Quantum interpretations and world war two

An analysis of the interpretive debate from the twenties to the sixties in historical context

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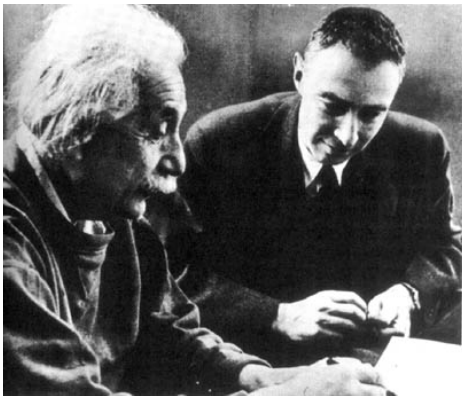


Front page image: Einstein and Oppenheimer, two generations of physicists. Wikimedia commons, image courtesy of US Govt. Defense Threat Reduction Agency

Introduction

The theory of quantum mechanics has puzzled physicists since the early 20th century, when they discovered that the atomic and subatomic world must be described by a theory that is fundamentally different from classical mechanics. It has since been incredibly successful in allowing physicists to describe physical systems with an accuracy that has never been reached before and making possible the development of technologies like semiconductor transistors and more recently quantum cryptography.

The puzzling part, however, is still unresolved. Because after modern quantum mechanics was born in 1925, it was clear that the mathematical formalism of quantum mechanics describes measurement outcomes and not necessarily the underlying physical processes. This has left many physicists and philosophers with a deeply rooted desire to explore the relation between the formalism and its underlying processes, which has led to the development of different interpretations of quantum mechanics.

The discussion of conceptual issues and interpretations finds place in a field that is called *the foundations of quantum mechanics.* It was highly popular until the second world war, after which the interest for the field reduced. Around this time, the scientific focus changed from foundational considerations and analytical methods to more pragmatic and numerical methods.

In this thesis, the main interpretive issues between the '20s and '60s are discussed, as well as the developments that led to the reduction of the foundational field, the increase of pragmatic methods and a conservative mentality around the war.

Structure of this thesis

You will find two highly interwoven narratives in this thesis.

Firstly, I will discuss the philosophical issues regarding the interpretation of quantum mechanics. I begin with a brief discussion of old quantum theory, and discuss the quantum revolution in the '20s, the following debates and the ideas of the Copenhagen interpretation (CI). Then I discuss two early post-war interpretations called De Broglie-Bohm (DBB) theory and the many-worlds (MW) interpretation and two "no go" theorems called Bell's theorem and the Kochen-Specker theorem. I attempt to give a neat way of comparing and talking about these three (CI, DBB, MW) interpretations, in terms of conceptual issues, ontology and ideology. Then I briefly discuss their viability in quantum field theory. I have tried to clear a way through the different approaches to and misconceptions about interpretations and the implications of no-go theorems, and I give an exposition of foundational matters as I understand them.

Secondly, there is the historical side of things. The main question I try to answer is: to what extent have social and cultural influences affected the interpretive debate? This can be broken up into less general questions. Why was the Copenhagen interpretation so popular in Weimar Germany? How did the second world war and the cold war influence the physics community? What caused the reduction of the interpretive debate and the conservative attitude of physicists from the '40s onwards? I try to make clear what answers the existing literature gives us to these questions, and give my views on them as well.

We will see that in both narratives, in a way, there is a movement from a unified "philosophy-physics" to philosophy and physics as more distinct things. The philosopher-physicist of the '20s has turned into a more pragmatic physicist by the '60s. And where before the war the search for an interpretation with an ideal ontology (or "set of physical entities") and ideology (or "set of philosophical premises") was still considered possible by some, the postwar interpretations are either ontologically or ideologically satisfying, but cannot be both.

This thesis consists of five chapters.

The first chapter covers prewar developments. Bohr's model of the atom established him as an authority on atomic physics. His crude model could not account for many experimental results, and modern quantum mechanics was born after the quantum revolution in the mid-'20s. The new formalism sparked discussions on a variety of interpretations and philosophical issues. The Copenhagen interpretation became the most popular school of thought. Most notably Einstein and Schrödinger raised objections to its issues, such as the measurement problem and nonlocality.

The second chapter covers the war and its impact on the physics community. The war caused a migration of mainly Jewish German physicists to (most importantly) the US, where the physics culture was significantly different from Europe.

The postwar advance of foundations is discussed in chapter three. Two new interpretations were introduced in the '50s and two important "no go" theorems were proven in the '60s. I will compare the Copenhagen interpretation, De Broglie-Bohm theory and the many-worlds interpretation in terms of conceptual issues, ontology and ideology.

In chapter four, the physicist's change of attitude regarding foundations is illustrated by the new foundational work was received. I will discuss the focus of the new generation of quantum physicists on technical research, the rise of numerical methods, and the position of the first quantum generation in the postwar physics community.

In chapter five I will talk briefly about quantum field theory (QFT). The development of quantum electrodynamics had its roots before the war, but its most important advances were made in the '40s and '50s and illustrate the postwar scientific mentality. I will give a brief overview of the possible ontologies for QFT, and discuss whether De Broglie-Bohm theory and the many-worlds interpretations are still viable in this framework.

Existing literature

There are some enormously informative books on the historical development of quantum mechanics. There is Max Jammer's slightly outdated but still useful book *The Philosophy of Quantum Mechanics*, *Inward Bound* by Abraham Pais and Helge Kragh's *Quantum Generations*. I draw from these books very often.

I have found the syllabus *Grondslagen van de Quantummechanica* by Michiel Seevinck of the Radboud University Nijmegen a very good mathematical guide to the no-go theorems; however, this book has not been published.

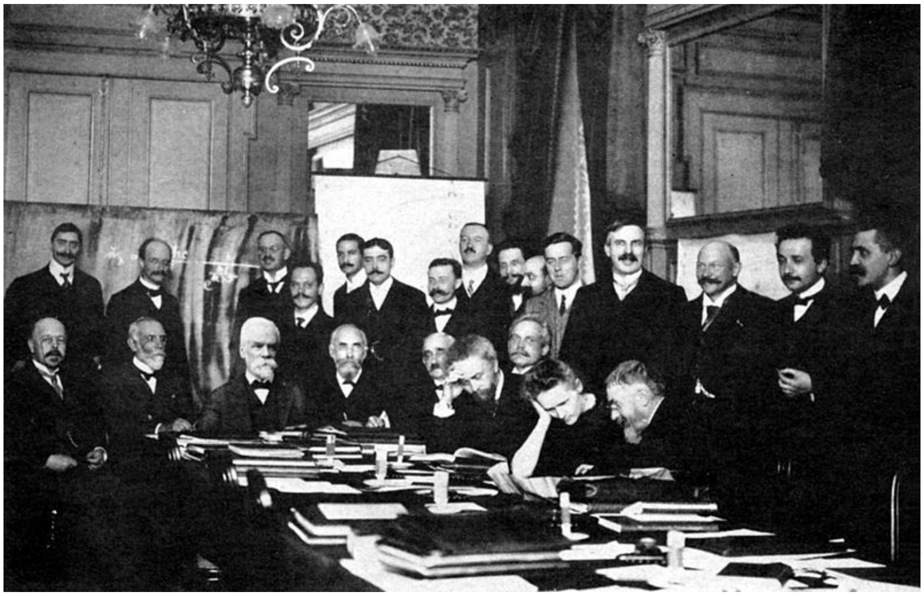
Another book I find very useful is James Cushing's *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony.* I greatly support his story on the historical contingency of the Copenhagen interpretation. I am however very hesitant to commit to one interpretation, and even though Cushing's arguments for De Broglie-Bohm theory are quite compelling, I am not convinced it is a 'better' theory than the many-worlds interpretation.

