior depends on what the first one has done. Of course, one extreme example is the Pauli exclusion principle, but it was not known at the time of the papers of Bose and Einstein.

I find it amusing to read those old papers and figure out what the authors regarded as examples for these new quantum statistics. Einstein, in his 1925 paper, mentioned hydrogen, helium, and the electron gas as possible candidates for Bose–Einstein condensation. There was controversy as to which particles obey which statistics. Then in 1925 the Pauli exclusion principle was formulated, and one year later Fermi–Dirac statistics. Pauli and Dirac were confused for a while in thinking that all the massive particles in the world were fermions. Dirac wrote: "The solution with symmetrical eigenfunctions must be the correct one when applied to light quanta, since it is known that the Einstein–Bose statistical mechanics leads to Planck's law of black-body radiation. The solution with antisymmetrical eigenfunctions, though, is probably the correct one for gas molecules, since it is known to be the correct one for electrons in an atom, and one would expect molecules to resemble electrons more closely than light quanta." [8]

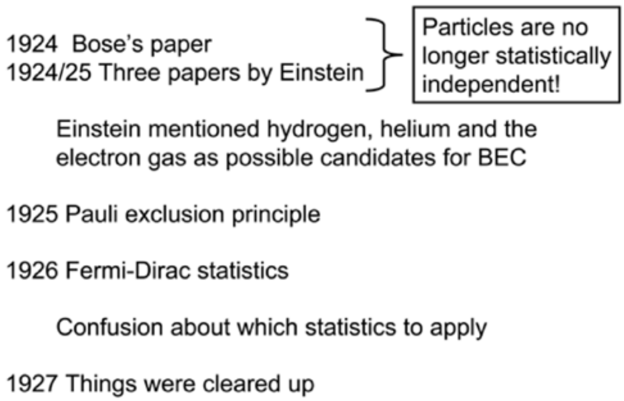

Fig. 9.8. Development of quantum statistics, 1924–1927

Pauli said: "We shall take the point of view also advocated by Dirac, that the Fermi, and not the Einstein–Bose, statistics applies to the material gas." [9] If he had been right, Bose–Einstein condensation would never have been observed. We know now that massive particles can be either bosons or fermions, depending whether they consist of either an even or odd number of protons, electrons, and neutrons. It was already in the following year that the situation became clear and people knew how to apply the two different kinds of statistics. These were dramatic years for quantum statistics.

Let me mention another aspect that delayed the acceptance of Einstein's ideas about Bose–Einstein condensation. Einstein demonstrated that almost all of the particles form this giant matter wave. Mathematically, it involved a singularity – the denominator of the Bose–Einstein distribution function becomes zero. To Einstein and others, this result looked like a mathematical oddity, which may have nothing to do with reality. Einstein said in December 1924: "The theory is pretty, but is there also some truth to it?" [10] Actually, a few years later, people showed mathematically that a singularity as predicted by Einstein cannot happen in any finite system. At this point, people did not understand the notion of phase transitions and the thermodynamic limit, so they dismissed the idea of Bose–Einstein condensation as a mathematical artifact. That lasted until 1937. It then became clear that Bose–Einstein condensation is a real phase transition, which should be observable and Fritz London postulated correctly that it had been realized in the superfluidity of helium [11].

9.8 What are the Conditions for Observing Bose–Einstein Condensation?

What is necessary for observing Bose–Einstein condensation? I mentioned earlier that the intuitive criterion is that the distance between the particles in the gas be comparable to their de Broglie wavelength. This can be achieved by cooling the gas at constant density. Take an example. If you cool down a substance of the density of water, the transition temperature would be around 1 K, which is fairly easy to reach. However, when you cool down water to such low temperatures, it freezes into a solid and there is no chance of observing Bose–Einstein condensation in a gas because particles are localized in a solid and not delocalized as required for BEC. The only form of matter that stays liquid at such low temperatures is liquid helium, and indeed, liquid helium becomes superfluid at 2.2 Kelvin. This was a strong indication that superfluidity is related to Bose–Einstein condensation. People thought for a long time that it would never be possible to accomplish Bose–Einstein condensation in a gas. Of course, it is nice for me as an experimentalist that I have done something that people thought was impossible to do. It also teaches us something about physics when we go through the argument.

Schrödinger, in 1952, in a textbook on statistical thermodynamics, was skeptical about the possibility of observing departures from classical statistics separate from effects of interactions between particles. "The densities are so high and the temperature so low – those required to exhibit a noticeable departure – that the van der Waals corrections are bound to coalesce with the possible effects of degeneration, and there is little prospect of ever being able to separate the two kinds of effect." [12]

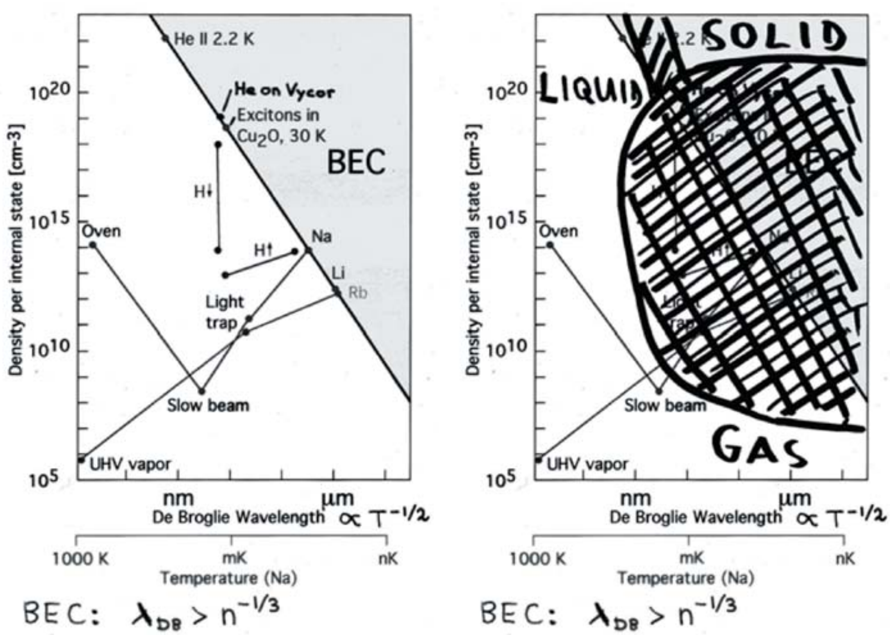

Fig. 9.9. Phase diagram for ideal gases (left) and real matter (right). The hatched area represents regions of temperature and density, which are usually not realized in nature. For example, at low temperature, the density of the gaseous phase is limited by the vapor pressure. Adding more gas to a fixed volume does not create a higher density gas, but a liquid or solid phase.

In other words, people expected that the system would simply freeze to a solid before it could form a gaseous Bose–Einstein condensate. Fig. 9.9 shows a phase diagram for an ideal gas and includes the line marking the phase transition to Bose–Einstein condensation, which has by now been achieved by many groups. It shows the relation I mentioned earlier, density versus temperature: the lower the density, the colder the temperature has to be for Bose–Einstein condensation. Notice that the scale goes down to nanokelvin temperatures. But this is for an ideal gas. If you look at real substances at low temperatures, they are rock-solid. If you have, for instance, some frozen substances at millikelvin temperatures, the vapor pressure is extremely small and the density of the vapor is orders of magnitude too small for BEC. All this holy land where we do all our experiments now seemed to be completely out of reach because of the properties of matter. In the standard phase diagram that shows the different states of matter, gas, liquid, solid, there is simply no place for BEC.

So what is the way out of it? The solution is that, in fact, we do not need full thermodynamic stability. We can use metastability. So the way that led to success sounds almost crazy: let's not work with something of the density of water, let's work with something that is a billion times more dilute, one hundred thousand times thinner than air (Fig. 9.10). It really is a *very* dilute gas. The consequence for such a dilute gas is that it takes a long time before two atoms find each other, form molecules, form clusters, form solids. We have, for a certain time, a super-cooled metastable atomic gas. We can take it down to extremely low temperatures and it is still a gas. Finally, after tens of seconds, a dilute sodium gas will realize it shouldn't be a gas, it should be a clump of metal, and this limits the lifetime of the Bose condensate. Ten seconds or so is very long to do all kinds of studies.

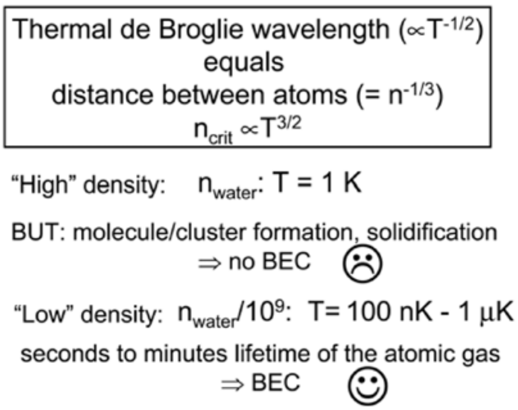

Fig. 9.10. Criterion for Bose–Einstein condensation

9.9 How to Cool to Bose–Einstein Condensation

The price we had to pay in our experiments for this long metastability at low density is that the BEC phase transition occurred at extremely low temperatures. To reach those temperatures, we had to find new cooling methods, methods that are now producing the coldest temperatures that have ever been achieved or observed by anybody on earth.

We start at room temperature or above and reduce the temperature by nine orders of magnitude. This is done by a combination of different cooling schemes. The first is laser cooling. If you shine laser light on atoms, and play some tricks, then the scattered, or emitted, light is more energetic than the absorbed light. The scattered light carries away the energy of the atoms and the atoms get colder and colder, and you can easily reach microkelvin temperatures with this method. But it turned out that we could not cool all the way down to Bose–Einstein condensation this way. Rather, we had to do something else after the laser cooling. First, we levitated the atoms in magnetic fields. This magnetic confinement is like a perfect thermos, it keeps the gas away from the warm walls of the surrounding vacuum chamber. The cooling method that got us to BEC is evaporative cooling. This is easy to explain: if you sit in a bathtub, the water gets colder and colder. Why does the water get colder? It is because the most energetic molecules escape from the liquid water as steam. What remain behind are the less energetic, colder molecules. We do the same in an atom trap. The hottest atoms jump out of the trap and what is left behind becomes colder and colder.

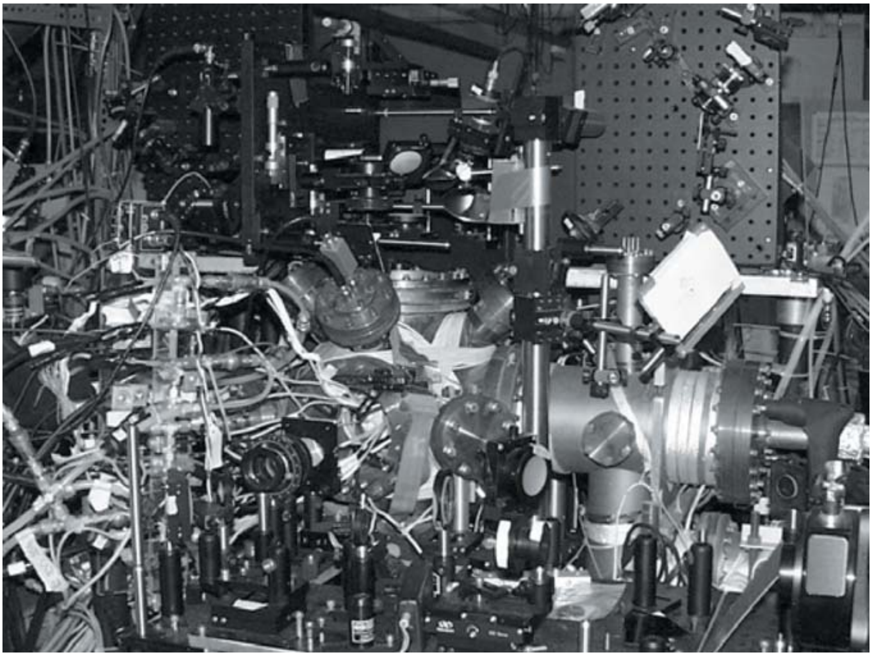

Fig. 9.11. Apparatus for studying Bose–Einstein condensates at MIT

The atom trap is inside an ultrahigh-vacuum chamber. We don't use any cryogenic cooling. (Actually, we do use liquid nitrogen to improve the vacuum, but it is not really essential). In the center of the chamber is a small sample, which is matter at the coldest temperatures ever achieved (Fig. 9.11). We have lots of viewports and we can look through them with laser beams to figure out what is happening inside. Many of our observations are simply photographs. We create our cold sample of atoms, we shine light on it, then we simply take a picture of the scattered light or of the shadow cast by the sample.

9.10 Observation of Bose–Einstein Condensation

I want to come back to the discussion at the beginning where I described the Bose–Einstein condensation as an identity crisis for particles. How do we tell if this identity crisis for bosons has really happened? To see the manifestation of the indistinguishability, we find out if all particles or most of them are populating the lowest energy state. To do this we must measure the energy of the gas. If I give you a container filled with gas and ask you to find the temperature, an easy way would be to punch a hole in the container and let the gas stream out. A measurement of the velocity is equivalent to a temperature measurement, because temperature is a measure of kinetic energy. In our experiments, the magnetic atom trap is the container. Instead of punching a hole, we remove the container completely by switching off the magnets. This allows the gas to spread out in all directions. The lower the temperature, the slower the gas spreads out. But then, at the critical temperature for BEC, all of a sudden there is a component that almost doesn't spread out. This is the Bose–Einstein condensate. The essential idea is that in the Bose–Einstein condensate all the particles are in the same state, so there is no distribution of velocities, and thus no tendency to spread out. This is a direct observation of Bose–Einstein condensation (Fig. 9.12). Actually, zero-point motion and repulsive interactions cause some spreading, which is, however, much less than for the thermal component.

Another way to measure temperature involves measuring the size of the confined cloud. Take the earth's atmosphere, for example. Its thickness is of order 10 kilometers. Indeed, when you climb one of the highest mountains on earth (which are about 8 km high), then the air gets noticeably thinner. This is the height of the atmosphere at around 300 Kelvin. But now assume that we cool down to 300 microkelvin. The whole atmosphere would be a layer one centimeter high

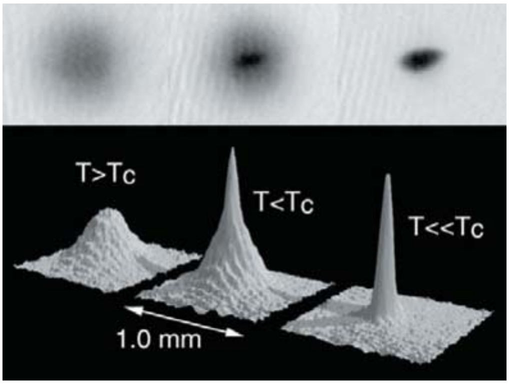

Fig. 9.12. Observation of Bose–Einstein condensation by absorption imaging. The upper row of images shows the original shadow pictures, the lower row shows absorption vs. two spatial dimensions. The Bose–Einstein condensate is characterized by its slow expansion observed 6 ms after the atom trap was turned off. The left picture shows an expanding cloud cooled to just above the transition point; middle: just after the condensate appeared; right: after further evaporative cooling has left an almost pure condensate. The total number of atoms at the phase transition is about 7×10^5, the temperature at the transition point is 2 μK.

(Fig. 9.13). The temperatures of our experiments are much colder, in the nanokelvin range, so gravity would crunch the gas together to just a few micrometers. What we have just learned is that the height of a gas layer in the gravitational field of the earth decreases with temperature. Similarly, the size of a gas cloud held by magnetic forces in an atom trap shrinks with colder temperatures. Therefore, the size of an atom cloud in our magnetic container is an absolute thermometer. We can measure nanokelvin temperatures simply with a ruler.

Using light scattering techniques, the shrinking of the cloud and the sudden formation of the condensate can be directly observed (Fig. 9.14). When you cool down, then all of a sudden there appears what looks like a liquid droplet, which has condensed out of a saturated vapor. It may look like a liquid droplet, but in fact it is a gas, a quantum gas: it is the Bose–Einstein condensate.

9.11 A Bose–Einstein Condensate Has a Macroscopic Wavefunction

The Bose–Einstein condensate gives us a way to observe the quantum mechanical wave function because there is now a macroscopic popu-

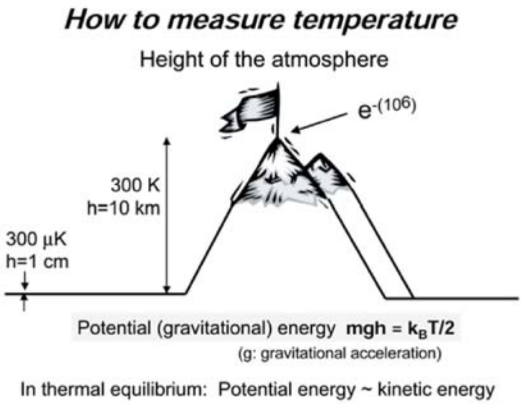

Fig. 9.13. The height of the atmosphere is proportional to the (absolute) temperature.