Even though I only cover the subject very concisely, I have to mention Silvan Schweber's *QED and the men who made it*, about the development of quantum field theory.

And lastly, I draw a great deal from the historian of science Paul Forman, who has written theses (known as the first and second Forman thesis) on quantum foundations in Weimar Germany and cold war America.

Contents

1 Early quantum physics



The participants of the first Solvay Conference in 1911. (Benjamin Couprie, Getty Images Editorial 52194952)

This thesis cannot lack a summary of old (1900 - 1925) quantum physics, which is why the first section of the chapter is devoted to it. Modern quantum mechanics emerged in 1925 when Heisenberg's *Umdeutung* paper was published. The formalism of modern quantum mechanics, which was introduced in the late '20s and early '30s, is covered in section two. This mathematical framework could be interpreted in different ways, which sparked the interpretive debate. I introduce the framework in which I will discuss this debate in section three, cover early interpretations in section four and then describe the Copenhagen interpretation in section five. Section five covers a prewar no-go theorem by Von Neumann. And finally, I discuss the new developments in quantum physics within a wider societal and philosophical context.

1.1 Old quantum theory

1.1.1 The birth of the quantum

On December 14, 1900 quantum theory was born. It was on this day that Max Planck suggested his *quantum postulate* to the Deutsche Physikalische Gesellschaft in order to explain black-body radiation: (Planck, 1900)

$$\boxed{E = h\nu.} \tag{1}$$

It is called the *quantum* postulate because it implies that energy E can be emitted or absorbed only in *quantized* form: one discrete "packet of energy" has an energy of Planck's constant h (about 6.6×10^{-34} m^2 kg/s) times the frequency of radiation ν. In insisting that this had nothing to do with the physical reality of the radiation itself, Planck initially failed to understand the revolutionary implications of this hypothesis (Kuhn, 1978). Einstein was the first person to realize how radically nonclassical this law was. The earliest of his 1905 Annus Mirabilis papers (Einstein, 1905) used the quantum hypothesis to explain the photoelectric effect (Compton effect), which won him the 1921 Nobel Prize. This work showed that light fits both a wave and a particle description, contradicting the classical idea that light is a wave phenomenon. At this point, the wave-particle duality was poorly understood, but generally accepted.

After a slow start, the scientific interest for quantum theory began to grow. Starting in 1911, Ernest Solvay, a Belgian philantropist with an interest in physics, funded a series of conferences in Brussels in which the brightest minds of physics would come together to discuss quantum theory. The first Solvay conference helped to establish a common understanding of the problems in quantum theory. Although the general attitude was quite skeptical, a few specialists recognized the importance of this new field, as illustrated by a quote from a 1911 lecture by Planck:

> The beginning is made: the hypothesis of quanta will never vanish from the world...I do not believe I am going too far if I express the opinion that with this hypothesis the foundation is laid for the construction of a theory which is someday destined to permeate the swift and delicate events of the molecular world with a new light. (Kragh, 1999, ch. 5)

1.1.2 Bohr's atomic theory

In 1913 the Danish physicist Niels Bohr published his model of the atom (Bohr, 1913), in which he used quantum theory to refine Rutherford's 1911 model[1] by stating that electrons orbit the nucleus in quantized orbits. Electrons are able to emit or absorb electromagnetic radiation with an energy of $h\nu_{mn}$, according

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[1] The Rutherford model contained a central charge with a cloud of electrons around it, based on his well-known gold foil experiment.

to the *Bohr frequency condition*:

$$E_m - E_n = h\nu_{mn}, \tag{2}$$

by jumping between orbits with energies E_m and $E_n < E_m$.

An important reason for the success of this model lies in the justification it offers for the 1888 *Rydberg formula* for wavelengths of spectral lines:

$$\frac{1}{\lambda_{mn}} = R\left(\frac{1}{n^2} - \frac{1}{m^2}\right), \tag{3}$$

where λ_{mn} is the wavelength of the emitted light in vacuum, R is the Rydberg constant and $n < m$ are integers. It was known since 1888 that this formula describes the wavelengths of spectral lines of many chemical elements quite well, but it lacked physical explanation at that stage.

In the Bohr model m is simply the number of the initial orbit and n is the number of the final orbit. We can see this as follows.

Firstly, we know that the total energy E equals the kinetic energy plus the electronic potential energy. In the case of hydrogen, this yields:

$$E = \frac{m_e v^2}{2} - \frac{k_e e^2}{r}, \tag{4}$$

with v the electron speed, r its orbit radius, e the electron charge, m_e its mass and k_e Coulomb's constant. With Coulomb's law we find a force that we can write in terms of v and r using the expression for uniform circular motion:

$$F = \frac{k_e e^2}{r^2} = \frac{m_e v^2}{r}, \tag{5}$$

The amplitude of the angular momentum of the electrons is $L = v m_e r$, so

$$E = -\frac{m_e v^2}{2} = -\frac{1}{2}\frac{m_e k_e^2 e^4}{L^2} \tag{6}$$

From the correspondence principle (see next section (1.1.3)) we find a value for the angular momentum in orbit n:

$$L_n = \frac{nh}{2\pi} = n\hbar, \tag{7}$$

so that, using $\Delta E_{mn} = h\nu_{mn} = \frac{hc}{\lambda_{mn}}$:

$$\frac{1}{\lambda_{mn}} = \frac{\Delta E_{mn}}{hc} = \frac{m_e k_e^2 e^4}{2hc}\left(\frac{1}{L_n^2} - \frac{1}{L_m^2}\right) = \frac{2\pi^2 k_e^2 e^4 m_e}{ch^3}\left(\frac{1}{n^2} - \frac{1}{m^2}\right). \tag{8}$$

We have found a value for the Rydberg constant

$$R = \frac{2\pi^2 k_e^2 e^4 m_e}{ch^3}, \tag{9}$$

which is in good accordance with experimental data.

During the first world war (1914 - 1918) the Bohr model was extended and improved by most notably Bohr himself and a German theoretical physicist named Arnold Sommerfeld. Sommerfeld published his *Atombau und Spektrallinien* (Sommerfeld, 1919) in 1919, which became the "bible" of atomic theory (Kragh, 1999, ch. 11). His work was based on action integrals, using the *Sommerfeld-Wilson quantization condition* as the foundation for old quantum theory:

$$J = \oint p dq = nh, \tag{10}$$

where p is momentum and q is position. Hence, Planck's constant is also referred to as the quantum of action.

The Bohr-Sommerfeld theory lacked logical consistency: it was a strange mix of classical and non-classical concepts as the electrons moved according to the laws of classical mechanics, but did so in quantized orbits. However, the spectacular success in explaining the spectrum of the hydrogen atom gave them the status of authorities on atomic theory.

1.1.3 The correspondence principle

In the 1913 paper we see a first *ad hoc* application of Bohr's trademark tool: the *correspondence principle*. It states that quantum theory approaches classical physics for h going to zero[2] or for quantum numbers n becoming big[3]. To show that the angular momentum of the electron L is quantized with a spacing of $\hbar$ (equation 7), Bohr wrote:

> If [the quantum number] N is great the ratio between the frequency before and after the emission will be very near equal to 1; and according to the ordinary electrodynamics we should therefore expect that the ratio between the frequency of radiation and the frequency of revolution also is very nearly equal to 1 (Bohr, 1913).

In other words, if $f = \frac{v}{2\pi r}$ is the frequency of the electron, we can use that $f = \nu$ for large n. Using equations 6 and 5, we can write:

$$\frac{dE}{dL} = \frac{m_e k_e^2 e^4}{L^3} = \frac{m_e k_e^2 e^4}{m_e k_e^2 e^4}\frac{v}{r} = 2\pi f. \tag{11}$$

We know that the energy spacing $dE = h\nu$, so:

$$dL = \frac{dE}{2\pi f} = \frac{h\nu}{2\pi f}. \tag{12}$$

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[2] This idea was first expressed by Planck in 1906: *"The classical theory can simply be characterized by the fact that the quantum of action becomes infinitesimally small."* (Jammer, 1966, ch. 3.2)

[3] Bohr used $n \to \infty$ rather than $h \to 0$.

Using $f = \nu$ we find a value for the spacing of angular momentum dL:

$$dL = \frac{h}{2\pi} = \hbar. \tag{13}$$

According to the correspondence principle this would also hold for small n.

Bohr later explicitly formulated the correspondence principle in his 1918 "On the quantum theory of line-spectra", referring to it as "a formal analogy between the quantum theory and the classical theory" (Jammer, 1966, ch. 3.2).

Besides quantizing orbital momentum, the correspondence principle was successfully used by Bohr for deriving a selection rule and by Hendrik Kramers for calculating intensities and polarizations of hydrogen spectral lines, including Zeeman and Stark effects (Kramers, 1920). Also, the correspondence principle provides a motivation for Sommerfeld's quantization condition:

$$J = \oint p dq = m_e v r \int_0^{2\pi} d\phi = 2\pi m_e v r = 2\pi L = 2\pi \frac{nh}{2\pi} = nh. \tag{14}$$

However, not everyone was equally charmed by Bohr's semi-intuitive principle. It was received skeptically in Germany, where physicists generally adopted a more formal approach. Kramers recalled in 1935:

> In the beginning the correspondence principle appeared to the physicists as a somewhat mystical magic wand, which did not act outside Copenhagen. (Kragh, 1999, ch. 11)

Van der Waerden wrote that 1919-1925 quantum theory research may be described as *"systematic guessing, guided by the principle of correspondence"* (van der Waerden, 1967, Introduction). Similarly, the German physicist Max Born said in his Nobel lecture:

> Theoretical physics maintained itself on this concept [the correspondence principle] for the next ten years. The problem was this: an harmonic oscillation not only has a frequency, but also an intensity. For each transition in the array there must be a corresponding intensity. The question is how to find this through the considerations of correspondence? It meant guessing the unknown from the available information on a known limiting case. (Born, 1954)

1.2 The early years of modern quantum mechanics

By the early '20s there were three main research centers for quantum theory: Bohr's institute in Copenhagen, Sommerfeld's school in Munich and Born's group in Göttingen.

It became clear that the Bohr-Sommerfeld theory was fundamentally inconsistent and could not explain a number of experiments, most importantly the spectrum of the helium atom and the anomalous Zeeman effect. The accumulation of experimental anomalies caused a crisis in the atomic community (Kragh, 1999, ch. 11), which led Born to say in 1923:

> We see that the similarity between atoms and planetary systems has its limitations. [...] It is increasingly likely that not only new assumptions in the ordinary sense of physical hypotheses will be needed, but that the whole system of concepts of physics must be rebuilt from the ground up.[4] (Born, 1923)

Which is exactly what his group was about to do.

1.2.1 Matrix mechanics

Modern quantum mechanics was born with Werner Heisenberg's 1925 paper *Über quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen* (Heisenberg, 1925). Heisenberg, Born and Pascual Jordan developed matrix mechanics in Göttingen in the following few months.

The crux of Heisenberg's paper, as wel as the key to his interpretive work, is condensed in a single sentence in its introduction:

> It seems more reasonable to try to establish a theoretical quantum mechanics, analogous to classical mechanics, but in which only relations between observable quantities occur. (Heisenberg, 1925)[5]

The idea that quantum mechanics is about measurements and not necessarily about the underlying physical phenomena is precisely what sparked the interpretive debate. Also, note how the same "heuristic principle of observability" was used in the formulation of special relativity by Einstein, who based it on the idea that absolute velocity and simultaneity are unobservable and only measurable quantities should appear in his theory.

Heisenberg believed that the difficulties of quantization had to do with the kinematics underlying classical mechanics. Bohr's theory had electrons spinning around nuclei with quantized angular momentum, but used classical kinematics and classically defined location q and momentum p. However:

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[4] *"Jedenfalls sehen wir, daß die Ähnlichkeit der Atome mit Planetensystemen ihre Grenzen hat. [...] Es wird immer wahrscheinlicher, daßnicht nur neue Annahmen im gewöhnlichen Sinne physikalischer Hypothesen erforderlich sein werden, sondern daß das ganze System der Begriffe der Physik von Grund aus umgebaut werden muß."*

[5] Translation by B.L. van der Waerden. (van der Waerden, 1967, paper 12)

> It is necessary to bear in mind that in quantum theory it has not been possible to associate the electron with a point in space, considered as a function of time. (Heisenberg, 1925)

Heisenberg tried to construct a formalism for quantum mechanics that was as close to classical mechanics as possible. His idea was that the equations of motion would hold but that the kinematical interpretation of q and p had to be changed, hence the title: *On quantum theoretical reinterpretation of kinematics and mechanics* (Heisenberg, 1925).

In classical mechanics the position $q(n,t)$ of an electron in orbit n can be written by the Fourier series

$$q(n,t) = \sum_a q_a(n) e^{2\pi i \nu_a(n) t}, \tag{15}$$

where $q_a(n)$ and $\nu_a(n)$ are respectively the amplitude and frequency of the ath Fourier component. However, these are both unobservable. To redefine q, Heisenberg took two observable quantities that are related to transitions between states n and m: the frequency of radiation ν_{nm} and the probability of transition[6] $|q_{nm}|^2$. Now:

$$q(n,t) = \sum_m q_{nm} e^{2\pi i \nu_{nm} t}. \tag{16}$$

The equations in Heisenberg's theory were given by long combinations of sums. Born pointed out that his theory fitted perfectly in the mathematical framework of matrices, defining a "position matrix" Q by:

$$Q_{nm} = q_{nm} e^{2\pi i \nu_{nm} t}. \tag{17}$$

Born and his pupil Jordan finished the article *Zur Quantenmechanik* only two months after Heisenberg's paper, in which they describe quantum mechanics in terms of matrices (Born and Jordan, 1925). Another three months later Born, Heisenberg and Jordan extended the theory to systems with arbitrarily many degrees of freedom in their so-called "three-men paper" *Zur Quantenmechanik II* (Born et al., 1926). In a time span of 5 months, these three men had made the earliest consistent theory of quantum phenomena.

In this theory, the Hermitian matrices Q and P determine the behaviour of the system, and satisfy the equations of motion:

$$\dot{P} = -\frac{\partial H}{\partial Q}, \quad \dot{Q} = \frac{\partial H}{\partial P}; \tag{18}$$

The energy spectrum is given by the eigenvalues of the Hamiltonian H_{nn}. Where old quantum theory relied on *ad hoc* solutions, finding the eigenvalues of a

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[6] Actually, these are Einstein's words: he had introduced the transition probability in 1916 (Einstein, 1916). Born and Jordan used this notion. Heisenberg originally called it the radiation amplitude.

matrix gave physicists a systematic approach to calculating quantized energy levels.

Finally, the quantum condition

$$\sum_k (Q_{nk}P_{km} - P_{nk}Q_{km}) = i\frac{h}{2\pi}\delta_{nm} = i\hbar\delta_{nm}. \tag{19}$$

is derived from the principle of correspondence. It introduces Planck's constant into the theory and led to the introduction of the *exact quantum condition*

$$[Q, P] = QP - PQ = i\hbar\mathbb{I}, \tag{20}$$

which was the first appearance of the now essential commutator $[A, B]$ in quantum mechanics. In the modern quantum theoretical formalism, the exact quantum condition is now known as *the canonical commutation relation*:

$$\boxed{[\hat{x}, \hat{p}] = i\hbar.} \tag{21}$$

It has to be mentioned that a few days before the three men's paper appeared, the English physicist Paul Dirac published an article in which he obtains equation 20 independently, in terms of Poisson brackets.