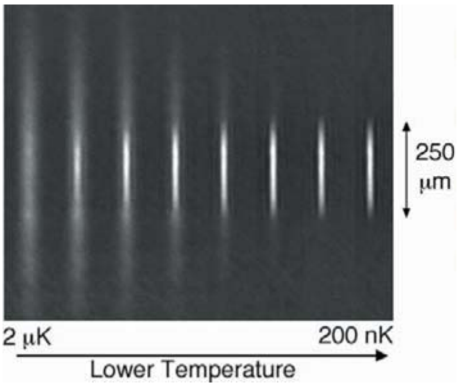

Fig. 9.14. Phase contrast images of trapped Bose gases across the BEC phase transition. At high temperature, above the BEC transition temperature, the density profile of the gas is smooth. As the temperature drops below the BEC phase transition, a high-density core of atoms appears in the center of the distribution (shown in white). This is the Bose–Einstein condensate. Lowering the temperature further, the number of atoms in the condensate grows and the thermal wings of the distribution become shorter. Finally, the temperature drops to the point where a pure condensate with no discernible thermal fraction remains.

lation of atoms (millions of them) in a single state. Since the wave function represents the probability of a particle being at a certain location, the shadow cast by the condensate is a projection of the (absolute square of the) wavefunction into the plane of the camera sensor. A sequence of photographs of these shadows is a direct record of the time evolution of the wave function, and therefore gives us insight into the workings of quantum mechanics and the Schrödinger equation. Of course our observation affects some particles, but even if we knock a few particles out of the condensate, there are still millions left and we can continue our observation. For me personally, such observations have given the concept of a quantum mechanical wave function new meaning. The wave function is no longer a merely computational quantity. It has an observable reality. By the way, shadow pictures heat up the condensate, because the photon is absorbed and transfers its recoil to the atoms. We therefore often use a different imaging technique, called phase-contrast imaging, which relies on small-angle light scattering (see Fig. 9.14 and 9.15).

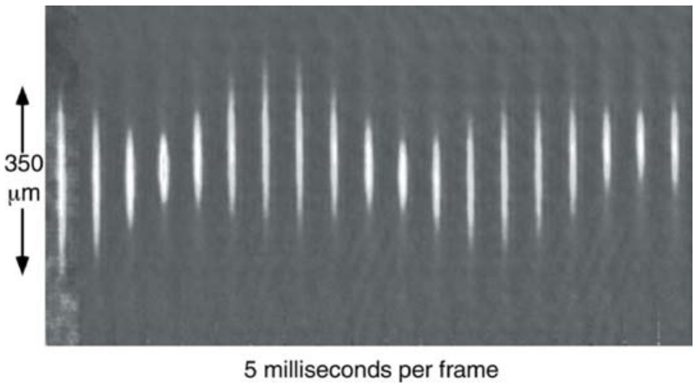

Fig. 9.15. Phase contrast images of the quadrupole-type shape oscillation. In this mode, the radial width and the axial length of the condensate oscillate out of phase. In addition, the condensate oscillates in the trap (up and down in the picture). The images were taken of a single condensate at a rate of 200 frames per second. From this data, the frequency of the shape oscillation was determined to be 30 Hz. Figure reprinted with permission from D. M. Stamper-Kurn, H.-J. Miesner, S. Inouye, M. R. Andrews, and W. Ketterle, "Collisionless and hydrodynamic excitations of a Bose–Einstein condensate," Physical Review Letters **81**, 500–503 (1998). ©1998 by the American Physical Society

9.12 Atom Lasers and Atom Amplification

Finally, consider the analogy between light and particles – the symmetry between photons and electromagnetic waves on the one side and massive particles and matter waves on the other. The analogy suggests we ought to be able to build an atom laser. Now, what do I mean by this? For an optical laser, you have a single electromagnetic wave that bounces back and forth in an optical resonator that consists of two mirrors. We can do the same thing with matter waves. Our matter-wave mirrors are magnetic mirrors. The magnetic coils produce a confining potential, and the matter wave is reflected back and forth. This leads to a standing wave in a matter-wave resonator, in complete analogy to the optical resonator for the optical laser.

In addition to the resonator, we also need an amplification process. You all know how it works for an optical laser: some light is sent through an inverted medium and stimulates the emission of additional photons. This is the principle of optical amplification (Fig. 9.16). You can do the same with matter waves. If you take a coherent matter wave and pass it through a reservoir of atoms, then by the same stimulation, which gives rise to the optical laser, you can amplify the matter wave.

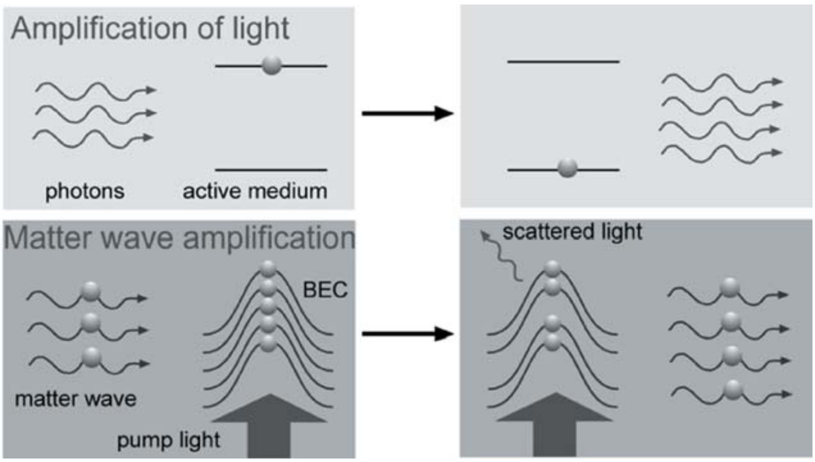

Fig. 9.16. Amplification of light and atoms: In the optical laser, light is amplified by passing it through an excited inverted medium. In the MIT atom amplifier, an input matter wave is sent through a Bose–Einstein condensate illuminated by laser light. Bosonic stimulation by the input atoms causes light to be scattered by the condensate exactly at the angle at which a recoiling condensate atoms joins the input matter wave and augments it.

In our recent demonstration of matter-wave amplification, we took some input atoms and passed them through a Bose–Einstein condensate [13]. First, nothing happened to them. They just came out. Condensate atoms are at rest and cannot augment the moving input wave without violating the law of momentum conservation. We added momentum in the form of photons by illuminating the whole scene with laser light. So now, an atom in the Bose condensate can say, "Well, if I scatter one of those photons at a certain angle, I just receive the correct recoil to amplify the input atoms." And this is what happened (Fig. 9.17).

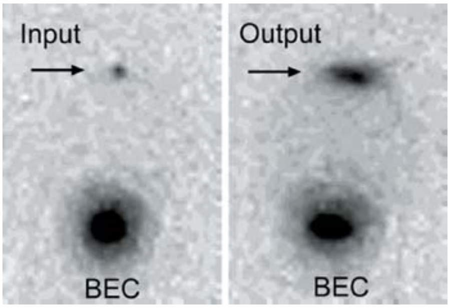

Fig. 9.17. Observation of atom amplification. Atom amplification is probed by sending an input beam through the atom amplifier, which is a Bose–Einstein condensate (BEC) illuminated with laser light. On the left side, the input beam has passed through the condensate without amplification. 20 ms later, a shadow picture is taken of the condensate and the input atoms. During this time of ballistic expansion, the input atoms have moved away from the condensate. When the amplification process was activated by illuminating the condensate with laser light, the output pulse contained many more atoms than the input pulse – typical amplification factors were between 10 and 100. The field of view is 1.9 mm $\times$ 2.6 mm.

In a laser, all photons are the same, they are coherent, they are one big wave. The coherence of light is usually demonstrated by showing the interference pattern of two light beams: when two beams of coherent light overlap on a screen, they produce a pattern of dark and bright lines, interference fringes. This is direct proof for the wave nature of light. The dark areas are created by the destructive interference of a crest and a trough of the two waves. To demonstrate that the atoms in a Bose condensate are coherent we used the following trick. We cut the condensate in half, thus creating two condensates. The goal was to overlap the two and observe the interference of two waves. This was done by switching off the trap. Then the two condensates fell down, spread out and overlapped (Fig. 9.18). The shadow picture of the two overlapping condensates shows strong shadows where the two wave functions are in phase, and no shadow where the wave functions are out of phase. Those areas of destructive interference show that matter combined with matter can result in no matter!

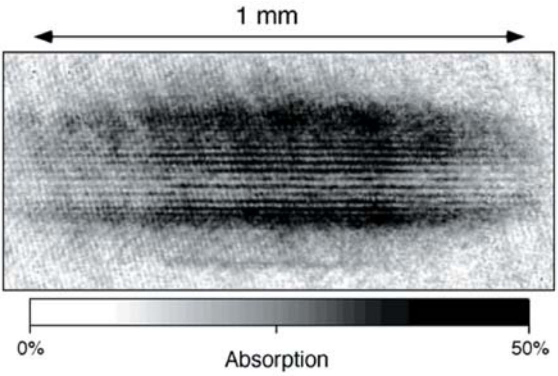

Fig. 9.18. Interference pattern of two expanding condensates observed 40 ms after the atoms were released from the trap. The width of the absorption image is 1.1 mm. The interference fringes have a spacing of 15 μm and are strong evidence for the long-range coherence of Bose-Einstein condensates. Figure reprinted with permission from M. R. Andrews, C. G. Townsend, H.-J. Miesner, D. S. Durfee, D. M. Kurn, and W. Ketterle, "Observation of interference between two Bose condensates," Science **275**, 637-641 (1997). ©1997 AAAS

The observation of interference fringes is a demonstration that matter has wave properties. Matter always has wave properties, but usually, the de Broglie wavelength is too small for us to observe wave phenomena. At room temperature, the de Broglie wavelength of atoms is smaller than the diameter of an atom. However, at the low temperatures (or energies) of our experiment, it has become macroscopic, of order 30 micrometers, much longer than the wavelength of light, and can therefore be photographed using visible light.

9.13 Outlook

Einstein's concept of a new condensation phenomenon in an ideal gas is no longer just a *gedanken* experiment: it has been realized in the laboratory. The realization of Bose–Einstein condensates has sparked many

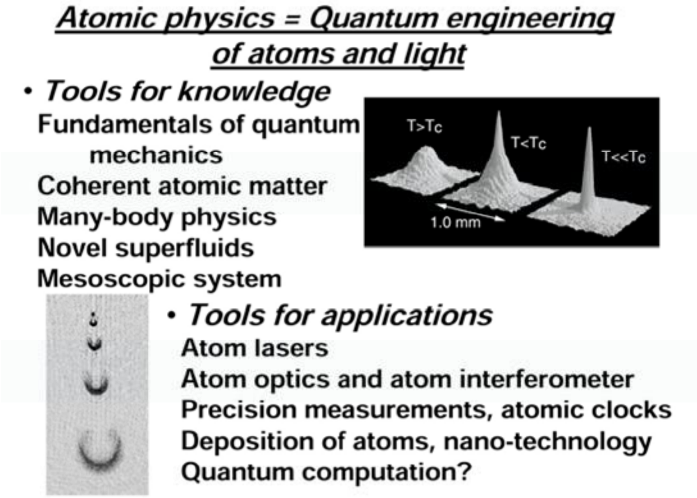

Fig. 9.19. Applications of Bose–Einstein condensation

activities in different areas including atomic physics, quantum optics, many-body physics, and precision measurements. I am an experimentalist, and regard myself as a quantum engineer. I want to engineer things out of light and atoms that have not existed before: this leads to tools, tools for uncovering new knowledge and tools for new applications (Fig. 9.19). We can expect that this unprecedented control over atoms will lead to applications in atom optics, precision atom interferometry, metrology (such as atomic clocks), nanotechnology, and perhaps quantum computation. Already now has the Bose–Einstein condensate allowed us to learn new and fascinating properties of ultracold matter, and there is more excitement to come.

Acknowledgments

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10

Quantum Fluctuations of Light: A Modern Perspective on Wave/Particle Duality

Howard Carmichael

10.1 Introduction

It was in October of 1900 that Max Planck stated his law for the spectrum of blackbody radiation for the first time publicly [1], and it was in December of that year that he presented a derivation of that law in a paper to the German Physical Society [2] in which he states: "We consider, however – this is the most essential point of the whole calculation – E to be composed of a very definite number of equal parts and use thereto the constant of nature $h = 6.55 \times 10^{-27}$ erg sec." Planck introduced his "equal parts" so that he might apply Boltzmann's statistical ideas to calculate an entropy. The ideas required that he make a finite enumeration of states and hence the discretization was necessitated by the statistical approach. The surprise for physics is that fitting the data required the discreteness to be kept while the more natural thing would be to take the size of the "parts" to zero at the end of the calculation.

The story of Planck's discovery and what may or may not have been his attitude to the physical significance of the persisting discreteness is one to be told by others [3]. But, looking backwards with the knowledge of physicists trained in the modern era, we see that the essence of the blackbody calculation is remarkably simple and that it provides a dramatic illustration of the profound difference that can arise from summing things discretely instead of continuously – i.e., making an integration. Mathematically, the difference is almost trivial, but why the physical world prefers a sum over an integral still escapes our understanding. Going, then, to the heart of the matter, the solution to the blackbody problem may be developed from a calculation of the average energy of a harmonic oscillator of frequency ν in thermal equilibrium at temperature T. In the case of a continuous energy variable $y = E/h\nu$, the average value $\bar{y}$ is obtained from the following calculation:

$$\bar{y} \equiv \frac{\int_0^\infty y\mathrm{e}^{-yx}dy}{\int_0^\infty \mathrm{e}^{-yx}dy} = -\frac{(1/x)'}{1/x} = \frac{1}{x}, \tag{10.1}$$

where we define

$$x \equiv \frac{h\nu}{kT}, \tag{10.2}$$

and k is Boltzmann's constant ($'$ denotes differentiation with respect to x). The result is the one expected from the classical equipartition theorem. But taking a discrete energy variable $E_n = nh\nu$ leads to the average value $\bar{n}$:

$$\bar{n} \equiv \frac{\sum_{n=0}^\infty n\mathrm{e}^{-nx}}{\sum_{n=0}^\infty \mathrm{e}^{-nx}} = -\frac{[1/(1-\mathrm{e}^{-x})]'}{1/(1-\mathrm{e}^{-x})} = \frac{1}{\mathrm{e}^{x}-1}. \tag{10.3}$$

There is agreement between the sum and the integral for $x \ll 1$, when the average energy is made up from very many of Planck's "equal parts;" this is the domain of validity of the Rayleigh–Jeans formula. Outside this domain discreteness brings about Planck's quantum correction.

The pure formality of the difference is striking. The blackbody problem hardly demands that we take the energy quantum $h\nu$ too seriously, whether the oscillator be considered to be a material oscillator or a mode of the radiation field [4]. Indeed, summed over oscillators of all frequencies, the total energy is in effect continuous still. And speaking of the radiation oscillators: it has, in fact, only recently become possible to make a direct observation of discrete single-mode energies. Remarkably, measurements are made at microwave frequencies where the energy quantum is exceedingly small. The feat is accomplished using the strong dipole interaction between an atom excited to a Rydberg state and a mode of a superconducting microwave cavity cooled near absolute zero [5]; the system has become a paradigm for studies in cavity quantum electrodynamics (QED) [6]. In this chapter I will discuss other results from the interesting field of cavity QED, but for a physical system in which the material and radiation oscillators have optical frequencies [7].

Equations (10.1) and (10.3) contrast the discrete with the continuous. Of course, concepts of both characters enter into classical physics: Newton's mass point is discrete, the particle or atom is discrete, as are any "things" counted. On the other hand, the evolution over time unfolds continuously, the *location* of a particle lies in a continuum, Maxwell's waves are continuous. The important point about classical thinking is that ideas on the two sides remain apart from one another, even if they have sometimes competed, as, for example, in the varied attempts to account for the nature of light. Quantum physics on the other hand, as it developed in the three decades after Planck's discovery, found a need for an uncomfortable fusion of the discrete and the continuous. Arguments about particles *or* waves gave way to a recognized need for particles *and* waves. Thus, throughout the period of the old quantum theory, from Planck until Heisenberg [8] and Schrödinger [9], a genuine "wave/particle duality" steadily emerged. The full history is complex [10] and I will mention only some of the most often quoted highlights.

Einstein, in a series of celebrated papers, laid down the important markers on the particle side [11]. Amongst other things, he brought Planck's quantum into clear focus as a possible particle of light [12], argued that discreteness was essential to Planck's derivation of the radiation law [13], and incorporated the quantum and its discreteness into a quantum dynamics that accounted for the exchange of energy between radiation and matter oscillators in a manner consistent with that law [14]. Adding to this, Bohr's work connecting Planck's ideas to fundamental atomic structure must be seen to support an argument on the particle side [15]. Yet Bohr, like most others, was opposed to Einstein's tinkering with the conventional description of the free radiation field as a continuous wave.