Stationary states (eigenvectors of the Hamiltonian) were a natural feature of this new theory. These states are constant in time; the matrices working on them are time dependent.

1.2.2 Wave mechanics

In January 1926 Erwin Schrödinger completed the first of his four-part article *Quantisierung als Eigenwertproblem* (Schrödinger, 1926), in which he developed wave mechanics. Schrödinger drew most of his inspiration from the work of the (then) relatively unknown French physicist Louis de Broglie.

De Broglie's 1924 thesis[7] (De Broglie, 1924) on the wave-particle duality stated that *any* moving particle or object has an associated wave, with a *De Broglie wavelength*

$$\boxed{\lambda = \frac{h}{p} = \frac{2\pi}{k},} \tag{22}$$

where $k = |\mathbf{k}| = \frac{p}{\hbar}$ is the wavenumber and $p = |\mathbf{p}|$ the absolute value of the momentum. From this idea he was able to reproduce the Bohr model, by stating that an integer number of wavelengths must fit on the electron's orbital path.

Inspired by these ideas, Schrödinger decided to find a three dimensional wave equation for the electron. Using Planck's $E = h\nu = \hbar\omega$ (where the angular frequency $\omega = 2\pi\nu$) and equation 22, he found an expression for a plane wave:

$$\psi(\mathbf{x}, t) = Ae^{i(\mathbf{k}\cdot\mathbf{x} - wt)} = Ae^{\frac{i}{\hbar}(\mathbf{p}\cdot\mathbf{x} - Et)}. \tag{23}$$

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[7] In 1927 the Davisson-Germer experiment confirmed the De Broglie hypothesis.

From this, we can find the derivatives

$$\nabla^2\psi = -\frac{p^2}{\hbar^2}\psi, \quad \frac{\partial\psi}{\partial t} = -\frac{iE}{\hbar}\psi. \tag{24}$$

Making the Hamiltonian $H = \frac{p^2}{2m} + V = E$ work on ψ gives us

$$\frac{p^2}{2m}\psi + V\psi = E\psi \rightarrow -\frac{\hbar^2}{2m}\nabla^2\psi + V\psi = i\hbar\frac{\partial\psi}{\partial t}, \tag{25}$$

which is the time-dependent Schrödinger equation for a single nonrelativistic particle. For the energy operator $\hat{E} = i\hbar\frac{\partial}{\partial t}$ and a Hamiltonian $\hat{H}$, the more general Schrödinger equation is

$$\boxed{\hat{H}\psi = \hat{E}\psi.} \tag{26}$$

The operator $\hat{H}$ is obtained by replacing every momentum $\mathbf{p}$ in the classical Hamiltonian by the momentum operator $\hat{\mathbf{p}} = \frac{\hbar}{i}\boldsymbol{\nabla}$. The location operator is simply $\hat{\mathbf{x}} = \mathbf{x}$.

The canonical commutation relation can now be derived (for simplicity in a one dimensional system) by

$$[\hat{x}, \hat{p}]f = x\frac{\hbar}{i}\frac{d}{dx}f - \frac{\hbar}{i}\frac{d}{dx}xf = \frac{\hbar}{i}\left(x\frac{df}{dx} - f\frac{dx}{dx} - x\frac{df}{dx}\right) = i\hbar f, \tag{27}$$

In this framework for quantum mechanics, the system is described by states ψ, that follow the time evolution given by the Schrödinger equation. The eigenvalues E_n of $\hat{H}$ give the energy spectrum.

Contrary to matrix mechanics, this allowed physicists to retain the conventional view of space and time. Also, it uses the then already well known methods of partial differential equations and its wave function ψ can be pictured in space. Moreover, it bore the same empirically satisfying results as matrix mechanics. As a consequence it was considerably more popular than its matrix counterpart (Born, 1954).

1.2.3 The Born rule

Around the time of publication of the fourth and final part of Schrödinger's *Quantisierung als Eigenwertproblem*, Born coined the infamous Born rule in a footnote (nota bene) of the paper *Zur Quantenmechanik der Stossvorgänge*, in the context of electron scattering:

> * Addition in proof: More careful consideration shows that the probability is proportional to the square of the quantity $\Psi_{n_t m}$ [the wave function]. (Born, 1926)

Born's reasoning involved an electron moving along the z-axis and scattering off an atom, after which it produces an outgoing spherical wave

$$\psi = \frac{f(k, \theta)}{r}e^{(ikr - i\omega t)}, \tag{28}$$

where θ is the polar angle with respect to the z-axis. He interpreted $|f(k,\theta)|^2 d\Omega$ as the probability that the electron is scattered in the solid angle element $d\Omega$, and then realized that he could generalize this result. The idea was developed further by Pauli, Heisenberg, Dirac, Jordan and Von Neumann (Landsman, 2009). In modern notation, for a discrete case, the Born rule can be stated in the following way:

The Born rule
Consider a normalized ($\langle\psi|\psi\rangle = 1$) wave function $|\psi\rangle$, and a discrete observable A with eigenvalues λ_n and eigenstates $|\lambda_n\rangle$. Upon measuring the quantity corresponding to A, we will always find one of its eigenvalues as the measurement outcome. The probability $p(A = \lambda_i)$ of finding λ_i is

$$p(A = \lambda_i) = \langle\psi|\lambda_i\rangle \langle\lambda_i|\psi\rangle \, . \tag{29}$$

As this rule links formalism and experiment together, it accounts for practically all predictions of quantum mechanics.

1.2.4 Mathematical equivalence of the two versions of quantum mechanics

At the beginning of 1926 physicists seemed to have two distinct versions of quantum mechanics with different assumptions and mathematical apparati. Quickly, Schrödinger showed that the two systems are mathematically equivalent (Schrödinger, 1926) and Paul Dirac established transformation theory in which he synthesized the two points of view (Dirac, 1927a). Dirac later also intruduced the bra-ket notation, which I shall be using from now on.

Von Neumann clarified the connection between the two versions of quantum mechanics in his classic 1932 textbook (von Neumann, 1932), showing that matrix mechanics and wave mechanics are isomorphic representations of a calculus of Hermitian operators in Hilbert space. Generalizations of matrix mechanics and wave mechanics appear in modern quantum mechanics in the form of respectively the Heisenberg picture and the Schrödinger picture.

Von Neumann's postulates of quantum mechanics

(1) *State space:* every quantum system has a corresponding Hilbert space $\mathcal{H}$. All states of the system are described by vectors in this space. A composite system corresponds with the direct product of the Hilbert spaces of the subsystems;

(2) *Observables:* every physical quantity $\mathcal{A}$ corresponds to a unique Hermitian operator A in $\mathcal{H}$;

(3) *The Born rule:* the only possible outcomes of the measurement of a physical quantity $\mathcal{A}$ of a system in the state $|\psi\rangle$ are the eigenvalues of the corresponding operator A; the probability of finding the eigenvalue λ_i is given by

$$p(A = \lambda_i) = \langle\psi|\lambda_i\rangle \langle\lambda_i|\psi\rangle\,, \tag{30}$$

where $|\lambda_i\rangle$ is the eigenstate of A with the eigenvalue λ_i;

(4) *Time evolution:* if no measurements are performed, the time evolution of the system is given by the Schrödinger equation:

$$\hat{H}\,|\psi\rangle = \hat{E}\,|\psi\rangle\,; \tag{31}$$

(5) *Projection:* directly after a measurement is performed and an eigenvalue λ_i is found, the system will be in the state

$$|\psi\rangle \xrightarrow{measurement} |\lambda_i\rangle\,. \tag{32}$$

1.3 The realist's dream

In the semantic (or model-theoretic) view, a theory consists of a model that represents reality in some way. In the case of quantum mechanics, we are provided with a number of rules (Von Neumann's postulates) that have shown their empirical value.

We could assume an instrumentalist position and say that that is all there is to it. But if we are being realist about our theories, we need an interpretation of this formalism to lay bare its relation to the real world. Hence, the interpretation of the formalism is terribly important, as it gives us the structures we see as fundamental, and tells us in what entities we ought to believe.

Where old quantum theory was mainly phenomenological, the new quantum mechanics initiated a number of philosophical discussions, resulting in a variety of interpretations.

The condition for any interpretation to be viable, is for it to reproduce all experimental predictions of quantum mechanics correctly. This makes them rigorously underdetermined by construction. What is left is a discussion of their answers to foundational issues and their descriptions of the nature of reality. In order to structure the interpretive debate from the '20s to the '60s, I have organised arguments into *conceptual issues*, *ontology* and *ideology*.

Firstly, conceptual issues are problems having to do with explanatory strength of an interpretation. For the Copenhagen interpretation, for example, the main conceptual issues are:

(a) *No well-defined measurement process:* it is not clear what physical process causes projection during measurement: projection is simply postulated;

(b) *No universality:* a universal interpretation describes the entire universe – the key requirement for this property is that the observer is internal to the quantum system, as the universe cannot have an external observer. In the Copenhagen interpretation, however, observers are always external.

Moreover, for this analysis, I use the notions of *ontology* and *ideology* of an interpretation, based on Quine (Quine, 1951). Here, I do not mean ontology in a mathematical sense, as in "the postulates of the theory", but in a physical sense: the ontology of an interpretation gives us the nature of the physical entities it claims to exist. The ideology of an interpretation is the collection of philosophical ideas it claims to be true.

We are looking for an interpretation with a "safe" ontology and ideology, where "safe" means "replicating the intuitive and classical notions of before the quantum revolution". Hence, a "safe" ontology would be an ontology with rigid particles that are classically picturable, and have observer-independent well-defined properties ("beables").

The weight of these arguments solely depends on philosophical tastes. After all, it might not seem very reasonable to assume that the quantum world has an ontology or ideology that is intuitive to us. However, practically speaking, ontological and ideological arguments are used very often in the interpretive debate.

The following ideological statements are at stake. Of course, we could arbitrarily add statements to this list. However, I intend to use this way of indexing arguments to describe the debate historically, and these are the major ideological statements that were used in the debate between the '20s and '60s:

(1) *Determinism:* in a deterministic interpretation the complete time evolution of a system can be calculated if its initial state is fully known;

(2) *Locality:* an interpretation can be local in the sense that all objects are influenced directly only by their immediate surroundings. It is perfectly possible for nonlocal interpretations to not permit any superluminal transfer of information, which would violate relativity theory, so nonlocality is not a conceptual issue;

(3) *Minimalism:* a minimal interpretation adds no extra "hidden" structure to the existing postulates. As these postulates already give all experimental predictions, any extra structure can be considered superfluous.

(4) *Noncontextuality:* an interpretation is contextual if measurement results can depend on properties of the measurement apparatus.

The realist's dream is finding an interpretation that has no conceptual issues and is both ontologically and ideologically safe.

We can see our realist's dream as an extension of Einstein's dream, which can be called *local realism.* He wanted quantum mechanics to be a local theory with a safe observer-independent ontology. It is a well-known story that he could not realise a theory with these properties, and that his dream was eventually proven to be impossible by Bell's theorem, as we shall see later.

As a final note: classical scientific realism is the idea that the world is built up out of entities with observer-independent and well-defined properties, and that measurement is the discovery of these properties. In other words, it is a combination of a safe ontology and noncontextuality. However, we will see that the Kochen-Specker theorem proves that this combination is not possible.

1.4 Early interpretations

1.4.1 Fundamentality and personal attachment

Physical laws can be put on a scale of fundamentality. Laws that pertain to a single experimental setup are rather unfundamental. Epistemological or statistical laws are "better", but not fundamental either. Laws that describe ontological properties of elementary objects are highly fundamental (or 'deep'[8]), as we can derive less fundamental laws from them. Generally, the more fundamental the law, the more impressive it is. The formulation of the deepest law in physics would be the summit of success for any theoretical physicist.

It is important to keep in mind what contributions to the formalism each physicist in the debate has made. After all, when working on the foundations of

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[8] Textbooks on physics would often say things like "This is a *deep* result.'

a field, a great personal investment is made in developing a certain formalism, and it is very tempting for a physicist to assume an interpretive position that implies the fundamentality of his own work.

Hence, even though it quickly became known that their representations were mathematically equivalent, Heisenberg thought of particles as fundamental, and Schrödinger thought of waves as elemental. De Broglie initially thought of the particle-wave duality as giving the basis for how we ought to interpret quantum mechanics.

This personal attachment to one's work is illustrated by the fact that there was a quite emotional (some might say unprofessional) aspect to the quantum debates. For example, Heisenberg called Schrödinger's work "disgusting" in a 1926 letter to Pauli:

> The more I ponder the physical part of Schrödinger's theory, the more disgusting it appears to me.

A second example is Schrödinger telling Bohr and Heisenberg to quit talking about that "verdammte Quantenspringerei" during a visit to Copenhagen:

> If all this damned quantum jumping were really to stay, I should be sorry I ever got involved with quantum theory. (Jammer, 1974, ch. 3.1)

In section 1.7, we shall see that other social and cultural aspects also played a major role in the 1925-1935 quantum debate.

1.4.2 Schrödinger's electromagnetic interpretation

Schrödinger himself had high hopes for his elegant wave mechanics to be a deterministic theory similar to classical mechanics. In his fourth communication he proposed to get rid of the particle representation entirely and to describe electrons as continuous charge density distributions $e|\psi|^2$ instead, implying that purely wave mechanical entities are all there is to a full description of the world.

This undulatory (wave only) interpretation met a number of serious difficulties, as illustrated by quotes from Born and Heisenberg:

> Schrodinger's reasoning is only viable for the case of the harmonic oscillator treated by him, all other cases a wave packet spreads out in the course of time over the whole immediate neighborhood of the atom. (Heisenberg, 1927)[9]

> To us in Göttingen this interpretation seemed unacceptable in face of well established experimental facts. At that time it was already possible to count particles by means of scintillations or with a Geiger counter, and to photograph their tracks with the aid of a Wilson cloud chamber. (Born, 1954)

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[9] Translation by John Wheeler and Wojociech Zurek. (Wheeler and Zurek, 1983, ch. I.3)

In other words, a wave packet will generally spread out in time, but it is still possible to *count* actual particles, hence the electromagnetic interpretation is not viable.

1.4.3 De Broglie's theory of the pilot wave

De Broglie's 1926 and 1927 interpretation of quantum mechanics (De Broglie, 1927) was an attempt to reconcile the wave-description and the particle-description of quantum mechanics. He assumed that the particle momentum is *guided* by the wave function:

$$\mathbf{p} = \hbar \boldsymbol{\nabla} \phi, \tag{33}$$

where ϕ is the phase of the wave function.

The essence of this (deterministic) interpretation is that the quantum object conserves its classical corpuscular nature *and* has the properties of a wave, thus focusing the spotlight on the De Broglie hypothesis ("any moving particle or object has an associated wave").

Most physicists were reluctant to accept these ideas. Pauli raised objections at the 1927 Solvay Conference, arguing that the theory could not provide a local account of many body systems. The theory was generally seen as unfruitful, and De Broglie capitulated to the Copenhagen interpretation (section 1.5) in 1928. Only in the '50s he went back to (a modification of) his earlier views, when David Bohm introduced a viable alternative interpretation of quantum mechanics in terms of particles and pilot waves (Jammer, 1974, ch. 5.1) (Bohm and Hiley, 1982).