The case on the wave side was easily made on the basis of interference phenomena. Nonetheless, over time it became clear that the particle idea could not simply be dismissed and it was suggested that the clue to a union lay, not in the nature of free radiation, but in the nature of the interaction of radiation with matter. Planck was among those to expressed this view: "I believe one should first try to move the whole difficulty of the quantum theory to the domain of the interaction of matter with radiation." [16] The suggestion was followed up most seriously in a bold work authored by Bohr, Kramers, and Slater (BKS) [17] and based on a proposal of Slater's [18]. The key element was not a part of Slater's original proposal, however, which had waves – a "virtual radiation field" – guiding light particles [19]. For there were no light particles. The virtual waves comprised the entire radiation field, radiated *continuously* by virtual material oscillators. The response of the matter to the continuous radiation obeyed a quite different rule though; following Einstein's ideas on stimulated emission and absorption [20], the wave amplitude was to determine probabilities for *discrete* transitions (quantum jumps) between stationary states [21]. The aim was to retain both the continuity of Maxwell waves and the discreteness of quantized matter by confining each to its own domain.

There was a price to be paid for preserving the apartness, however. The BKS scheme was noncausal (stochastic) at a fundamental level and although energy and momentum were conserved on average, they would not be conserved by individual quantum events. Statistical energy conservation had been considered before; Einstein was one of those who had toyed with the idea [22]. BKS cast the idea in a concrete form with predictions that would be tested within less than a year. Their proposal was not entirely misguided; we meet with a "virtual" radiation field – though mathematically more sophisticated – in modern field theory. The fatal weakness was that their scheme did not causally connect the downward jump of an emitting atom with the subsequent upward jump of a particular absorbing atom. Direct correlation between quantum events was therefore excluded, yet correlation was just what the X-ray experiments of Bothe and Geiger [23] and Compton and Simon [24] revealed.

Quantum optics took up the theme of correlations between quantum events in the 1970s, as lasers began to be used for investigating the properties of light. In this chapter we will review a little of what has grown from those beginnings. Only a small piece of the history is covered since the main story I want to tell is about a particular experiment in cavity QED performed by Foster et al. [25]. The experiment uncovers the tensions raised by wave/particle duality in a unique way, by detecting light as both particle and wave, correlating the measured wave property (radiation field amplitude) with the particle detection (photoelectric count). Thus, light is observed directly in both its character roles, something that has not been achieved in a single experiment before.

We will work up to the new results gradually. We begin with an updated statement of the BKS idea (Sect. 10.2) which we use as a criterion to define what we mean when speaking of the nonclassicality of light. I will then say a few things about the cavity QED light source and what it is about cavity QED that makes its fluctuations of special interest (Sect. 10.3). I first discuss the fluctuations in their separate particle and wave aspects: photon antibunching, seen if one correlates particle with particle (Sect. 10.4), is contrasted with quadrature squeezing which is seen if one correlates wave (amplitude) with wave (Sect. 10.5). Individually, photon antibunching and quadrature squeezing each show light to be nonclassical by our criterion. Each may be explained however by modeling light as either purely particle or wave. Finally, I will describe the wave–particle correlations measured by Foster et al. (Sect. 10.6), where neither conception alone can explain what is observed.

10.2 A Criterion for Nonclassicality

Although there are still a few contrary voices, the opinion amongst physicists generally is that light – electromagnetic radiation at optical frequencies – must be quantized, with the introduction of Einstein's light particle, in order to account for the full range of observable optical phenomena. Einstein stated his view that something of the sort might be the case in the introduction to his 1905 paper, where he writes: "One should, however, bear in mind that optical observations refer to time averages and not to instantaneous values and notwithstanding the complete experimental verification of the theory of diffraction, reflexion, refraction, dispersion, and so on, it is quite conceivable that a theory of light involving the use of continuous functions in space will lead to contradictions with experience, if it is applied to the phenomena of the creation and conversion of light." [26]

Einstein identified specifically "the phenomena of the creation and conversion of light" as the point where contradictions might be found. Considering modern quantum optics experiments, it is indeed to the "conversion" or, more precisely, detection of the light that we look to define what is, or is not, a failure of the classical wave theory. Light is detected through the photoelectric effect where it is responsible, through some process of conversion, for the appearance of countable events – i.e, the production of photoelectric pulses. If the light is to be a continuous wave, it interfaces awkwardly with the discreteness of the countable events. The BKS attempt at an interface is nevertheless remarkably successful in accounting for the action of the light from most sources on a detector. It is therefore commonly adopted as a criterion, or test, for those phenomena that truly contradict classical ideas. It is adopted in the spirit of Bohr's comment to Geiger after he had learnt of Geiger's new X-ray results: "I was completely prepared [for the news] that our proposed point of view on the independence of the quantum process in separated atoms should turn out to be incorrect. The whole thing was more an expression of an attempt to achieve as great as possible application of classical concepts, rather than a completed theory." [27]

Fig. 10.1 illustrates the BKS interface as it is applied to the photoelectric detection of light. On the left, the light is described by a

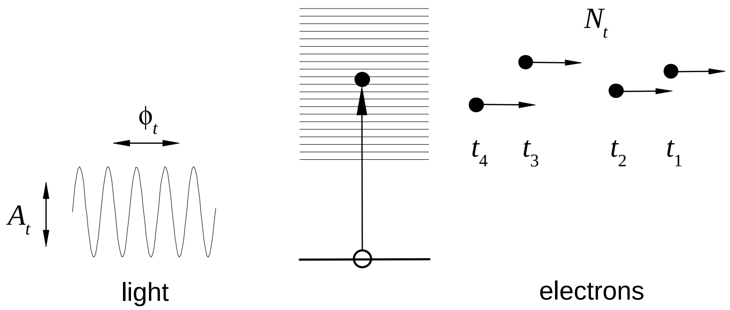


Fig. 10.1. Semiclassical photoelectric detection of quasi-monochromatic light couples a discrete stochastic process N_t (photoelectron counting sequence) to a continuous stochastic process $2A_t \cos(\omega_0 t + \phi_t)$ (classical electromagnetic field) through random detection events occuring, at time t, at the rate A_t^2.

continuous wave, specified at the position of the detector by an electric field, $2A_t \cos(\omega_0 t + \phi_t)$, whose amplitude A_t and phase ϕ_t are generally fluctuating quantities (random variables at each time t). We consider the fluctuations to be slow compared to the frequency ω_0 of the carrier wave; thus, although the light has nonzero bandwidth, it is still quasi-monochromatic. On the other side of the figure, the sequence of photoelectrons is discrete. Photoelectrons are produced at times $t_1, t_2, \ldots$, with some number N_t of them generated up to the time t. The difficulty is to interface the continuity on the left with the discreteness on the right. This is done by allowing the amplitude of the wave to determine the "instantaneous" rate at which the random photoelectric detection events occur. With a suitable choice of units for A_t, the probability per unit time for a bound electron to be released between t and $t + dt$ is given by the local time average of the light intensity, A_t^2.

The issue now is whether or not this model can account for what one observes with real photoelectric detectors and light sources. Specifically, is it always possible to choose a continuous stochastic process (A_t, ϕ_t) such that, in their statistical properties (correlations), the observed photoelectric detection sequences which constitute the experimental data can, in fact, be produced through the suggested rule? The short answer, as we would expect, is that it is not always possible to do so. On the other hand, for most light sources the BKS rule works just fine. It has actually been quite an experimental challenge to produce light for which the rule fails.

10.3 Light Sources and Their Fluctuations

To start out we might ask how blackbody radiation fares. By filtering a thermal source, such as a spectroscopic lamp, it is possible to produce quasi-monochromatic blackbody radiation. Consider then the fluctuations of such a light source. Let us calculate the variance of the quasimode energy as Einstein first did in 1909 [28]. The continuous variable approach of (10.1) makes the calculation appropriate to classical waves. Here we need the mean value of y^2, given by

$$\overline{y^2} = \frac{\int_0^\infty y^2 \mathrm{e}^{-yx} dy}{\int_0^\infty \mathrm{e}^{-yx} dy} = \frac{(1/x)''}{1/x} = 2\bar{y}^2, \tag{10.4}$$

where $\bar{y}$ is the average energy, $1/x$. For the variance we therefore obtain

$$\overline{\Delta y^2} \equiv \overline{y^2} - \bar{y}^2 = \bar{y}^2. \tag{10.5}$$

Alternatively, in the discrete variable approach of (10.3) we make a summation to obtain

$$\overline{n^2} = \frac{\sum_{n=0}^\infty n^2 \mathrm{e}^{-nx}}{\sum_{n=0}^\infty \mathrm{e}^{-nx}} = \frac{[1/(1-\mathrm{e}^{-x})]''}{1/(1-\mathrm{e}^{-x})} = 2\bar{n}^2 + \bar{n}, \tag{10.6}$$

where $\bar{n} = 1/(\mathrm{e}^x - 1)$ is the average energy. In this case we obtain for the variance

$$\overline{\Delta n^2} \equiv \overline{n^2} - \bar{n}^2 = \bar{n}^2 + \bar{n}. \tag{10.7}$$

Equation (10.7) is the result obtained by Einstein and taken by him as evidence that the theory of light would eventually evolve into “a kind of fusion” of wave and particle ideas: light possess both a wave character, which gives the $\bar{n}^2$, and a particle character, which gives the $\bar{n}$.

It would appear, then, that the detection of blackbody radiation would be incorrectly described by the scheme of Fig. 10.1, since there the amplitude and energy of the light wave is continuously distributed, which should lead to (10.5), the incorrect result. This, however, is not the case at all; thermal light fluctuations do not meet our criterion for nonclassicality. In fact BKS made an attempt at the needed “fusion”. They did not eliminate particles to favor waves. They attempted only to keep the particles and waves separate. The separation recovers the two terms of (10.7) from two distinct (independent) levels of randomness. To see this we must identify the integer n, not with the free radiation field, but with the number of photoelectrons counted in a *measurement* of the field energy. The first term, the wave-like term in (10.7), is then recovered from the randomness of the field amplitude A_t, just as in

(10.5), while the second particle-like term is recovered from the additional randomness of the photoelectron counting sequence introduced by the rule governing the production of photoelectrons. Even if A_t fluctuates not at all, the photoelectron number will still fluctuate. It will be Poisson distributed. The second term of (10.7) is recovered as the variance of the Poisson distribution (which equals its mean).

Laser light is a good approximation to the ideal, coherent Maxwell wave which produces only the Poisson fluctuations generated in the detection process. Of course, once one has a laser, one can make a whole range of fluctuating light sources by imposing noisy modulations of one sort or another. So long, however, as the fluctuations are imposed, and thus independent of the randomness introduced in generating the photoelectrons, nothing more regular than a Poisson photoelectron stream will be seen. Here, then, is the Achilles heel of the BKS approach; it permits only super-Poissonian photoelectron count fluctuations. Once again, the limitation involves a discounting of correlations at the level of single quantum events. To illustrate, imagine a light source in which the emitting atoms make their quantum jumps from higher to lower energy at perfectly regular intervals. In the particle view, the source sends out a regular stream of photons, which, supposing efficient detection, yields a regular, temporally correlated, photoelectron stream. Such a photoelectron stream is impossible in the BKS view; its observation would meet our criterion for nonclassicality.

Any experimental search for the disallowed correlations must begin with a method for engineering light's fluctuations on the scale of Planck's energy quantum. What one can do is begin with laser light and scatter it, through some material interaction, to produce light that fluctuates in an intrinsically quantum mechanical way. Coherent scattering is of no use, since it looks just like the laser light – neither is incoherent scattering in which the fluctuations simply arise from noisy modulations. The fluctuations must be caused by the "quantum jumpiness" in the matter; the experiment must be sensitive enough to see the effects of individual quantum events. This is rather a tall order, since if we have in mind scattering from a sample of atoms, say, the effect, on the fluctuations, of what any one atom is doing is generally very small. Happily, cavity QED comes to our aid.

The light source used in the experiment I wish to discuss is illustrated in Fig. 10.2. Basically, a beam of coherent light is passed through a dilute atomic beam – at right angles to minimize the Doppler effect. The light is resonant with a transition in the atoms, which make their "jumps" up and down while scattering some of the light, and hence

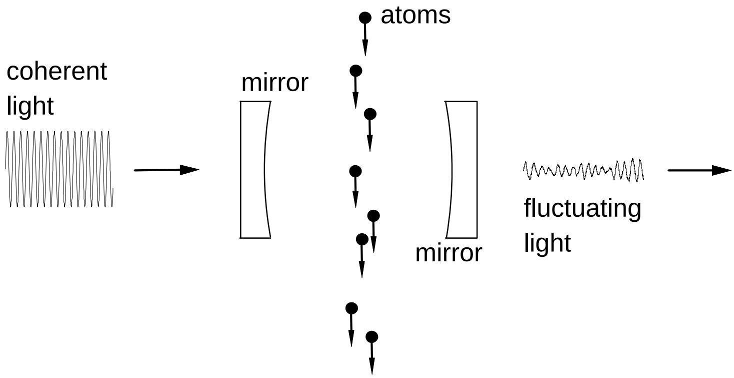


Fig. 10.2. Schematic of the cavity QED light source. The input laser light and Fabry–Perot cavity are both tuned to resonance with a dipole allowed transition in the atoms.

add fluctuations to the transmitted beam. The incoherent part of this forward scattering would be extremely small without the mirrors. They are essential; they form a resonator which enhances the fluctuations. We might understand the requirements for the resonator by observing that the goal (thinking of light particles) is to redistribute the photons in the incoming beam. The interaction of the atoms with a first photon must therefore change the probability for the transmission of the next. The strength of any such collusion between pairs of photons is set by Einstein's induced emission rate in the presence of a single photon. This rate must be similar to the inverse residence time of a photon trapped between the mirrors. It follows that the resonator must be small so that the energy density of one photon is large, and the mirrors must be highly reflecting so that the residence time is long.

The experimental details go beyond the scope of this book, but a few numbers might be of interest [29]. Typical resonator lengths are 100–500μm with 50,000 bounces of a photon between the mirrors before it escapes. The transverse width of the resonator mode is typically 30μm, which means the resonator confines a photon within a volume of order 10^{-13}m^3; the electric field of that photon is approximately $10\,\mathrm{V cm}^{-1}$. The duration of a fluctuation written onto the light beam may be estimated from the photon lifetime, $(L/c)N_{\mathrm{bounce}} \sim 50\,\mathrm{ns}$, where L is the resonator length and N_{bounce} is the number of mirror bounces. This is a long time compared with the speed of modern photoelectric detectors which makes it possible to observe the fluctuations directly in the time domain. We should note, also, that the fluctuations are extremely slow compared with the period of the carrier wave; a typical fluctuation will last more than 10^7 optical cycles.

It is not really necessary to understand what takes place inside the resonator. We are interested in the results of measurements made on the output beam and whether or not they can be reproduced by our BKS detection model (Fig. 10.1) and *any* fluctuating wave $A_t \cos(\omega_0 t + \phi_t)$. One feature of the data is particularly noticeable though: an oscillation at a frequency of around 40 MHz (see Fig. 10.4 for example), which suggests that the fluctuations caused by the interaction with the atoms take the form of an amplitude modulation. The modulation is a fundamental piece of phenomenology from the world of cavity QED, referred to variously as a vacuum Rabi oscillation [30], a normal-mode oscillation [31], or a cavity polariton oscillation [32]. The physics involved is rather simple. The electric field of the resonator mode excited by the incident light obeys the equation of a harmonic oscillator, of frequency ω_0. To a good approximation the electric polarization induced in the atoms by that field is also described as a harmonic oscillator (Lorentz oscillator model), also with frequency ω_0. The two oscillators couple through the interaction between the atoms and light; and coupled harmonic oscillators exchange energy coherently, back and forth, so long as the period of the exchange – determined by the inverse of the coupling strength – is shorter than the energy damping time. It is just this coherent energy exchange that is seen in the fluctuations. The small mode volume of the resonators used in cavity QED experiments ensures that the energy oscillation has a period shorter than the damping time – although there are still some 10^7 optical cycles during any one period.

10.4 Photon Antibunching: A Probe of Particle Fluctuations

Let us look first at a measurement that leads us towards the opinion that what is transmitted by the resonator is a stream of light particles. In Fig. 10.3, we return, with more details, to our criterion for nonclassicality. Here, in a somewhat arbitrary example, I have generated a realization of the photoelectric counts that might be produced for a particular wave $A_t \cos(\omega_0 t + \phi_t)$. It is of course unreasonable to use a realistic carrier frequency, and therefore the frequency in the picture is about a million times smaller – relative to the timescale of the fluctuations – than it would be in reality.

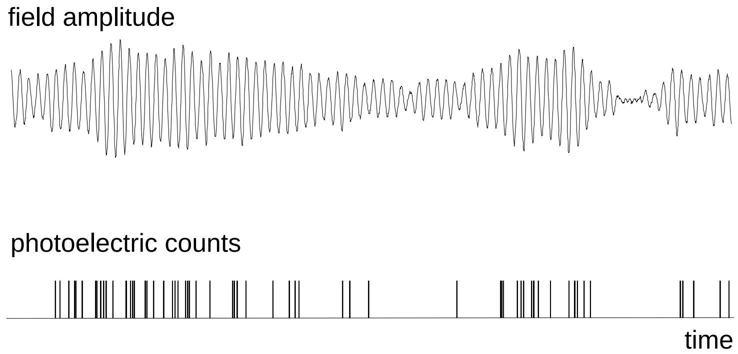


Fig. 10.3. Monte Carlo simulation of a photoelectric count sequence produced, with count rate A_t^2, by the shown fluctuating electromagnetic field $A_t \cos(\omega_0 t + \phi_t)$.