1.4.4 The consciousness interpretation

Von Neumann coined a surprising interpretation in his 1932 book that is sometimes called a variant of the Copenhagen interpretation.

> First, it is inherently entirely correct that the measurement or the related process of the subjective perception is a new entity relative to the physical environment and is not reducible to the latter. (von Neumann, 1932)

In a nutshell: according to Von Neumann, consciousness cannot be described by physical law, as opposed to the body (a highly Descartian dualistic concept); the consciousness of the experimenter causes the collapse of the wave function. Next to the obvious connection to the mind-body problem (which Von Neumann called "psycho-physical parallelism"), this interpretation has been linked to the anthropic principle. Since these assumptions about consciousness are highly dubious and the interaction between consciousness and matter is not clear, this interpretation is not generally seen as viable.

1.5 The Copenhagen interpretation

The Copenhagen interpretation is the name of the standard interpretation that appears in modern textbooks on quantum mechanics. It can be seen as the name of a *collection* of interpretations, as its followers had similar but slightly different ideas. Most notably Bohr and Heisenberg created and advocated the Copenhagen spirit. Other physicists associated with this school of thought are Pauli, Born and Jordan.

Their views had a number of things in common. Firstly, they accepted indeterminism. Secondly, according to them the mathematical formalism of Von Neumann gives a complete description of quantum phenomena. Thirdly, they dispensed with the view of microscopic objects as rigid and classically picturable. The aspect that a lot of subscribers to the Copenhagen spirit *disagreed* about was the observer role in the wave function collapse, arguably because it is the most difficult aspect of quantum mechanics to treat adequately within the Copenhagen framework (Howard, 2004).

It is not true that the interpretation is purely instrumentalistic or positivistic. Eventually, Bohr's interpretation simply tells us that the reality of quantum mechanical observables must always be considered within an experimental context.

The term 'Copenhagen interpretation' was coined by Heisenberg only in the '50s, when he used it in discussions about other interpretations that arose in this decade.

1.5.1 The uncertainty principle

Trying to clarify the relation between quantum mechanics and measurement, Heisenberg and Bohr attempted to account for the observed path of an electron in a Wilson cloud chamber. However, the concept of "path" was not defined in matrix mechanics and the waves of wave mechanics would disperse rather than show a clear path (Jammer, 1974, ch. 3.1).

In February 1927 Heisenberg recalled something Einstein had said to him after the Berlin Physics Colloquium in the spring of 1926:

> Only the theory decides about what one can measure. (Heisenberg, 1969, ch. 5)[10]

Heisenberg reasoned that he could consider the observed phenomenon in a way that is consistent with quantum mechanics by looking at what the theory says about measurement. Classically, a path is given by exact momentum and position values in time. However, in his 1927 paper (Heisenberg, 1927) he showed that quantum mechanics decides that we cannot strictly simultaneously observe the exact position and momentum of the electron. As a consequence we should regard the observed trajectory as a set of imprecisely defined momentum and position values instead.

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[10] "Erst die Theorie entscheidet darüber, was man beobachten kann."

Heisenberg derived that the allowed precision of simultaneously measuring p and x (given by respectively δp and δx) is proportional to h:

$$\delta p \delta x \sim h. \tag{34}$$

The American physicist Earle Hesse Kennard derived the more formal uncertainty equation: (Kennard, 1927)

The Heisenberg uncertainty principle
The standard deviation of momentum σ_p and the standard deviation of position σ_x are related by

$$\sigma_p \sigma_x \geq \frac{\hbar}{2}. \tag{35}$$

This is actually a specific case of the *generalized uncertainty principle* (Robertson, 1929)

$$\sigma_A^2 \sigma_B^2 \geq \left(\frac{1}{2i} \langle [A, B] \rangle \right)^2 , \tag{36}$$

of which a derivation is added as an appendix (chapter 7.1). From this general principle we can see that there is a uncertainty relation for every non-commuting pair, e.g.:

$$\sigma_E \sigma_t \geq \frac{\hbar}{2}. \tag{37}$$

Note, however, that this is slightly different than the previous case, for strictly speaking there is no observable for time, so time has a different status than position.

Heisenberg stated that uncertainty exists because every measurement disturbs the quantum system. The more precisely one measures an electron's position, the more the measurement will disturb its momentum and therefore make it uncertain. According to Heisenberg, this *observer effect* explains quantum uncertainty.

In other words, he stated that the process of measurement *mechanically* changes the quantum mechanical system. In 1930 he illustrated this idea with a thought experiment called *Heisenberg's microscope*[11] (Heisenberg, 1930). He started out with the Abbe diffraction limit (1873), that gives us a measure of how precisely we can do spatial measurements with a microscope, using light with a wavelength λ that is converging with an angle θ:

$$\delta x \sim \frac{\lambda}{\sin\theta}. \tag{38}$$

Now, if we try to produce an image of a single electron, the light will cause a Compton recoil. Using the De Broglie hypothesis, the recoil momentum is

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[11] This is mentioned but not clarified in his 1927 uncertainty paper.

proportional to $\frac{h}{\lambda}$. The direction of the recoil is uncertain within the bundle of light rays, so the uncertainty δp in the x-direction is

$$\delta p \sim \frac{h}{\lambda}\sin\theta. \tag{39}$$

Hence,

$$\delta p \delta x \sim h. \tag{40}$$

In words: the more precise we measure the location of the electron (the bigger the Compton recoil is), the more uncertain its momentum becomes.

Heisenberg argued for an *epistemic* interpretation of the uncertainty relations: it is in principle impossible to simultaneously measure x and p of a single electron.

> One might be led to the presumption that behind the perceived statistical world there still hides a "real" world in which causality holds. But such speculations seem to us, to say it explicitly, fruitless and senseless. Physics ought to describe only the correlation of observations. (Heisenberg, 1927)

In other words: Heisenberg dismisses any explanation in terms of well-defined particles as senseless, because we cannot measure it and physics should be about what we can measure.

Heisenberg gives a neat argument in a lecture in 1930 (Heisenberg, 1931) as to how his uncertainty principle makes quantum physics epistemologically consistent. He states that a theory is only "closed in the small" (*die Natur im kleinen abgeschlossen*), if it renders any questions about what happens in nature beyond a certain smallest scale meaningless.

1.5.2 Bohr's interpretive work

In his 1927 lecture entitled *The Quantum Postulate and the Recent Development of Atomic Theory* (Bohr, 1928)[12], he introduced a number of highly influential new concepts.[13] A more detailed account of Bohr's thinking can be found in the first chapter of Scheibe's 1973 book *The Logical Analysis of Quantum Mechanics* (Scheibe, 1973).

The quantum postulate and the buffer postulate give insight in Bohr's essential ideas about quantum mechanics, in which the interaction between a quantum object and the observer, and the language in which we speak about quantum phenomena plays a central role.

The *quantum postulate* is described as follows:

> Notwithstanding the difficulties which, hence, are involved in the formulation of the quantum theory, it seems, as we shall see, that

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[12] Also printed in (Wheeler and Zurek, 1983, ch. I.4).

[13] It has to be mentioned that interpreting Bohr is almost a science by itself: his style is unique and mainly consists of qualitative arguments and notoriously complicated sentences.

> its essence may be expressed in the so-called quantum postulate, which attributes to any atomic process an essential discontinuity, or rather individuality, completely foreign to the classical theories and symbolized by Planck's quantum of action. (Bohr, 1928)

A quantum phenomenon consists of an object, a measuring apparatus and the interaction between the two. We cannot simply evaluate the object by itself as we do in classical systems, because the interaction ought not to be neglected in the quantum case, making it impossible to sharply separate the behaviour of the object and the interaction.

To Bohr, measurement is an interaction between a classical measurement apparatus and a quantum system. In other words, quantum mechanics does not apply to the measurement apparatus itself, so quantum physics is not applicable to the whole universe. This is a grave conceptual difficulty in the Copenhagen interpretation. After all, where exactly do we place the border between quantum and classical (the "Heisenberg cut")? Later we will see that universality was the reason for Everett to coin his relative state interpretation in the '50s.

Moreover, projection (the "essential discontinuity") is simply postulated, but no physical process as to how it occurs is given. This is the second big conceptual issue of the Copenhagen interpretation. In fact, it can be said that the measurement problem is a consequence of Bohr's introduction of a quantum-classical duality. We see that universality and well-defined measurement are intimately connected.

The *buffer postulate* (term coined by Scheibe) is best illustrated by a 1949 quote:

> It is decisive to recognize that, however far the phenomena transcend the scope of classical physical explanation, the account of all evidence must be expressed in classical terms. (Wheeler and Zurek, 1983, ch. I.1)

For the sake of unambiguity and communicability, the description we give of a measurement is always stated in classical terms. However, whenever we give a classical description of a measurement of a quantum object and say things like "The electron was found on position x", we omit the interaction, limiting the possibility of fully characterizing the object.

This limitation is described in a decisive step of Bohr's reasoning, the notion of *complementarity*, which is best quoted as follows (1934):

> Complementarity: any given application of classical concepts precludes the simultaneous use of other classical concepts which in a different connection are equally necessary for the elucidation of the phenomena. (Wheeler and Zurek, 1983, com. I.1)

In a single quantum phenomenon we can only characterize a part of the object. We need a new experimental arrangement to characterize the remaining part, but this new quantum phenomenon will be incompatible with the previous one.

Bohr called phenomena like these *complementary* and stated that the uncertainty relations represent complementarity in a quantitative way. As opposed to Heisenberg's epistemic understanding, Bohr had an ontic understanding of the uncertainty relations. This understanding has become the norm in the Copenhagen interpretation, and paints a picture of the world in which particles are "blurry": they do not have a well-defined position and momentum at the same time. Hence, the Copenhagen interpretation does not have a safe ontology.

1.5.3 The Bohr-Einstein debate

Even though Heisenberg and Born claimed that Einstein was an essential source of inspiration for their work, Einstein was notoriously sceptical of the Copenhagen interpretation. His standpoint may be called *local realism* and consisted of two major principles that formed the basis of his attacks on Copenhagen.

(1) *Safe ontology:* reality has observer-independent, well-defined properties;

(2) *Locality:* objects are influenced directly only by their immediate surroundings.

Einstein's words in a discussion with Bohr are appropriate here: "I like to think that the moon is there even if I am not looking at it" – when determining the location of a particle and measuring x_0, Einstein liked to think it was already at x_0 before the measurement was performed.

Einstein mainly gave negative arguments in the debate, telling his opponent which of his ideas were certainly *not* true. Being his typically Einsteinian self, he forged his intellectual weaponry out of thought experiments, most importantly the *double-slit experiment*, *Einstein's box* and the *EPR paradox*.

Einstein gave his first critique of the Copenhagen interpretation at the fifth Solvay conference in 1927, which set the stage for the rest of the debate. Among its participants were pretty much all the famous names I have mentioned so far: Bohr, Born, de Broglie, Dirac, Einstein, Heisenberg, Pauli, Planck and Schrödinger. (A good book on this particular conference is (Bacciagaluppi and Valentini, 2009))

The double-slit experiment is the last of a series of slit experiments conceived by Einstein (Schlipp, 1959, vol. I, ch. II.7). Consider the double-slit setup as drawn in figure 1.

An electron beam passes the left slit in the first screen (slit 0), diffracts and passes the double slits (1 and 2) in the second screen, after which it leaves an interference pattern on the third screen. Einstein stated that it would be possible to measure which slit an individual electron went through by measuring the momentum recoil of slit 0 (either up or down), implying that the electron was not in a superposition of the double slit and that the ensemble interpretation of the wave function is correct.

Bohr's response consisted of an argument showing that the proposed measurement will change the experiment to the extent that no interference pattern will appear.

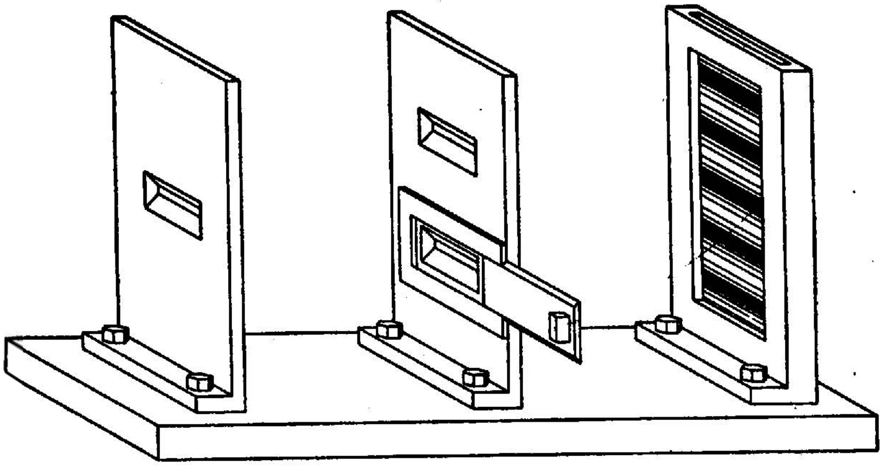

Figure 1: The double-slit experiment as drawn by Bohr. (Schlipp, 1959, vol. I, ch. II.7)

If the distance between the first screen and the second screen is l and the distance between slit 1 and 2 is d, the angle between the two possible paths is $\alpha \approx \sin\alpha = \frac{d}{l}$ for small α. The momentum of the electron is $p = \frac{h}{\lambda}$. We have to measure the vertical recoil momentum of the first screen with a precision δp, which has to be smaller than the difference in momentum transfer between measuring an electron going through slit 1 or slit 2:

$$\delta p \lesssim p \sin\alpha \approx \frac{hd}{\lambda l}. \tag{41}$$

As we are doing momentum measurements on the first screen, not only does it have to be movable but it also has to be regarded as a quantum object that obeys the Heisenberg uncertainty principle. Now, because

$$\delta p \delta x \sim h, \tag{42}$$

we find:

$$\delta q \gtrsim \frac{l\lambda}{d}. \tag{43}$$

So the location of the first screen is indetermined with an order of magnitude that is roughly the same as the distance between interference bands. As a result we can choose between observing the path of the particle or observing the wavelike interference behaviour, but we cannot have both, which is a strong argument in favour of Bohr's complementarity.

The double slit experiment has been important throughout the development of quantum mechanics. In his book *The Feynman Lectures on Physics*, Feynman called it "a phenomenon which is impossible [...] to explain in any classical way, and which has in it the heart of quantum mechanics. In reality, it contains the only mystery [of quantum mechanics]."

With another thought experiment called Einstein's box, that was also intended to show that the uncertainty relations do not hold for individual particles, he focused on equation 37:

$$\sigma_E \sigma_t \geq \frac{\hbar}{2}. \tag{44}$$

But again, Bohr had a way to counter his argument. This time, he used Einstein's own theory of general relativity to do it (Seevinck, 2014, Ch. 4). The physics community generally accepted this argument and therefore the validity of the uncertainty relations for individual particles.

The climax of the Bohr-Einstein debate was the so-called EPR[14] article (Einstein et al., 1935) in 1935, describing a thought experiment in which two entangled particles seem to convey information faster than light.