We see from the figure that there are correlations between what the wave is doing and the sequence of photoelectrons: when the wave amplitude is large the photoelectric counts come more quickly; when the wave amplitude is small there are gaps in the count sequence. There are also correlations, over time, within each of the time series. Thus, if at time t the intensity A_t^2 is high, it is likely that $A_{t+\tau}^2$ is also high for small positive and negative delays τ. Equivalently, if there is a photoelectron produced at time t, there is a larger than average chance that another is produced nearby at a delayed time $t+\tau$. Quantitative statements about the correlations can be made by introducing the correlation function

$$g^{(2)}(\tau) \equiv \frac{\text{probability for a photoelectric detection at times } t \text{ and } t+\tau}{\left(\text{probability for a photoelectric detection at time } t\right)^2}, \tag{10.8}$$

where we will assume we are talking about stationary fluctuations, which simply means that all probabilities and averages are independent of t; the correlation function depends only on the time difference τ.

According to the BKS detection model, photoelectrons are produced as random events at rate A_t^2. The correlations in the photoelectron counting sequence are therefore connected to the fluctuations of the wave through the joint detection probability

$$\left\{\begin{matrix}\text{probability for a photoelectric} \\ \text{detection at times } t \text{ and } t+\tau\end{matrix}\right\} \propto A_t^2 A_{t+\tau}^2; \tag{10.9}$$

there can be no stronger connection between events than can be expressed through the correlations of the continuous variable A_t. It is rather easy to see that this feature imposes constraints on the function $g^{(2)}(\tau)$. Specifically, averaging over the random variables A_t and $A_{t+\tau}$ one finds that the inequalities

$$g^{(2)}(0) - 1 \geq 0 \tag{10.10}$$

and

$$|g^{(2)}(\tau) - 1| \leq |g^{(2)}(0) - 1| \tag{10.11}$$

must hold. Inequality (10.10) restates a point we have already noted; namely, that the BKS idea leads, unavoidably, to a photoelectron counting sequence that is more irregular than a Poisson process. The inequality relies on nothing more than the fact that the variance of A_t^2 must be positive. Needless to say, to take over Einstein's expression [12], inequalities (10.10) and (10.11) "lead to contradictions with experience." The data of Fig. 10.4, as an illustration, violate both.

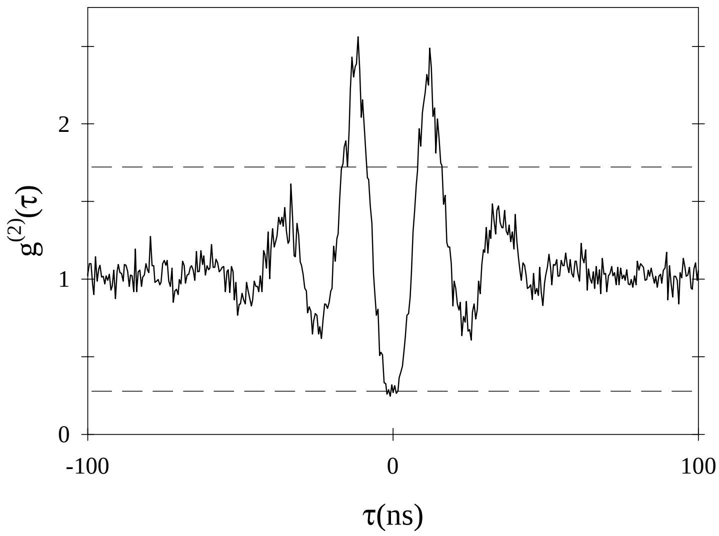


Fig. 10.4. Violation of the inequalities imposed on the intensity correlation function by the photoelectric detection scheme of Fig. 1. To satisfy the inequalities the correlation function should show a maximum greater than unity at zero delay; accepting the observed minimum, it must then lie entirely between the dashed lines. Figure reprinted with permission from G. T. Foster et al., Phys. Rev. A **61**, 053821 (2000). ©2000 by the American Physical Society.

In order to obtain the result shown in the figure, the photoelectric counts must be anticorrelated, in the sense that the appearance of a count at any particular time in the photoelectron count sequence makes it less, rather more likely, that another will appear nearby. The phenomenon is called photon antibunching to contrast it with the photon bunching – positive correlation – seen with a source of blackbody radiation [33]. The simplest example of antibunched light is provided by the resonance fluorescence from a single atom [34] and the first observations of the phenomenon were made on the fluorescence from a dilute atomic beam [35]. More recently, beautiful measurements have been made on individual atoms, or more precisely, electromagnetically trapped ions [36]. The data of Fig. 10.4 were taken for a cavity QED source like the one illustrated in Fig. 10.2 [37]. Such a source produces a weak beam of antibunched light [38]. Recently photon antibunching was observed in cavity QED with a single atom [39].

Photon antibunching is nonclassical by our adopted criterion. It is incompatible with the demarcation enforced by BKS between continuous light waves and discrete photoelectric counts. It is quite compatible, on the other hand, with a particle constitution for light. Indeed, there is nothing particularly peculiar, in principle, about a sequence of photoelectric counts more regular than a Poisson process, and such a sequence could be generated causally by a regular stream of light particles.

10.5 Quadrature Squeezing: A Probe of Wave Fluctuations

The only difficulty with the stream of light particles is that, looked at in another way, the same source of light does appear to be emitting a noisy wave. Whenever interference is involved, a wave nature for light seems unavoidable. There are, of course, numerous situations in which the interference of light is seen. We are all familiar, for example, with Young's two-slit experiment. Considering wave aspects of the fluctuations of light calls for an interference experiment that is just a little bit more complex.

Balanced homodyne detection provides a method for directly measuring the amplitude of a light wave. The method is carried over from the microwave domain and was proposed in the 1980s [40] for detecting the fluctuations of what is known as quadrature squeezed light [41]. The light that produced Fig. 10.4, which is antibunched when photoelectron counts are considered, is quadrature squeezed when its amplitude is measured. Like photon antibunching, quadrature squeezing contradicts the BKS model of photoelectric detection; according to our criterion it is also nonclassical. It, however, leads us away from the stream of light particles and towards the view that light is indeed a noisy wave; not, on the other hand, exactly the wave BKS had in mind.

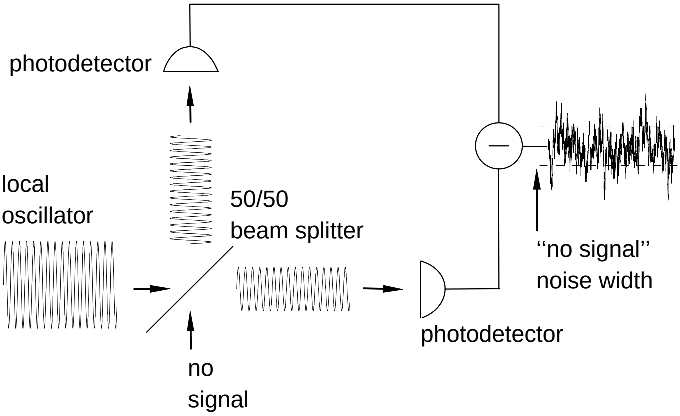


Fig. 10.5. In balanced homodyne detection, a coherent local oscillator field with frequency ω_0 matching that of the signal carrier wave is superposed with the signal at a 50/50 beam splitter. The resulting output light is detected with a pair of fast photodiodes and the two photocurrents subtracted to zero the mean “no signal” current. An electronic shot noise remains, uncanceled; it is necessarily present according to the scheme of Fig. 10.1 due to the randomness of the detection events that generate the photocurrents. The “no signal” noise width measures the size of this noise and scales with the amplitude of the local oscillator field and the square root of the detection bandwidth.

I find it most helpful to understand quadrature squeezing in an operational way, so I will proceed in this direction, and hopefully move ahead in a series of easy steps. The basic idea in balanced homodyne detection is to interfere the signal wave, $A_t\cos(\omega t+\phi_t)$, with a reference or local oscillator wave, $A_{\rm LO}\cos(\omega t+\phi_{\rm LO})$, which ideally has a stable amplitude and phase. If the interference takes place at a 50/50 beam splitter as illustrated in Fig. 10.5 (the signal wave is injected where it says “no signal”), then there are in fact two output fields,

$$\textit{field 1} = \frac{1}{\sqrt{2}}[A_{\rm LO}\cos(\omega t+\phi_{\rm LO}) + A_t\cos(\omega t+\phi_t)], \quad (10.12)$$

$$\textit{field 2} = \frac{1}{\sqrt{2}}[A_{\rm LO}\cos(\omega t+\phi_{\rm LO}) - A_t\cos(\omega t+\phi_t)], \quad (10.13)$$

which respectively display constructive and destructive interference. These fields are separately detected, and the rates at which photoelectrons are generated in the two detectors, once again adopting the detection model of Fig. 10.1, are

$$\textit{rate 1} \approx \frac{1}{2}[A_{\mathrm{LO}}^2 + A_{\mathrm{LO}} A_t \cos(\phi_{\mathrm{LO}} - \phi_t)], \tag{10.14}$$

$$\textit{rate 2} \approx \frac{1}{2}[A_{\mathrm{LO}}^2 - A_{\mathrm{LO}} A_t \cos(\phi_{\mathrm{LO}} - \phi_t)], \tag{10.15}$$

where to obtain these expressions we square the fields, average over one carrier wave period, and drop the term A_t^2 under the assumption $A_t \ll A_{\mathrm{LO}}$. The average photocurrents from the detectors are proportional to the rates (10.14) and (10.15), and when the photocurrents are subtracted, the average difference current provides a measurement of the amplitude A_t (consider the case $\phi_{\mathrm{LO}} = \phi_t$). Thus, we have a device that measures the amplitude of a light wave and its operation depends explicitly on the capacity of waves to interfere.

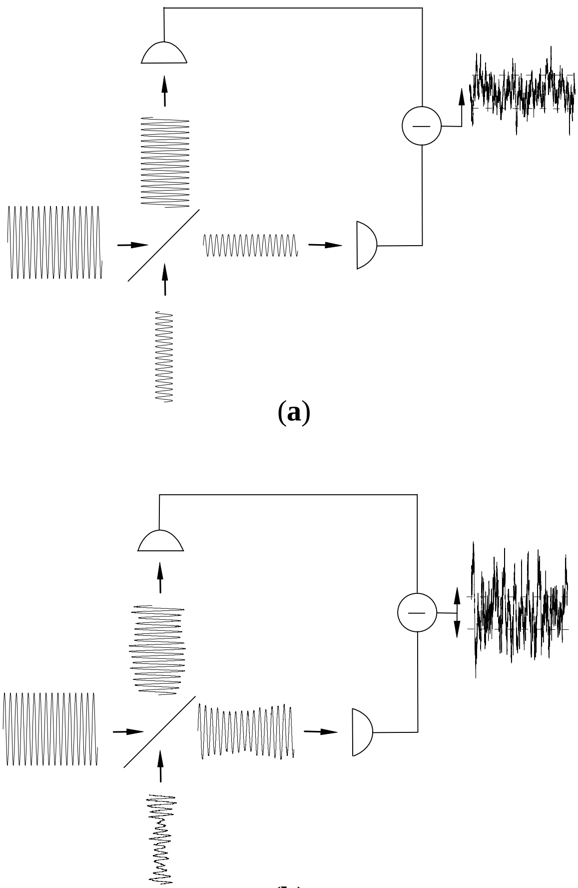
We now turn to the issue of fluctuations. Imagine first that there is no signal injected. The two photocurrents are produced with equal photoelectron count rates, $\frac{1}{2}A_{\mathrm{LO}}^2$. The average difference current is therefore zero. But according to the detection model of Fig. 10.1, individual detection events occur randomly, and independently at the two detectors; hence, the current fluctuates about zero. Since the counts are Poisson distributed, there is a

$$\textit{“no signal” noise width} \propto \sqrt{\frac{1}{2}A_{\mathrm{LO}}^2 + \frac{1}{2}A_{\mathrm{LO}}^2} = A_{\mathrm{LO}}. \tag{10.16}$$

This is an unavoidable background noise level and when a clean, noiseless signal is injected, to unbalance the detector, as illustrated in Fig. 10.6a, the measurement of the signal amplitude is made against this background noise.

In the end, then, there is again a constraint, akin to inequalities (10.10) and (10.11), imposed by the detection model. To see what it is, consider finally the injection of a fluctuating signal. The signal adds a fluctuating offset, or unbalancing of the detector, which sweeps the “no signal” noise band backwards and forwards (Fig. 10.6b) to produce a *larger* overall noise width; for a fluctuating signal we must *add* the statistically independent

$$\textit{signal noise width} \propto \sqrt{A_{\mathrm{LO}}^2 \overline{A_t^2}} = A_{\mathrm{LO}} \sqrt{\overline{A_t^2}} \tag{10.17}$$



(a)

(b)

Fig. 10.6. (**a**) A signal field of fixed amplitude and phase unbalances the homodyne detector so that the mean difference current moves away from zero while the noise width remains unchanged. (**b**) A fluctuating signal unbalances the detector in a noisy way, sweeping the difference current back and forth. This introduces additional low-frequency noise which must *increase* the overall noise width.

to the "no signal" noise width, where $\overline{A_t^2}$ is the variance of the signal fluctuations. Thus, according to the BKS detection model, measuring the fluctuations of the light wave amplitude can never yield a noise width smaller than (10.16), the width that in more conventional language is called the *shot noise level.*

In reality smaller noise can be seen, and is seen, for squeezed light. The first successful experiment was performed in the mid-1980s [42] and squeezing for a cavity QED source like the one that produced the antibunched data of Fig. 10.4 was observed soon thereafter [43]. In general, the noise level is measured as a function of frequency. It is therefore characterized fully by a spectrum of squeezing. Fig. 10.9c shows an example for a cavity QED source. The squeezing occurs around the frequency of the oscillation seen in Fig. 10.4.

Quadrature squeezing, like photon antibunching, reveals that a beam of light may exhibit smaller fluctuations – more regularity – than is permitted by the random events that make the interface between light waves and photoelectrons in Fig. 10.1. In the case of photon antibunching, we may imagine that the regular photoelectrons are seen because the light already, before interaction, possesses the discrete property revealed in the photoelectron counting data – i.e., the light beam is itself a stream of particles. With quadrature squeezing a similar tactic might be followed; the fluctuation properties of the photocurrent might be transferred, ahead of any interaction with the detector, to the beam of light. The one difficulty here, though, is that the injection of no light also generates photocurrent noise, which is the situation depicted in Fig. 10.5. The way around this obstacle is to say that a fluctuating wave is present – call it the vacuum fluctuations – even in absolute darkness, and that it is the interference of this "noisy darkness" with the local oscillator that is responsible for the "no signal" noise width. A smaller noise level can then be seen if one can deamplify the "noisy darkness" (vacuum fluctuations); the cavity QED system of Fig. 10.2 is a device that brings about deamplification.

I should stress that when one accounts for quadrature squeezing in this way, the vacuum fluctuations need not be encumbered by any abstractions of modern quantum field theory. The vacuum of radiation is literally filled with noisy waves, precisely in the way proponents of stochastic electrodynamics assert it to be [44].

10.6 Wave–Particle Correlations

What we have seen so far amounts to a fairly traditional view on wave/particle duality, although the players, photon antibunching and quadrature squeezing, are possibly unfamiliar; photon antibunching sits comfortably on the particle side, while quadrature squeezing, because of the role of interference, speaks for light as a wave. The recent experiment by Foster et al. brings the duality into focus in a more perplexing way by putting both players into action at once. That is not to say that it demonstrates a contradiction, of the sort that would be met if, in a double-slit experiment, one could record the choice, slit 1 or slit 2, for the path of every particle, yet still observe an interference pattern on the screen. Nevertheless, data of the discrete, particle-type, and continuous wave-type are taken simultaneously, so that light is seen in the experiment to act as particle *and* wave. The experiment underscores the subtlety involved in the coexistence of waves and particles under Bohr's complementarity, the illusive contextuality of quantum mechanical explanations. Specifically, the apparently satisfying explanations given for photon antibunching and quadrature squeezing – passing whatever properties are seen in the data over to the light – appear, in this wider context, to be something of a deception.

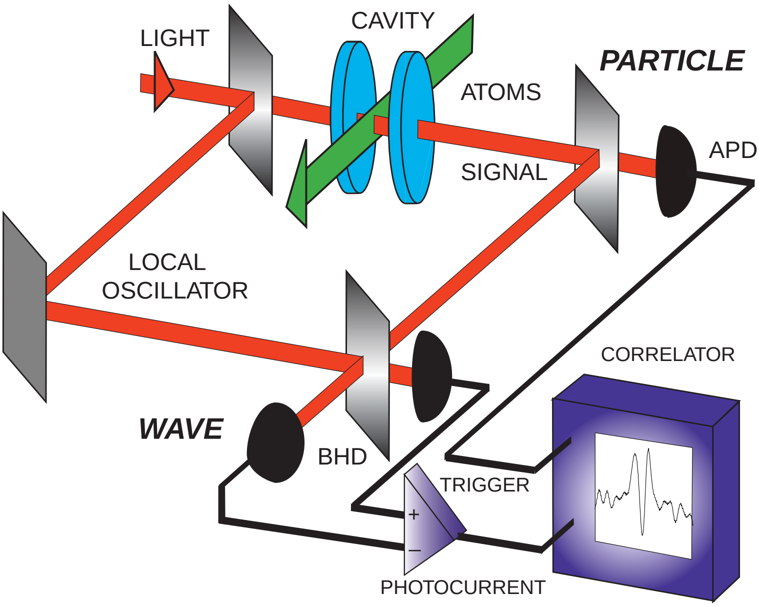


Fig. 10.7. Experimental apparatus used to measure wave-particle correlations for the cavity QED light source. Photoelectric detections at the avalanche photodiode (APD) trigger the recording of the photocurrent from the balanced homodyne detector (BHD). The correlator displays the cumulative average over many such records. Figure reprinted with permission from G. T. Foster et al., Phys. Rev. Lett. **85**, 3149 (2000). ©2000 by the American Physical Society.

The experimental apparatus is sketched in Fig. 10.7. At the top of the figure there is a cavity QED system which acts as the source of fluctuating light. The emitted light is divided between two detectors. One detector, labeled PARTICLE, records discrete photoelectric counts. The other detector, labeled WAVE, is a balanced homodyne detector. If all of the light were sent to just one detector, the apparatus could be used to measure either photon antibunching or quadrature squeezing. In fact, the detectors are running simultaneously. A count at the particle detector triggers the recording of the photocurrent at the wave detector output – a little before and a little after the time of the count – and many of these records are averaged to produce what appears on the oscilloscope. What might we expect to see from this *conditional* measurement of the wave amplitude? The experiment records the fluctuation of the amplitude of the wave that accompanies the arrival of a photon at the particle detector. How will the wave and particle properties be correlated?

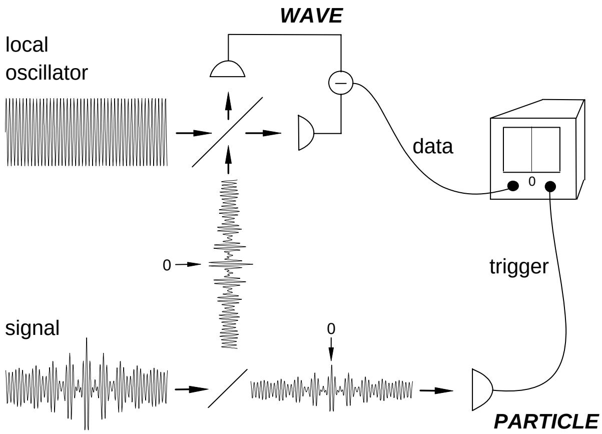


Fig. 10.8. Semiclassical analysis of the wave-particle correlator: The signal fluctuation incoming from the lower left is divided at a beam splitter into two parts, with one part sent to the particle detector and the other to the wave detector. Each "click" of the particle detector fixes the time origin, $\tau = 0$, for a sampling of the wave detector output over the duration of the fluctuation; the local oscillator phase is set to measure the amplitude of the wave envelope. According to the photoelectric detection scheme of Fig. 10.1, the particle detector "clicks" most often when the intensity is *maximum*. This should place the *maximum* of the measured wave amplitude at $\tau = 0$.

In Fig. 10.8 I attempt to show what would be expected on the basis of the BKS detection model. A fluctuation from the light source is injected into the correlator at the lower left; I give it the sort of amplitude modulation evident in Fig. 10.4. The input wave is split, and passed on, at half size, to the two detectors. Now the triggering sets the time origin for the amplitude envelope function measured by the wave detector. The question, then, is, at what point in time is the particle detector most likely to fire? ... The answer: when the fluctuation in the amplitude of the wave reaches its maximum. Strangely, the reality is exactly the opposite, as is seen from the data shown in Fig. 10.9b. Fig. 10.9a shows the corresponding particle-particle correlation function, $g^{(2)}(\tau)$, which in this case satisfies the inequalities (10.10) and (10.10). In Fig. 10.9c we see the spectrum of squeezing, which might have been measured directly, but was in fact deduced from the correlation function plotted in Fig. 10.9b.

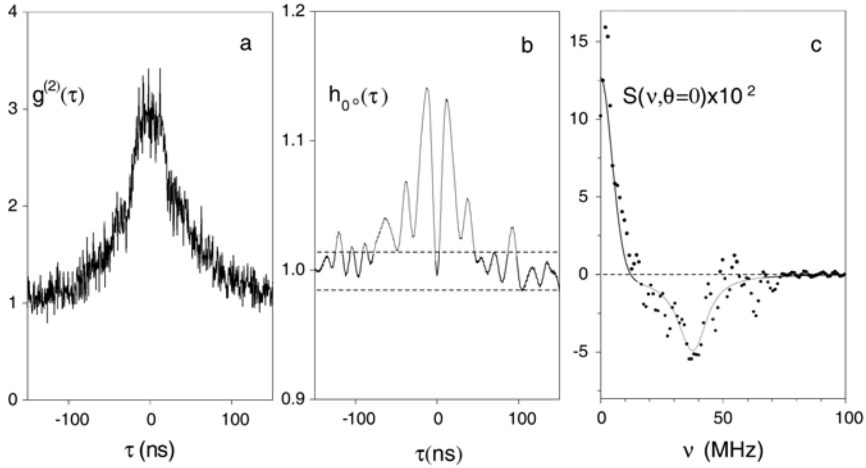


Fig. 10.9. Nonclassical wave-particle correlations for the cavity QED light source: (**a**) the measured intensity correlation function is classically allowed, (**b**) the corresponding wave-particle correlation function, which should lie entirely within the shaded region according to the photoelectric detection scheme of Fig. 10.1, (**c**) the spectrum of squeezing obtained as the Fourier transform of (b); for a classical field the spectrum would lie entirely above the dashed line. Figure reprinted with permission from G. T. Foster et al., Phys. Rev. Lett. **85**, 3149 (2000). ©2000 by the American Physical Society.

There are stronger signatures of nonclassicality to be observed than this conversion of an expected maximum to a minimum. These may be stated quantitatively, as violations of inequalities like those of Eqs. (10.10) and (10.11) [45]. The most interesting says that the function plotted in Fig. 10.9b is constrained under the BKS detection model by an absolute upper bound, $h_{\theta^\circ}(\tau) \leq 2$. The bound is not violated in the figure, but is predicted to be violated in a more sensitive experiment by a factor of 10 or even 100. Considering the minimum itself, though; how is it to be understood; and what does it have to say about the interplay of waves and particles?

Of course a calculation within the modern mathematical framework for treating quantized fields predicts the minimum at $\tau = 0$. Merely calculating gives little physical insight though; for insight we turn to something more qualitative. First, I should expand a bit on what is shown in Fig. 10.8. Over an ensemble of triggered measurements, the phase of the modulated envelope function will vary from shot to shot. Two extreme cases are shown in Fig. 10.10. In both, according to the detection model of Fig. 10.1, triggering off a maximum of the envelope places a maximum of the measured field amplitude at $\tau = 0$ – the absolute locations of the maxima in the incoming fluctuations do not matter, only the correlation between locations in the two waves emerging from the beam splitter. The unexpected minimum of Fig. 10.9b may now be obtained, rather simply, by viewing the cases shown, not as two possible outcomes realized on distinct occasions – one *or* the other on any occasion – but as two possibilities that occur simultaneously, and yet retain their distinctness. The words "retain their distinctness" are essential. We are not to add together the waves shown bracketed in Fig. 10.11 as one would add classical waves. Each of these waves also has a discrete attribute, indicating an individuality with respect to its counterpart, as a whole, distinct, "one-particle wave" – the two pieces being assembled, the bracketed object is a "two-particle wave." In modern language we would call it a two-photon wavepacket.

To explain the data we now assert, that at the beam splitter, the discreteness, or wholeness, comes into play, and one one-particle wave goes in either direction. We may then continue with the idea that the particle detector is most likely to fire when the intensity of the wave it sees is a maximum. With the now built-in anticorrelation of modulation phases, whichever one-particle wave goes to the particle detector, the amplitude recorded by the wave detector is at a minimum at the triggering time. The two possibilities are shown in Fig. 10.11. There is of course a possibility that both one-particle waves go to the same detector; occurences like this cannot, however, upset the correlation recorded by the data.

Thus, if we are to account for the correlations observed with the apparatus of Fig. 10.7, neither the particle stream that explained photon antibunching, nor the noisy wave that explained quadrature squeezing will do. We need a composite notion like the "two-particle wave"

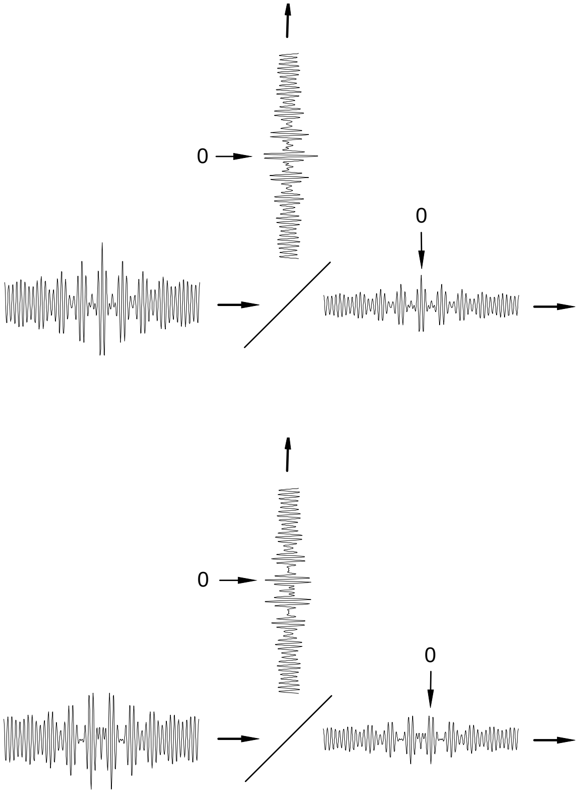


Fig. 10.10. Two possible signal fluctuations, centered, respectively, on a maximum and a minimum in the amplitude of the wave envelope. In either case the particle detector "clicks" most often on a maximum, which places the *maximum* of the measured wave amplitude at $\tau = 0$.

in order to embrace both pieces of the correlation, both the discrete triggering event and the continuously measured amplitude.

10.7 A Concluding Comment

The main topics of this chapter can be revisited in a short summary. We have seen how the BKS idea embodied in the photoelectric detection model of Fig. 10.1 is unable to account for certain correlations exhibited by the fluctuations of light. In these cases, an understanding of the correlations must relax the strict separation of particle and wave concepts carried over by BKS from classical theory. Substituting lasers for the blackbody sources studied by Planck, many experiments

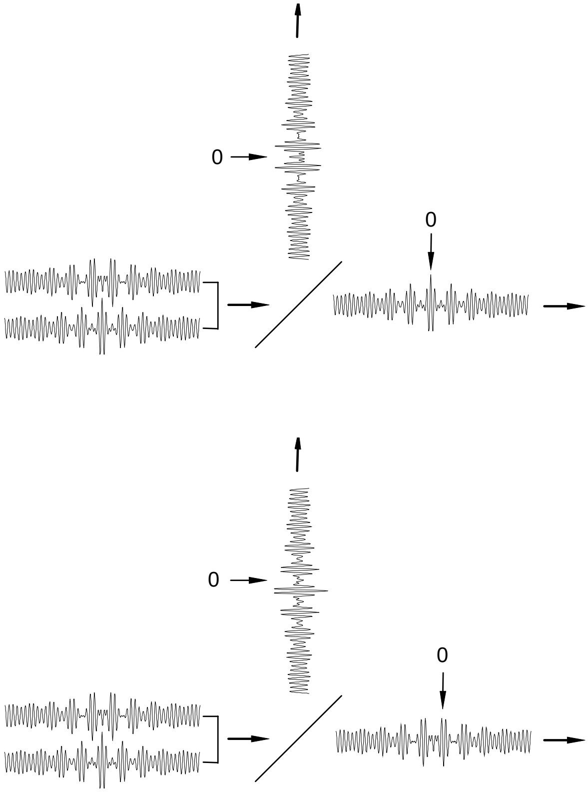


Fig. 10.11. Schematic illustration of how the anomalous wave-particle correlations may be accounted for by combining wave and particle ideas. The alternatives in Fig. 10.10 are united as a single input fluctuation carried by a two-particle wave with correlated maxima and minima. The beam splitter then splits up the two-particle so as to preserve the wholeness of the individual waves. For either of the splittings shown, the correlation between maxima and minima is thus conveyed to the detectors so that the firing of the particle detector at the intensity maximum places the measured wave amplitude *minimum* at $\tau = 0$.

in recent years have observed such nonclassical correlations. The experiment of Foster et al. [25] is notable, in particular, because if its simultaneous measurement, and correlation, of the conflicting particle and wave aspects of light.

In a way, all of this serves only as an introduction to a second, more interesting, story. Looking to the quantum mechanics that eventually emerged over the years after Planck, surely, now, we can give an unproblematic account of what light really is? Unfortunately, in fact, we cannot, because we move here into new territory, where we have to admit that although we have a formalism with which to calculate what we see, it is not at all unbroblematic to put forward an ontology on which that formalism can rest. After BKS, Bohr's thinking moved on to his ideas about complementarity [46]. Einstein never accepted these views, and on occasion dismissed them rather harshly [47]: "The Heisenberg–Bohr soothing philosophy – or religion? – is so finely chiseled that it provides a soft pillow for believers . . . This religion does dammed little for me." Thus, the second story, the enunciation of exactly where quantum mechanics has led us, is an interesting one, but certainly also a difficult one to tell. All I can really do at the conclusion of this chapter is indicate how the scheme of Fig. 10.1 is changed to give a unified, quantum mechanical description of the incoming light from which the correct correlations can be extracted, whatever measurement is made.

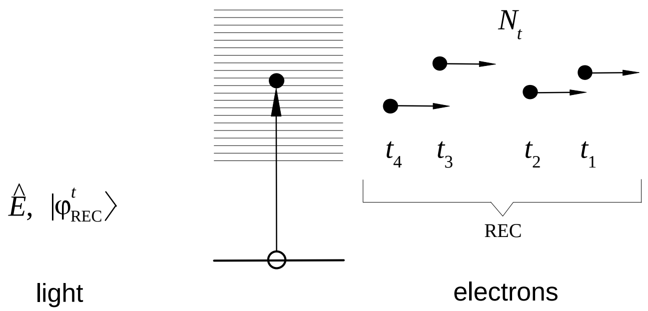


Fig. 10.12. The quantum trajectory treatment of the photoelectric detection of light couples the stochastic data record, N_t, to a stochastic state of the quantized electromagnetic field, $|\psi^t_{\mathrm{REC}}\rangle$, through random detection events occuring, at time t, at the rate $\langle\psi^t_{\mathrm{REC}}|\hat{E}^\dagger\hat{E}|\psi^t_{\mathrm{REC}}\rangle$. The evolution of the quantum state becomes stochastic because there is a state reduction $|\psi^t_{\mathrm{REC}}\rangle \to \hat{E}|\psi^t_{\mathrm{REC}}\rangle$ (plus normalization) at the random times of the detection events.

The changed picture appears in Fig. 10.12 [48]. I must point out two things. In change number one, although, on the right, the photoelectrons are still conceived as a classical data record, the light on the left is accounted for in abstract form. It is no longer a wave of assigned quantitative value. It is represented by an *operator*, $\hat{E}$, which is defined, not by a value but by the actions it might take; the value of the wave emerges only when the operator acts – upon a second mathematical object, the state vector $|\psi^t_{\mathrm{REC}}\rangle$. There is of course some mathematics that gives the explicit forms of $\hat{E}$ and $|\psi^t_{\mathrm{REC}}\rangle$. For an appreciation of the scheme, however, the mathematics is only a distraction.

Change number two, and an essential thing missing from the BKS proposal, is the label on the state vector, REC. Through this label, the state of the incoming light is allowed to depend on the history of the data record – the detection events that have already taken place. At the time of each event, $\hat{E}$ acts on $|\psi^t_{\mathrm{REC}}\rangle$ to annihilate a light particle, and in so doing updates the state of the incoming light to be consistent with the record of photoelectric counts. In this way, correlations at the level of the individual quantum events are taken into accounted. The communication through the label REC is what, today, quantum physicists call back action, or in other words the reduction of the state vector (or, less appealing, "collapse of the wavefunction"), applied here to the individual detection events. Without state reduction the Schrödinger equation entangles the two sides of Fig. 10.12. It offers a nonlocal description in terms of a global state vector. State reduction disentangles the state of the light from the realized photoelectrons, and the correlations we have called nonclassical are indirect evidence of this disentanglement.

Entanglement, nonlocality, state reduction, these are all words to remind us of the problematic issue of ontology. Other chapters in this volume will discuss them more directly [49]. It is difficult to say what will come from the attention these words are receiving one hundred years after Planck. Shall we come to understand better, perhaps through a refinement of our faculties of perception following from the incredible advances experimentation has made over these one hundred ears. It might be appropriate to reserve the final thoughts on the subject for Max Planck himself:

> ... There is no doubt whatsoever that the stage at which theoretical physics has now arrived is beyond the average human faculties, even beyond the faculties of the great discoverers themselves. What, however, you must remember is that even if we progressed rapidly in the development of our powers of perception we could not finally unravel nature's mystery. We could see the operation of causation, perhaps, in the finer activities of the atoms, just as on the old basis of the causal formulation in classical mechanics we could perceive and make material images of all that was observed as occurring in nature.
>
> Where the discrepancy comes in today is not between nature and the principle of causality, but rather between the picture

which we have made of nature and the realities in nature itself [50].

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11

Quantum Entanglement as a Resource for Communication

William K. Wootters

Entanglement, a remarkable kind of correlation that can exist between two quantum objects, is fundamentally different from classical correlation. When two quantum particles are entangled, they must in some respects be treated as a single entity: neither particle has a pure quantum state of its own. In 1935 Schrödinger identified entanglement as *the* feature of quantum theory that forces its departure from the classical paradigm [1], and for decades now, entanglement has been studied as a marvel of nature and as evidence of nonlocality in the physical world [2]. But it is only in the past ten years or so that a new focus has been given to the study of entanglement: researchers have begun to say, "We know it's interesting, but is there anything we can do with it? Can we use it?" And indeed, it turns out that we can use it. We can use it as a resource for certain novel modes of communication, which are the subject of this chapter. The chapter is divided into three parts: (1) an introduction to entanglement; (2) a discussion of three applications of entanglement for communication – dense coding, teleportation, and the pooling of separated data – and (3) a description of an actual teleportation experiment. As I explain the three applications, it will become clear that none of them is particularly close to being marketable. But they are all interesting nonetheless, and someday they probably will be practical.

11.1 What is Entanglement?

Let us start by describing a single spin-1/2 particle, such as an electron or a proton. For definiteness I will imagine the particle as an electron. The state of the spin can be described as a direction in space – the spin axis points in some direction – and any direction is possible. I recall learning in high school that an electron's spin can point only up or down, and this bothered me a lot, because how did the electron know which axis was vertical? So I was very pleased to learn in college that in fact any spin direction is possible. The binary aspect of the electron's spin does, however, determine what sorts of *measurement* we can make. In particular, it is *not* possible to find out by any measurement the direction in which the electron's spin is pointing, if we know nothing about this direction *a priori*. That is, we cannot ask the electron, "What is your spin direction?" Rather, we can ask only binary questions that give the electron a choice between two opposite directions.[1] We can ask, "Is your spin up or down?" or "Is it right or left?" Typically the actual spin direction will not be one of the two choices we have given the electron, in which case the electron must change its spin direction so as to conform to our question: it must adopt one of the states we have offered it. The change is made probabilistically, with greater weight being given to the choice that is closer to the electron's actual spin direction. If the actual direction is only a few degrees away from "up," then the electron is likely to choose "up" over "down."

Now, any spin direction can be thought of as a quantum superposition of up and down, or of right and left, or of any other pair of opposite directions. If we take up and down to be our basic directions, we can write a general superposition as

$$\alpha\,|\uparrow\rangle + \beta\,|\downarrow\rangle, \tag{11.1}$$

where α and β are complex numbers whose squared magnitudes add up to one: $|\alpha|^2 + |\beta|^2 = 1$. The numbers $|\alpha|^2$ and $|\beta|^2$ are the probabilities that the electron will choose up and down, respectively, when it is given that binary choice.

It turns out, by the way, that a photon's polarization is described by exactly the same mathematics as an electron's spin. However, in the case of a photon, the directions are not in physical space but in an abstract space. The up direction corresponds to right-hand circular polarization, the down direction to left-hand circular polarization, the horizontal directions to linear polarizations, and everything else to elliptical polarizations. These correspondences will not be important in what follows, but it will be important to remember that photon polarization and electron spin are essentially isomorphic. Experiments on

[1] More precisely, we are restricted to binary questions if we insist that each possible answer to our question correspond to a quantum state that will always yield that answer. There exist more general measurements, but they are probabilistic for *every* quantum state.

entanglement are most often carried out on photons, but in my examples I will usually talk about electrons, because it is easier to imagine directions in ordinary space.

If the spin of a single electron is described as a direction in space, how do we describe the spins of a pair of electrons? If we were doing classical physics, the answer would be simple: a pair of electrons would have two spin directions, one for each electron. Such a state is possible in quantum mechanics also, and we write it this way, expressing each spin direction as a superposition of up and down:

$$(\alpha_1 \left|\uparrow\right\rangle + \beta_1 \left|\downarrow\right\rangle) \otimes (\alpha_2 \left|\uparrow\right\rangle + \beta_2 \left|\downarrow\right\rangle) . \tag{11.2}$$

The state in the first set of parentheses describes the first electron, the other one describes the second electron, and the symbol "⊗" (the tensor product symbol) is what we use to put them together. The tensor product is much like ordinary multiplication, and in fact we can multiply out the above expression to obtain

$$\alpha_1\alpha_2 \left|\uparrow\uparrow\right\rangle + \alpha_1\beta_2 \left|\uparrow\downarrow\right\rangle + \beta_1\alpha_2 \left|\downarrow\uparrow\right\rangle + \beta_1\beta_2 \left|\downarrow\downarrow\right\rangle. \tag{11.3}$$

Here $\left|\uparrow\uparrow\right\rangle$ represents the state in which both electrons have spin up, and so on.

Remarkably, this state is not the most general state possible for the spins of two electrons. The most general state is

$$\left|\psi\right\rangle = a \left|\uparrow\uparrow\right\rangle + b \left|\uparrow\downarrow\right\rangle + c \left|\downarrow\uparrow\right\rangle + d \left|\downarrow\downarrow\right\rangle, \tag{11.4}$$

where the only restriction on the complex numbers a, b, c, and d is that their squared magnitudes add up to one. It is not hard to see that there are states of the form (11.4) that are not of the "product" form (11.3). In order for the state $\left|\psi\right\rangle$ to be of the product form, it would have to satisfy $ad = bc$, because in Eq. (11.3), $(\alpha_1\alpha_2)(\beta_1\beta_2)$ is necessarily the same as $(\alpha_1\beta_2)(\beta_1\alpha_2)$. But in general there is no reason why ad should be the same as bc; so most allowed states are not product states. They are entangled. In an entangled state, as we have said before, neither particle has a pure spin state of its own.

The first two figures show two examples of entangled spin states. The first example, shown in Fig. 11.1, is the singlet state: $(1/\sqrt{2})(\left|\uparrow\downarrow\right\rangle - \left|\downarrow\uparrow\right\rangle)$. As the notation suggests, if we were to measure each spin along the vertical axis, that is, if we were to make the up-versus-down measurement on each particle, then the two particles would give opposite results: if one of them chose "up" the other would have to choose "down." What is not so obvious from the notation, but is true, is that

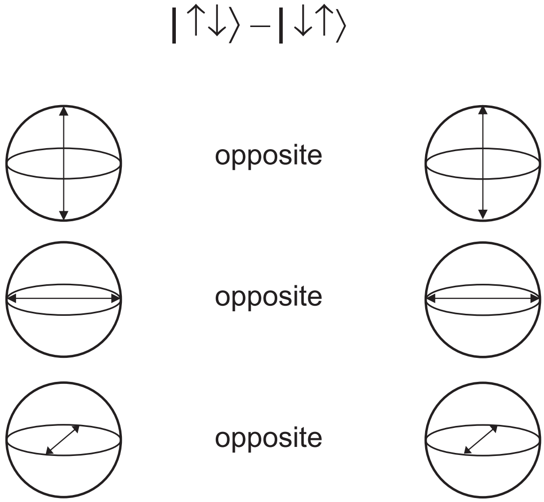

Fig. 11.1. Two particles in the singlet state will give opposite results for a measurement along any spin axis, as long as the axis is the same for both particles. (Here and in the other figures we omit the normalizing factor $1/\sqrt{2}$.)

if we were to make the right-versus-left measurement on both particles they would again choose opposite outcomes. (To see why, we could express the state in the right-left basis.) In fact, if we measure along any axis, as long as it is the same axis for both particles, the outcomes will always be opposite. Sometimes I call electrons in the singlet state "deeply opposite," because they are opposite along every axis.

The other example, in Fig. 11.2, looks almost the same as the singlet state – it is $(1/\sqrt{2})(|\uparrow\downarrow\rangle + |\downarrow\uparrow\rangle)$. Again the electrons will give opposite results if both are measured along the vertical axis. But if we measure them both along some horizontal axis (the same horizontal axis for both particles), they will give *identical* results. Again, this is not supposed to be obvious from the form of the state; one has to do a little algebra. I present these two states because one might be tempted to think they are the same – that the sign joining the two terms in the superposition might not be important. But as we see, it makes a significant difference. By the way, it is possible to change the first state into the second state simply by rotating one of the two electrons by 180° around the vertical axis. The rotation changes the anticorrelation of the singlet state into a positive correlation for all the horizontal directions. We will use this fact shortly in our communication examples.

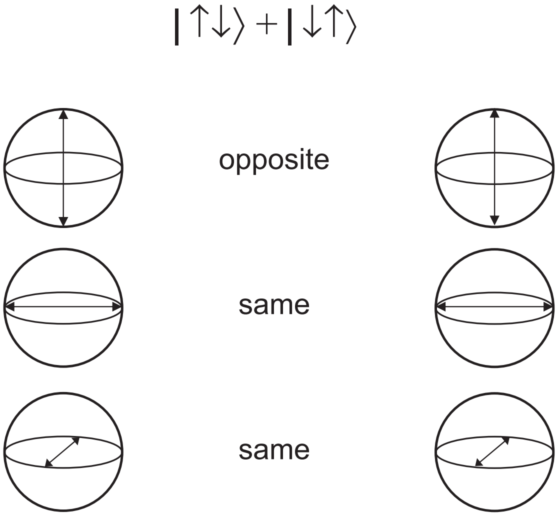

Fig. 11.2. Changing the sign in the superposition has the effect of changing the negative correlation along the horizontal directions into a positive correlation.

Moving now beyond the mathematical description of entanglement, how does one make entangled pairs in real life? In most experiments on entanglement, the particles of interest are photons, and it is usually their polarizations that are entangled. The most popular method nowadays for producing entangled photons is downconversion: a pulse of ultraviolet light impinges on a non-linear crystal. Most of the photons in the pulse pass straight through, but a few are split into two lower-energy photons that go off in different directions. If these directions are selected properly, the two daughter photons will have their polarizations entangled.

One can also entangle atoms. I will describe briefly an experiment done by Hagley et al. in the group led by Serge Haroche [3]. They worked with Rydberg atoms, that is, atoms in which a single electron is in a highly excited orbit around the rest of the atom. Imagine two such atoms, one excited to the $n = 50$ level and the other to the $n = 51$ level. (These are the only two levels that will be relevant to this experiment.) The two atoms travel, one behind the other, toward a microwave cavity that initially has zero photons in it. (The cavity is simply a pair of mirrors, kept sufficiently cold that the thermal energy is unlikely to produce a photon resonating between them.) The first atom to enter the cavity is the one in the higher energy level. The apparatus is adjusted so that this first atom has a 50% chance of decaying to the lower level ($n = 50$) and leaving a photon in the cavity. After the first atom has entered and left, the second atom enters the cavity, and the timing is such that if there is a photon in the cavity, this second atom will certainly absorb it. The net result is this: the extra energy (i.e., the difference between $n = 50$ and $n = 51$) could be found either in one atom or in the other (but not both), and these two possibilities are in a coherent quantum superposition, which is to say that the atoms are entangled. It is not simply that we do not know where the extra energy is; in effect the two mutually exclusive possibilities coexist, in defiance of classical logic.

Let me emphasize the difference between entanglement and mere correlation. Consider again the entangled state $(1/\sqrt{2})(|\uparrow\downarrow\rangle - |\downarrow\uparrow\rangle)$. To say that two electrons are in this state is quite different from saying, "The electrons are either in the state $|\uparrow\downarrow\rangle$ or in the state $|\downarrow\uparrow\rangle$, but I don't know which." In the latter case, there is no correlation along any horizontal axis. Moreover, in the latter case, the electrons will not perform any of the communication tricks we are about to discuss. And this observation leads us nicely to the second part of the chapter.

11.2 Entanglement in Communication

In this section I will go over the three promised examples of communication schemes that use entanglement. But let me begin with two general observations. First, entanglement by itself does not provide a communication channel. Suppose Alice and Bob are some distance apart but share an entangled pair; that is, Alice holds one member of the pair and Bob holds the other. There is nothing Alice can do to her particle that will make any observable difference in Bob's particle. One needs more than entanglement in order to communicate. But as we will see, entanglement can *aid* communication. My second general observation is this: in all the schemes we will consider, the entanglement can be prepared well before anyone knows what message is to be transmitted; so we are not in any sense hiding part of the message in the entangled pair.

11.2.1 Dense Coding

Our first application is called "dense coding," proposed by Bennett and Wiesner in 1992 [4]. Suppose Alice is trying to send Bob ordinary classical information – a string of zeroes and ones – but she wants to encode this information in the spins of electrons. How much information – how many bits – can she fit into a single electron? If no entanglement is present, the answer is: exactly one bit. She can send a bit by encoding zero as “up” and one as “down.” We know that Bob can distinguish “up” from “down”; so he can tell whether she sent a zero or a one. But she cannot encode, say, the four possible messages 00, 01, 10, 11 (this would count as two bits) in a single electron’s spin, because there do not exist four spin directions that can be distinguished perfectly. (Actually the proof that one bit is the maximum possible is quite involved [5], because one has to rule out sophisticated probabilistic schemes that could conceivably do better.)

On the other hand, if Alice and Bob share an entangled pair of electrons, then Alice *can* encode two bits in a single electron. The scheme is illustrated in Fig. 11.3. Alice and Bob share a pair of electrons in the singlet state – the deeply opposite state – which may have been prepared years ago. (Again, Alice holds one member of the pair and Bob holds the other.) When Alice finds out which of four possible messages she wants to send, she performs one of four operations on her electron, the operation being determined by the message. She then sends her electron to Bob, so that when he receives it, he will be holding both electrons. Bob now performs a measurement on the pair of electrons to determine which operation Alice performed and therefore which message she intended to send.

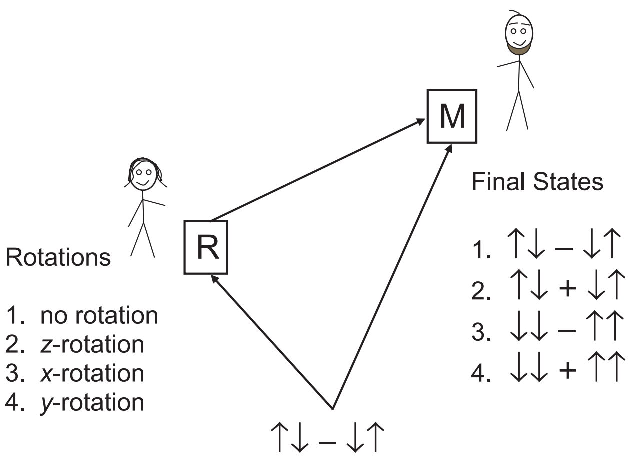

Fig. 11.3. Dense coding: Alice’s message is encoded in her choice of rotation R applied to a single electron. Bob can distinguish, via his measurement M, all four possible states once he is in possession of both electrons.

Here are the details. As I have said, the starting state of the pair of electrons is the singlet state $(1/\sqrt{2})(|\uparrow\downarrow\rangle - |\downarrow\uparrow\rangle)$. The four operations that Alice might perform are:

1. the identity operation, that is, the null operation
2. a 180° rotation around the vertical axis (the z axis)
3. a 180° rotation around the x axis
4. a 180° rotation around the y axis

Corresponding to these operations, there are four possible resulting states of the pair of electrons:

1. $(1/\sqrt{2})(|\uparrow\downarrow\rangle - |\downarrow\uparrow\rangle)$
2. $(1/\sqrt{2})(|\uparrow\downarrow\rangle + |\downarrow\uparrow\rangle)$
3. $(1/\sqrt{2})(|\downarrow\downarrow\rangle - |\uparrow\uparrow\rangle)$
4. $(1/\sqrt{2})(|\downarrow\downarrow\rangle + |\uparrow\uparrow\rangle)$

The second of these was mentioned earlier: a 180° rotation around the vertical axis changes the sign of the superposition. The results of the other two rotations, though by no means obvious, are, I hope, at least plausible. Now, it is a fact that the four states listed above are mutually *orthogonal*, which means that they are perfectly distinguishable from each other, just as the states $|\uparrow\rangle$ and $|\downarrow\rangle$ of a single electron are perfectly distinguishable. Therefore Bob, once he is in possession of both electrons, can in principle perform a measurement to determine which of the four above states the pair is in, and thereby to determine which of the four possible messages Alice sent. This is dense coding: Alice sent Bob only a single electron, and yet she managed to convey one of four possible messages, i.e., two bits of information.

One might object that the preparation of the entangled pair in the first place had to involve another transmission: e.g., Alice may have prepared the pair in her lab and then sent one of the electrons to Bob. Should we not count that electron as a potential carrier of information? We could count it only if we are willing to accept backward-in-time transmission, because by the time Alice knew what message she wanted to send, that electron had already been received by Bob. At the actual moment of communication, only one electron was sent from Alice to Bob, and somehow it delivered two bits of information. That's quite a trick.

11.2.2 Teleportation

The second application is called teleportation and was proposed by Bennett and others in 1993 [6]. It is motivated by a scenario different from that of dense coding. We still imagine Alice and Bob as sender and receiver, but now Alice is trying to convey not a classical message, but rather a quantum state. She has been given an electron with its spin pointing in some direction represented by a state vector $|\psi\rangle$, and in the end, she wants Bob to have an electron with its spin pointing in this same direction. How does she accomplish this? She could simply put the electron in a box and send it to Bob. Unfortunately, though, she does not know where Bob is, and he cannot tell her where he is. (He is a secret agent.) Perhaps she could broadcast the spin direction by radio to all the places Bob might be, and then once Bob receives the message he could prepare an electron of his own having the correct orientation. Unfortunately, Alice does not know the spin direction. She was given the electron by Carol, who was not at liberty to tell her the direction. Moreover, it is impossible for Alice to find out the spin direction by any measurement on the electron. She could make the up-versus-down measurement, but then the measurement would change the electron's state, as we have said before.

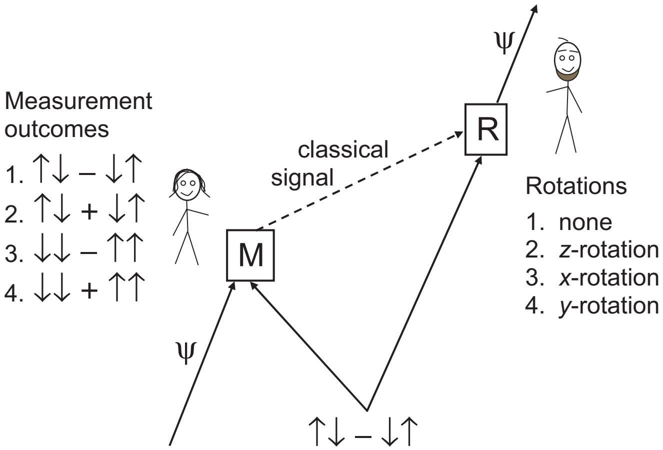

Fig. 11.4. Teleportation: Alice performs a four-outcome measurement on a pair of particles and tells Bob the outcome. Bob then applies an appropriate rotation to his particle to complete the teleportation. The net effect is to convey an unknown state $|\psi\rangle$ from Alice to Bob.

The only solution is teleportation, shown in Fig. 11.4. Again assume that Alice and Bob share a pair of electrons in the singlet state, prepared long before Carol gave her particle to Alice, back when Alice and Bob were at the same place, for example. I like to imagine Alice carrying her half of the entangled pair around with her in a suitcase, while Bob carries his half in his own suitcase. Even if Alice and Bob get far apart, the electrons in their suitcases are still entangled with each other.

Now, sometime after Carol has given Alice the particle whose spin state $|\psi\rangle$ is to be teleported, Alice takes her two electrons – the one she got from Carol and the one she has been carrying in her suitcase – and performs a joint measurement on them. The measurement is exactly the one that Bob performed in the dense coding scheme. In effect she is asking her two particles which of the following four states they are in:

1. $(1/\sqrt{2})(|\uparrow\downarrow\rangle - |\downarrow\uparrow\rangle)$
2. $(1/\sqrt{2})(|\uparrow\downarrow\rangle + |\downarrow\uparrow\rangle)$
3. $(1/\sqrt{2})(|\downarrow\downarrow\rangle - |\uparrow\uparrow\rangle)$
4. $(1/\sqrt{2})(|\downarrow\downarrow\rangle + |\uparrow\uparrow\rangle)$

In actuality they are not in *any* of these states. These four states are all entangled states, but Alice's two particles have never seen each other before and they are definitely not entangled. However, this is a *quantum* measurement, that forces the particles to choose one of the specified outcomes. So they choose. In fact, they choose randomly: the chosen outcome is not at all correlated with the actual state of Carol's particle.

Let us suppose that the particles choose the first outcome; that is, they say (in effect) to Alice, "We have decided to be in the singlet state." Then from this moment on, according to the mathematics of quantum mechanics, it turns out – this is not supposed to be obvious – that the particle that has been sitting all this time in *Bob's* suitcase can be regarded as being in the state $|\psi\rangle$ that was originally embodied in Carol's particle. That is, if Alice happens to get the first of the four possible outcomes of her measurement, she can be sure that Bob's particle now has the desired spin direction.

On the other hand, if Alice gets one of the other outcomes, she must regard Bob's particle as being not in the original state $|\psi\rangle$, but rather in a rotated version of that state. The rotation is by 180° around the z, x, or y axis, depending on whether she got outcome number 2, 3, or 4. Thus, to complete the teleportation, Alice broadcasts her outcome, and when Bob hears her message, he either leaves his particle as it is (if Alice got the first outcome) or rotates his particle by 180° around the appropriate axis (if Alice got one of the other outcomes). In the end, Bob's particle is guaranteed to be in the state $|\psi\rangle$.

There are a number of remarkable features of this teleportation scheme. First, neither Alice nor Bob, nor anyone else, ever learns the spin state $|\psi\rangle$ of Carol's particle. Bob ends up holding an electron having exactly this spin state, but he does not know what this state is. Second, the message that Alice actually broadcasts, the one that Bob needs in order to complete the teleportation, contains no information about the teleported state: recall that the outcome of Alice's measurement is entirely random. Finally, it is remarkable that Alice sent nothing to Bob but two bits of classical information: she told Bob which of four possible outcomes she obtained. And yet the state that Bob finally possesses is one of a *continuum* of possible states: the spin of Carol's electron could have been pointing in any direction. Clearly the entanglement is responsible for the success of the transmission, and yet the entanglement was set up long before Alice possessed the particle whose state was to be teleported. Again, this is quite a tricky business.

By the way, in case you are expecting later in this chapter a simple explanation of these remarkable feats of entanglement, I should warn you now that you will be disappointed. Even if one were to go through the math, which I will not do in this chapter, the mystery would remain. All of the phenomena I am describing are simple consequences of the basic laws of quantum mechanics, but these basic laws are contrary to much of the intuition we have gained about the physical world from our everyday experience.

11.2.3 Pooling Separated Data

The final application is in the pooling of separated data, more commonly called the communication complexity problem. The particular version of the problem that I will describe here was invented by Buhrman, Cleve and van Dam in 1997 [7]. The scenario is quite different from the ones we have been imagining. Again we have Alice and Bob, and now Carol has joined the scene as a regular participant. Each of them has been given an integer (by David, I suppose, or to fulfill a different pattern, by Ted), and the person who gave them the integers has made sure that the sum of the three integers is an even number. Each participant knows only his or her own integer and that the sum is even. Their object is to figure out whether the sum is also divisible by four. That's the game. Moreover, we insist that all three participants, not just one of them, must learn whether the sum is divisible by four.

Clearly in order to accomplish this task the participants must communicate with each other. The communication complexity question is this: How much communication do they need in order to succeed? We will quantify communication in terms of "one-bit broadcasts." A one-bit broadcast is the transmission of one bit of information from one of the participants to each of the other two. For example, if Alice says, "Bob and Carol, my number is even," this counts as a one-bit broadcast, because she has conveyed one of two equally likely possibilities. I will not go through the analysis of this problem. One can check, though, that in a classical setting, that is, without any entanglement, the participants must use at least four one-bit broadcasts to make sure that each of them finds out whether or not the sum is divisible by four.

Now we bring entanglement into the picture. We imagine that, well before the game was set up, Alice, Bob and Carol prepared three electrons in the entangled state $(1/\sqrt{2})(|\uparrow\uparrow\uparrow\rangle + |\downarrow\downarrow\downarrow\rangle)$, and divided this entangled triple of electrons among them, so that each participant holds one electron. At this point David comes along and sets up the game, giving each participant an integer. The rules are the same as before, and we quantify communication just as before. We do not even allow Alice, Bob and Carol to send each other electrons or any other quantum particle. They can send only classical information. But there is a way in which they can use their entangled particles: as they make their decisions about what classical information to send to each other, each participant can perform a measurement on his or her electron and use the outcome to inform his or her decisions. Of course the electrons contain no information whatsoever about the values of the integers. They were prepared before David showed up. But remarkably, it turns out that by performing appropriate measurements and using the outcomes to guide their communications, the participants can accomplish the desired task using only three one-bit broadcasts, i.e., one fewer than what they needed without entanglement. Again I will not go through the argument, which can be found in Ref 7.

Note that ordinary correlation would not have helped them at all. If each had been given a marble and told that either all three marbles were black or all three were white, this information would not have reduced the amount of communication necessary. After all, to be told that all three were in fact black would constitute even more information, and yet it quite clearly would not help them solve their problem, because the marbles have nothing to do with the integers they have been given. So it is not just correlation that is helping them; it is the very special kind of correlation that is embodied in a set of entangled particles.

In the above scenario the improvement due to entanglement may not seem very impressive – from four bits to three bits. But in other versions of the problem, one can make the improvement arbitrarily large, in the sense that the communication required in the presence of entanglement can be made an arbitrarily small fraction of what is required without entanglement [8].

11.2.4 Summary of the Three Schemes

It is interesting to classify the three communication scenarios we have just discussed according to the sort of message that is to be sent and the kind of medium one is using.

In dense coding, one is trying to send a classical message – recall that Alice wanted to convey one of four possible messages to Bob. So the message is classical, but the object that is actually sent from Alice to Bob is an electron, which is a quantum object. Thus the medium is quantum mechanical.

In teleportation the situation is the exact opposite. Alice is trying to convey a quantum state – the direction of spin of an electron – so the message is quantum mechanical. But all that is actually sent from Alice to Bob is a pair of classical bits that tell him the outcome of her measurement. Thus the message is quantum mechanical but the medium is classical.

Finally, in the pooling of separated data, which may be the most impressive of all of these schemes, both the medium and the message are classical. The problem involves only classical information: one is supposed to find out something about the sum of three integers. And the participants are allowed to send only classical messages. Nevertheless, they can be helped by the presence of shared entanglement.

The following table summarizes this classification of the three modes of communication.

mode	message	medium
dense coding	classical	quantum
teleportation	quantum	classical
pooling data	classical	classical

Thus the three communication scenarios we have considered highlight three distinct ways in which entanglement can aid communication. I cannot think of a setting in which both the message and the medium are quantum mechanical and where entanglement enhances the communication, but it is not clear to me that such a scenario is not possible.

Note also that in each of the three cases we have considered, the entanglement is *used up* in the course of communication. In this sense entanglement really is a resource, like the electricity that comes to your home. In order to keep using it, one needs to keep supplying it.

11.3 A Teleportation Experiment

By now a number of research teams have performed teleportation experiments, and I would like to describe one of them briefly. First, though, we need to think about how to carry out the hardest part of the procedure, namely, Alice's measurement.

Recall that in teleportation Alice needs to make a joint measurement on two particles, in effect asking them which of four entangled states they are in. The particular measurement she needs to do is called a Bell measurement. This turns out to be a very difficult measurement to perform. But it is not so hard to perform a *partial* version of the measurement on a pair of photon polarizations, and this is what I want to discuss now.

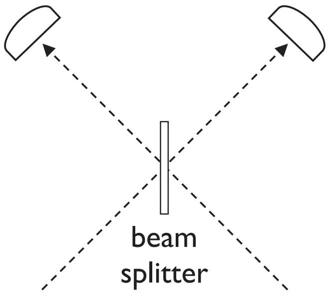

Fig. 11.5. Two identical photons approach a beam-splitter. Because of a two-photon interference effect, both photons must land in the same detector.

Consider a pair of photons – and for now we will assume that they both have the same polarization, say, along the vertical axis – following converging paths and meeting each other on opposite sides of a 50-50 beam-splitter as shown in Fig. 11.5. Each photon could be either reflected off the beam-splitter or transmitted, and there are two detectors that will detect each photon in one of the two possible outgoing paths. A very interesting question is this: what is the probability of the two photons landing in opposite detectors? The most reasonable answer, if one does not take into account quantum mechanics, is one-half: each photon taken by itself has a 50% chance of landing in either detector; so the two photons should have a 50% chance of landing in opposite detectors. In fact the correct answer is 0%. The two photons always land in the same detector. This is because, when one computes the probability of the photons landing in opposite detectors, one has to take into account the two ways in which this can happen: (i) both photons are reflected, or (ii) both photons are transmitted. It turns out that these two possibilities interfere destructively with each other, so that the net probability of the event is zero. (There is a 90° phase shift associated with each reflection; so the process involving two reflections has a quantum amplitude shifted by 180° with respect to the other possible process, thereby leading to a cancellation.)

The same is true if the two photons approach the beam-splitter in an entangled polarization state, as long as the entangled state is symmetric under interchange of the two photons. However, if the photons are in the antisymmetric entangled state $(1/\sqrt{2})(|\updownarrow\leftrightarrow\rangle - |\leftrightarrow\updownarrow\rangle)$, then the extra negative sign changes the destructive interference to a constructive interference, and the two photons *must* land in opposite detectors. Thus, if two photons approach a beam-splitter in this way and are found to land in opposite detectors, one has effectively made the measurement that distinguishes the "deeply opposite" state

$$(1/\sqrt{2})(|\updownarrow\leftrightarrow\rangle - |\leftrightarrow\updownarrow\rangle)$$

from the three symmetric states

$$(1/\sqrt{2})(|\updownarrow\leftrightarrow\rangle + |\leftrightarrow\updownarrow\rangle),$$
$$(1/\sqrt{2})(|\updownarrow\updownarrow\rangle + |\leftrightarrow\leftrightarrow\rangle),$$
$$(1/\sqrt{2})(|\updownarrow\updownarrow\rangle - |\leftrightarrow\leftrightarrow\rangle).$$

Ideally, Alice would like to distinguish all four of these states from each other – this would be the Bell measurement – but it is interesting even to be able to distinguish the first one. Recall that in teleportation, this first state is the one corresponding to the outcome for which Bob does not need to perform any rotation on his particle; his particle automatically goes into the state that was to be teleported. Thus in a teleportation scenario, if Alice lets her two photons meet at a beam-splitter as above, and if they happen to land in opposite detectors, then Bob's photon is guaranteed to end up in the desired state. (Here I am making use of the mathematical equivalence between electron spin and photon polarization. I described teleportation earlier in terms of electron spin, but the experiment is done with photon polarization.)

Exactly this strategy was used by Bouwmeester et al. in the group led by Anton Zeilinger [9] in their 1997 teleportation experiment, shown in Fig. 11.6. The entangled pair is a pair of photons produced by down-conversion. One member of the pair goes to Alice and the other to Bob. The pulse that produced that pair is reflected back to the non-linear crystal to produce another pair, one member of which will be

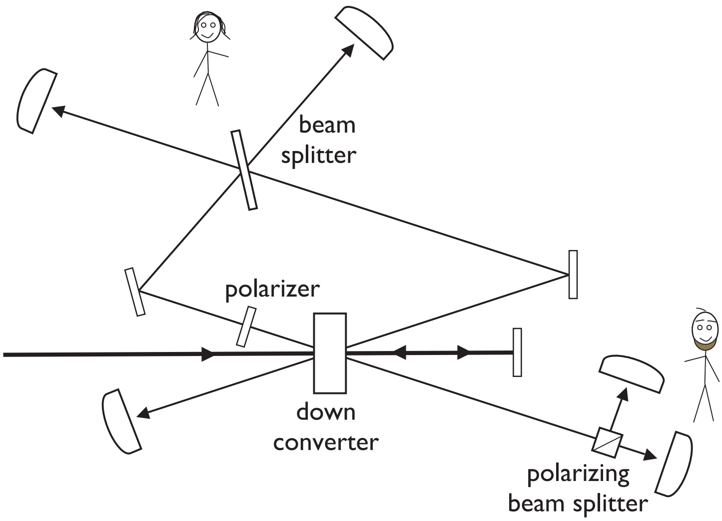

Fig. 11.6. An actual teleportation experiment, performed by Bouwmeester et al. (Ref 9). The beam-splitter and detectors at the top perform a partial Bell measurement. Ideally, when these two detectors fire in coincidence, the teleportation is successful and Bob's photon should have the desired state, that is, the state defined by the polarizer. Bob's apparatus at the lower right is for making sure that Bob's photon arrives, and for testing its polarization. In one version of the experiment, the detector at the lower left is used, in effect, to "warn Alice" that a photon is coming.

Carol's photon, the one whose polarization state is to be teleported. The polarization of Carol's photon is determined by the angle of the polarizing filter shown in the figure. (If the photon does not get through the filter, then there is nothing to teleport; but the photon will pass through the filter about half of the time.) At the top of the picture you see Alice's measurement: she lets the two photons come together at a beam-splitter and checks to see whether they land in opposite detectors. If they do, then Bob's photon in the lower right-hand part of the picture (the particle we imagined to be in his suitcase) should come out with the same polarization that was given to Carol's photon. In practice the teleportation is not perfect, of course, but one understands the sources of error, and the principle of teleportation is indeed confirmed.

Many other teleportation experiments have now been performed; I mention here just a few of them. In an experiment performed around the same time as the one I have described, the two "particles" measured by Alice were realized in two different degrees of freedom of the same photon [10]. In several other experiments, the first of which was reported in 1998, the quantum variable being teleported was not a discrete variable such as photon polarization or electron spin, but a continuous variable realized in a coherent state of a beam of light [11]. There have been teleportation experiments performed over kilometers of optical fiber [12]. And more recently two groups have implemented the full four-outcome Bell measurement to achieve teleportation in a system of trapped ions [13].

11.4 Conclusion

We have seen that entanglement, in addition to being an amazing physical phenomenon, can also be useful as a resource for communication. It is a resource like any other resource, in that it can be used up and replenished. I find it remarkable that, even though entanglement by itself does not constitute a communication channel, the presence of entanglement allows modes of communication that are not possible without it. Moreover, as we have seen, these modes are quite different from each other with regard to the nature of the medium and the nature of the message. In this sense, entanglement is a versatile resource.

If entanglement is a resource, then one should be able to quantify it, just as one can quantify other resources such as energy and information. Indeed, a whole literature has sprung up in recent years addressing problems in the quantification of entanglement, and one can even begin to speak of laws of entanglement [14]. The pace of research on entanglement has become quite rapid, and I think it is fair to expect many more novel ideas on the subject in the coming years.

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12

The Three Cases of Doctor von Neumann

Roland Omnès

I know that speaking of oneself is generally improper, but let me tell you something about my name. *omnes* is a Latin word. It means "everybody," like you and me. Unfortunately, few people speak Latin and when they hear the sound of this name, they spell it randomly, particularly in English-speaking countries where the result is often "Holmes." It happened again recently and that gave me the idea of telling you a Holmes story. The only murdered person is a cat, but there is nonetheless a mystery of the best kind in it.

12.1 Setting the Stage

As we read in Chapter 2, Max Planck discovered quanta in the year 1900. After only a quarter of a century, several young men found the basic rules of these quanta. The quantum mechanics they obtained was a real wonder and solved lots of problems, old and new; but a few inquiring people dug deeper into its meaning and what they saw frightened them. It sounded like a highly dangerous doctrine, shattering past wisdom, negating causality, asserting that electrons have no trajectories and, as a matter of fact, standing so much at variance with common sense in so many ways that some of its founders could not believe it.

Niels Bohr tried to protect us from the main dangers of that Thing. He drew a safety line around the macroscopic world, which was supposed to remain true to common sense and to provide a harbor of sanity. The atomic world, on the contrary, was left to schizophrenia. In it you may believe you are speaking of a single object, with a position and a velocity, and be told suddenly that this One is Two. There is one electron with a position or a velocity, a wave or a particle. This double-talk is called "complementarity".

The obvious remedy, of course, is to resort to mathematics and to rely only on a wave function and a Schrödinger equation. But then comes the rub. How do we know the wave function? From a measurement you might say. But a measurement device is macroscopic and Bohr had identified the macroscopic realm as strictly classical, as a pure matter of common sense, and common sense has no place for wave functions! Bohr was therefore compelled to the strange and deep idea of wave packet collapse (or reduction), thereby violating the Schrödinger equation – one of the most basic rules of the theory he was supposed to interpret.

12.2 Von Neumann's Three Enigmas

Einstein and Schrödinger disagreed strongly with Bohr, as you know, but I must shorten the story and leave these great men aside. Let me rather introduce another actor, who is the main character in my way of telling the story: John von Neumann. He published a great book on the foundations of quantum mechanics in 1932, although he was not known at that time as a physicist, but rather as a mathematician and logician [1]. It will be also important for our story to note that he had been much influenced by David Hilbert.

As you may know, Hilbert was not only a mathematician but also a philosopher of mathematics and of everything having to do with it. He therefore had his own idea of what a physical theory should be, an idea that was considered rather strange at that time although it is now becoming commonplace. According to Hilbert, a physical theory must first assert clearly and explicitly its basic principles (including its logical framework) as one does with axioms in mathematics, then proceed from there in a rigorous and purely deductive fashion so as to obtain every relevant consequence.

It is not clear whether von Neumann shared Hilbert's grand view when working on his book: did he look for a deductive formulation of quantum mechanics, including its interpretation? Or was his ambition solely to help in a great enterprise? We do not know for sure because, at that time (long ago), scientists were supposed to publish bare results and no speculation. Let us assume however that von Neumann had at least for one moment, or even in a dream, contemplated the possibility of a logical and deductive understanding of quantum mechanics. Whether this is true or not, it will at least make the story much more dramatic, as a story should be. So, von Neumann began his work as one might expect from so good a mathematician. He introduced the Hilbert space formalism as now given in textbooks, considerably improving the theory of operators for that purpose. Perhaps less intuitively than Dirac but in full accordance with Hilbert's requirements, he brought clarity to the basic mathematical principles of quantum physics: its axioms.

Most physicists were not then much inclined towards the resulting formulation of quantum physics. It looked too abstract for them. Von Neumann had found, however, a great idea for bringing his abstraction closer to common sense. He had noticed that every description of a quantum process can always be expressed, very simply, by using standard sentences, each one of them stating that "the value of an observable A lies in a domain Δ of the real numbers at a time t." He named a sentence of that kind an elementary predicate.

His most important remark was that every predicate is associated with a definite projection operator in Hilbert space (projecting on a subspace that is spanned by the eigenvectors of A which have eigenvalues in Δ). Projection operators have only two possible eigenvalues: 1 or 0, which can mean "true" or "false" as they do now on a computer. Von Neumann had therefore found a bridge between the abstractness of quantum principles and the language of common sense, with a convenient new language for physics.

Had he also found thereby the key to a deductive interpretation? He had not, apparently, because he encountered almost immediately three dire difficulties:

• He was unable to extend satisfactorily his language to include classical statements. A classical property does not typically refer to a unique physical quantity, such as a position or a momentum observable, but involves the two of them together. Even if one allowed for "errors" in the values of these quantities, by giving them enough latitude for the Heisenberg uncertainty limits to be considered very small, there was no direct expression of a classical property by means of a projection operator. It looked therefore as if predicates cannot provide a universal language for physics and they must remain confined to microscopic quantum properties.

• The fact that "1" and "0" are given the logical values "true" and "false" is far from enough for asserting a satisfactory logic. As a matter of fact, von Neumann discovered to his dismay that a language in which every elementary predicate is considered as a possible proposition cannot satisfy the basic rules of standard logic! Rather than reasserting some measure of common sense, he had only proved apparently that such an enterprise is impossible. His new language did not make sense from the standpoint of logic, unless one is ready to use non-standard logic. But this is not a step to which many physicists are inclined.

• The final blow was reported in the last two pages of von Neumann's book, where he described a simple mathematical model for a measurement. There he found that a superposition of two microscopic states, so frequent for atoms, is amplified to yield a superposition of two macroscopic events after a measurement. This essential remark became famous a few years later when Schrödinger turned it into a dramatic story by introducing a cat as a part of a measurement device. Stranger than the Cheshire cat of Lewis Carroll, the poor animal was simultaneously dead and alive!

Finally, when the book by von Neumann was published, it endorsed completely the Copenhagen interpretation. It contained no mention of a deductive interpretation. Worse than that, the idea was never put forward, even as an assumption, till 1988 [2]. The reluctance to embrace the idea was strong, however, and I remember a private conversation with John Bell in the following year. Always precise and sharp, he asked "What is the idea, in a nutshell?" The answer was: "The principles of quantum mechanics are enough to provide their own interpretation." The response came immediately: "No. That's impossible!"

12.3 Three Lines of Investigation

Let us come back to our story, for which the stage was fully set after von Neumann's three dramas. A crime had been committed, with a victim strangely dead and alive. Perhaps it was not quite the plot of a detective story but rather of a fantasy, since logic had become mad and the ordinary world of classical reality was still estranged. (As a matter of fact, after the invention of the many-worlds interpretation, we might say it had become science fiction.) I promised you, however, something in Hercule Poirot's or Holmes's style with a solution at the end. So let us see how the little gray cells of many industrious people have managed to solve the three cases that had been unearthed by Doctor von Neumann, and how reason was finally put back on her feet in the last chapter (although, of course, there is never a last chapter in physics).

12.3.1 Macroscopic Superpositions

The three problems were taken one by one. Let us begin with the case of the cat, i.e., macroscopic quantum superpositions. Very early, Heisenberg had noticed that the approach by von Neumann relied explicitly on a model of a measuring device with only one degree of freedom. Schrödinger's considerations, though less explicit, concentrated also on a unique characterization of the cat: dead or alive. A real macroscopic device, on the contrary, typically involves billions of billions of particles and many more. Can there be then a mechanism that results from such a huge complexity and that destroys quantum interferences? Several valuable suggestions were made from time to time in that direction and the most promising one was proposed by Hans Dieter Zeh as the so-called decoherence effect [3].

My own intuitive understanding of the effect is as follows. Imagine for instance a measuring device involving an old-fashioned voltmeter with a dial and a pointer. It contains many billions of atoms, all of them obeying quantum mechanics, so that we may think of its overall wave function, which depends on billions of variables. One among these variables represents the pointer position. Suppose now that the measurement ends up formally in a quantum superposition of two different positions of the pointer. The wave function is then a sum of two functions depending on billions of atomic variables, although each component corresponds to a rather well-defined position of the pointer, say positions 1 and 2. Now look closely at the pointer when it begins to move. If one had measured a pure state for which only position 1 could be reached, the pointer motion would have strongly interacted anyway with the surrounding atoms. There is friction along the pointer axis and at the level of atoms, friction amounts to a catastrophic earthquake. Said otherwise, a motion of the pointer strongly affects the wave functions of many atoms. Consider now what happens to these wave functions when the pointer reaches either position 1 or 2. They are very complicated and their local phases are very different (hence the name of "decoherence"). And what can one expect of two very complicated functions of many variables if they are significantly different? They are most probably orthogonal!

Let us take it for granted that this orthogonality is enough for destroying macroscopic interferences. Notice also that this way of looking at things suggests that there may be no decoherence if there is no friction at the level of atoms. Decoherence is furthermore a dynamical process, which takes some time for acting. However, the trouble with the description we have just given is that one cannot easily turn it into a theory, because we have no handle on the phase of a wave function in a many-body system. Quantitative investigations had therefore to rely on simplified models [4, 5, 6, 7] or on the methods that had been devised for the study of quantum irreversible processes [8] (decoherence is certainly irreversible). To cut a long story short, it was thus found that decoherence is by far the most efficient quantum effect with action at a macroscopic level. It is so rapid that it defied observation for a long time, because it had destroyed interferences long before they could be observed. Only recently a clever experiment has succeeded in detecting and measuring the effect, in good agreement with theoretical predictions [9]. With this result we can say that most of the suspense over the cat's murder ended (although decoherence remains of course a lively field of research).

12.3.2 Classical Properties

One of the troubles with von Neumann's language was its lack of universality, because it (apparently) could not express classical statements. A first step in that direction was made by Hermann Weyl, who in some sense related the vocabularies of quantum and classical physics [10]. Given a quantum observable A, he defined a "classical" or rather a classically meaningful dynamical variable $a(x, p)$ corresponding to it (through a Fourier transform of the matrix elements of A in the position basis). The algebra of operators became then a "Weyl calculus" for the functions of (x, p). Things lay there for almost twenty years till mathematicians developed a new branch of mathematics, "microlocal analysis" also called "pseudo-differential calculus," in which Weyl calculus was integrated and considerably developed.

In the meantime, significant progress had been made in semi-classical physics with the introduction of coherent states, allowing a derivation of classical electromagnetism from quantum electrodynamics. However, the derivation of standard classical physics remained a problem, because classical physics deals simultaneously with position and momentum quantities, which do not commute. Typically, a classical statement asserts that the values of x and p belong to some cell in phase space, which may be, for instance, a rectangle with half-sides Δx and Δp (with $\Delta x \cdot \Delta p \gg h$). More generally, one may consider a cell (i.e., a closed and simply connected domain in phase space) as being classical ("regular") if it is big enough (in units h) and its boundary is smooth enough ("enough" having of course a precise quantitative expression).

On the forefront of physics, there had been some progress in the formulation of classical statements by means of coherent states, but they were not convenient for a description of dynamics [11]. The solution came in fact from the camp of mathematicians for their own purposes, and its application to physics took some time. A theorem in microlocal analysis by Lars Hörmandergave the clue for an extension of von Neumann's language [12]. It says that although a classical statement (i.e., a regular cell) is not associated with a unique projection operator in Hilbert space, it can be related with a family of such projections, all of them equivalent in a well-defined sense. The meaning of this theorem is essentially that one can "speak classically" in the von Neumann language!

Another important theorem by Yuri Egorov enlightened the meaning of classical determinism [13]. Grossly speaking, the theorem is concerned with a regular cell C which becomes another regular cell C' through classical motion during a time t. Then, according to Hörmander's theorem, one can consider a projection operator P associated with C and another P' associated with C'. Egorov's theorem says that P' and P are related together through the unitary evolution of quantum mechanics during a time t.

Of course, these theorems are rather abstract and they must be interpreted for their application to physics [14]. Hörmander's theorem already told us that we may speak classically in von Neumann's language. Egorov's theorem means that this way of speaking agrees with the time evolution in classical dynamics, and therefore with determinism. Of course, there are limitations and some errors are involved. This is best seen with the status of determinism. When expressed in the probabilistic framework of quantum mechanics, determinism receives a probabilistic meaning because there is always a finite probability for its assertion to be wrong (think of quantum fluctuations). This probability is however known and it is extremely small in most circumstances. The derivation of determinism by means of Egorov's theorem is therefore quantitative and, interestingly enough, it fails in two cases of physical interest. It does not apply to strongly chaotic motion or in the presence of narrow potential barriers. This last case was the occasion of a very clever experiment, which has indeed shown macroscopic systems (SQUIDs) exhibiting a quantum behavior [15, 16]. Finally, one obtains a nice agreement of the theoretical developments with common sense as well as with refined experiments, explaining the validity of determinism and limiting it explicitly.

12.3.3 Logic and Consistent Histories

What about standard logic, which did not seem to agree with von Neumann's language? That was the last problem and its main solver was Robert Griffiths with the discovery of consistent histories [17]. Let us try to explain this idea. Suppose we read a good experimental paper in *Physical Review*. It describes an experimental apparatus and how the various pieces in it work. We may notice that this is mostly classical physics and we have just seen how to express it in our favorite language. The authors of the paper may also describe some events that happen at a microscopic level. They say for instance, "at that time an atom enters a cavity," "a nuclear reaction now takes place," or "the photon hits a photomultiplier." But we also know how to translate these descriptive sentences by so many projection operators. Finally, we can write down a sequence of projection operators, following each other in the order in which the properties they express do occur in the course of time, some of these properties being classical and others quantum predicates. Such a sequence is called a history. It can be considered, essentially, as an ordinary description of the physical events during an experiment. The only trick is that we speak "von Neumann" as we might have spoken English or German, or we might use the machine language on our computer to recount the same story: everything is only a matter of translating.

The authors of a paper know, however, that they must exert some caution when describing an experiment. Referees would object, for instance, if it were mentioned through which arm of an interferometer a photon has gone during an interference experiment. This means that some descriptions – some histories – are meaningless. But how can we distinguish the good descriptions from the nonsensical ones? Griffiths chose to imbed a history into a "family" involving all the alternative histories that could happen because of quantum randomness. He found that in some families the histories can be assigned probabilities – although not in most families. There was a remarkable coincidence between the two kinds of families and the histories we have learned from Copenhagen as making sense or not. They could be distinguished moreover by explicit equations: the so-called "consistency conditions."

Later investigations have shown that the form of history probabilities is unique, so that the construction relies only on the basic principles, as required in Hilbert's program. Consistency conditions were found to imply the validity of standard logic, thereby removing the main logical stumbling block for a "logical" interpretation [18]. It was also shown that decoherence is by far the most important and frequent reason why consistency, and therefore logical soundness, is satisfied [19].

Elementary, my dear Watson, but it took some time.

12.4 The Last Chapter

The last chapter, when all the characters sit in the same room to hear the answers, has been told several times elsewhere [20]. There is therefore probably little suspense and we may be brief.

Using the four main ingredients (von Neumann's projections, decoherence, the derivation of classical physics, and consistent histories), one can build up a deductive interpretation. It only assumes the basic principles of quantum theory and is therefore in full agreement with Hilbert's program. The main results are:

• A theory of measurement, which is in essential agreement with the Copenhagen rules, although it consists now in established theorems.

• Although several different families of histories can equivalently describe the same quantum experiment (this is an explicit form of the "complementarity principle"), a unique kind of histories can describe a purely macroscopic system with a classical behavior. The logical framework of quantum mechanics coincides in that case with educated common sense, i.e., standard logic relying on classical mechanics. This recovery of common sense is, in my opinion, particularly satisfactory.

• There are interesting consequences concerning the arrow of time and similar questions, which are still to be fully developed.

There are also some controversies concerning the ultimate meaning of decoherence, the status of probabilities and the necessity of introducing histories, but the fact that a breakthrough has occurred is most often agreed, even if its significance is disputed. An important aspect of quantum physics remains however unexplained: Why is there a unique result at the end of a quantum measurement?

This is not exactly the old problem of wave packet collapse, because the Bohr–von Neumann–Lüders rule for successive measurements can now be derived from the first principles, using only decoherence, as far as joint probabilities are concerned. It is really the problem of why physical reality is unique. Is it a problem in physics (i.e., to be solved by new or old developments in physics), or is it a still deeper problem about physics (i.e., intrinsic to the mathematical nature of physical theories)? My own inclination goes towards the second alternative as part of a new program, which is to investigate the consequences of our little story in the direction of epistemology and, in a wider sense, the philosophy of knowledge.

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