We can describe this line of thought in a system with an entangled electron-positron pair in the spin singlet state

$$|\psi(t_0)\rangle = \frac{1}{\sqrt{2}}(|\uparrow\downarrow\rangle - |\downarrow\uparrow\rangle). \tag{45}$$

Here, $|\uparrow\downarrow\rangle$ means that the first particle has spin up and the second particle has spin down. Now we bring the electrons far apart, after which observer A measures the spin of electron 1 in the (arbitrary) z-direction. Shortly afterwards, observer B measures the spin of electron 2 in the same direction. If A measures spin *up*, the wave function of the system collapses into

$$|\psi(t_1)\rangle = |\uparrow\downarrow\rangle. \tag{46}$$

Observer B will now definitely find spin down. This in itself already constitutes a superluminal effect.

Moreover, observer A could have also decided to measure the spin in the x-direction. This means that the second electron will have the opposite spin in the x-direction, when measured. As observer A can decide at the very last moment in which direction he decides to measure, and this information cannot travel faster than light, the second particle must have information about its x and z spin simultaneously, even though the uncertainty relations forbid this.

This result can be explained in two ways:

(i) The particles "communicate" superluminally: somehow the positron instantaneously knows that the electron spin has been measured and aligns itself in the opposite direction.

(ii) Quantum mechanics is not a complete description of reality, because it cannot describe a particle with well-defined spin in the x and z directions. We must either find a different description or introduce hidden variables.

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[14] The authors were Albert Einstein, Boris Podolsky and Nathan Rosen.

As, according to EPR, explanation (i) violates special relativity, explanation (ii) is the only viable option.

> While we have thus shown that the wave function does not provide a complete description of the physical reality, we left open the question of whether or not such a description exists. We believe, however, that such a theory is possible. (Einstein et al., 1935)

Please note that the authors originally used a description in terms of position and momentum for the two particles, instead of spin in the z and x directions. I have used a description in terms of spin, because I will use it more often later on.

In Bohr's reply, he says that indeed there must be some influence at a distance, but this influence is not *mechanical*. This means that a collapse of the wave function may spread out superluminally. He uses complementarity to explain this: the type of measurement that is done on particle A, defines the type of phenomenon we can study in the experiment. Although this is a neat argument for Bohr to use, it still means that the Copenhagen interpretation implies nonlocality in some way.

However, even after the EPR argument, it was apparent that the general attitude in the physics community towards this debate was still one in favour of Bohr and the Copenhagen interpretation. This has been attributed to a lack of interest in the EPR article due to its metaphysical character. As many of Bohr's articles on the interpretation of quantum mechanics had a similar character this is not a convincing argument. Rather, the physics community's accepting attitude towards the Copenhagen school might have been influenced by the intellectual milieu at the time, which will be discussed in section 1.7.1.

While EPR thought of entanglement as a *problem* in the theory, we now know that it is actually a fundamental *property* of quantum mechanics. Experiments have proved its existence and it is already being applied in fields such as quantum cryptography. The no-communication theorem tells us that the violation of locality does not lead to "spooky communication at a distance", in the (subtle) sense that the "communication" between entangled particles is superluminal, but there is no possibillity of actually *transferring information* between observers in the process. We can easily see this in our example, where the expectation values for observer B do not change after A has performed a measurement.

1.5.4 Schrödinger's cat

Next to Heisenberg's microscope and Einstein's anti-Copenhagen arguments, a few other interesting thought experiments were introduced during the early development of quantum mechanics, most famously Schrödinger's cat. Whereas the earlier arguments pertained to ontological and ideological issues, this cat lays bare the Copenhagen interpretation's conceptual issues.

The cat was not originally intended to give rise to a number of funny T-shirts and internet comics, but rather to show Schrödinger's negative sentiments towards the Copenhagen interpretation in a *reductio ad absurdum* argument. In a 1935 paper (Schrödinger, 1935), he performs a thought experiment in which he places a cat in a chamber, along with a flask of poison and a single radioactive atom. The decay of the atom triggers the release of the poison via a Geiger counter, killing the cat.

The decay of the atom is governed by quantum mechanics. According to the Copenhagen interpretation, as long as we do not measure the state of the atom, it is in a superposition of being undecayed and decayed. As a consequence, *the cat is in a superposition of being dead and alive*[15]. We can make the wave function collapse by opening the chamber and investigating the liveliness of the cat – but we might be reluctant to do so as we can cause the cat to "collapse into death" simply by opening the chamber.

Actually, Einstein had made a similar suggestion in an earlier letter to Schrödinger, as an argument for incompleteness that avoids talking about locality:

> The system is a substance in chemically unstable equilibrium, perhaps a charge of gunpowder that, by means of intrinsic forces, can spontaneously combust, and where the average life span of the whole setup is a year. In principle this can quite easily be represented quantum-mechanically. In the beginning the psi-function characterizes a reasonably well-defined macroscopic state. But, according to your equation [i.e., the Schrdinger equation], after the course of a year this is no longer the case. Rather, the psi-function then describes a sort of blend of not-yet and already-exploded systems. Through no art of interpretation can this psi-function be turned into an adequate description of a real state of affairs; in reality there is no intermediary between exploded and not-exploded. (Fine, 1996)

The absurd part of this enterprise is of course the classical object (cat) being in a quantum superposition. The question that Schrödinger asks is: how do the classical features of the macroscopic world (in which there are no superpositions) emerge from the quantum world?

> It is typical of these cases that an indeterminacy originally restricted to the atomic domain becomes transformed into macroscopic indeterminacy, which can then be resolved by direct observation. That prevents us from so naively accepting as valid a "blurred model" for representing reality. (Schrödinger, 1935)

In other words: the conceptual problems of the Copenhagen interpretation make it impossible to accept its "blurred" ontology.

The paradox arises because the Copenhagen interpretation is not universal, and does not have a good answer to the measurement problem. Even though this is a serious conceptual difficulty, the thought experiment did not arouse much

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[15] Contrary to popular belief, this is quite different from the cat being both dead and alive – a phrase which a physicist would not use.

debate in the '30s. It was only after the '70s that the cat received its legendary status and appeared prominently in discussions on quantum foundations.

There are different ways of resolving the paradox: a physicist might say for example, upon finding a dead cat in a box, that

(a) we should assume a position in favour of incompleteness and say that the cat was dead all along;

(b) this cat is not a good argument against the Copenhagen interpretation, as there is no paradox here: large objects can be in a quantum superposition;

(c) we can refute the existence of the quantum superposition of the cat by observing that the Geiger counter performs the measurement, not the human opening the chamber.

Of course, Schrödinger had response (a) in mind. As an argument in favour of (b) it can be said that quantum systems are getting bigger and bigger as we speak: recently, biomolecules have been brought in quantum states (Hackermüller et al., 2003). Answer (c) simply gives us another possibility for placing the Heisenberg cut.

1.6 Von Neumann's impossibility proof

In his 1932 book, Von Neumann tried to show that observables of a quantum system do not have definite values that can be specified with extra (hidden) variables. At the time, this proof was widely believed to give a logically irrefutable argument. However, he turns out to have made an unwarranted assumption, as pointed out in the '60s by John Bell, whose contribution to the hidden variable debate will be discussed later.

Consider an ensemble E and two noncommuting observables A and B. For a large number of measurements N on A, the measurement outcomes $a_1, a_2, \ldots, a_N$ give the expectation value:

$$\langle A \rangle = \frac{1}{N} \sum_{i=1}^{N} a_i, \tag{47}$$

Now assume that there are hidden variables λ. For a particular hidden variable λ_1, a measurement of A would yield a value $a(\lambda_1)$ with absolute certainty. The hidden variables are distributed according to some density function $\rho(\lambda)$, so that:

$$\langle A \rangle = \int \rho(\lambda) a(\lambda) d\lambda. \tag{48}$$

Using the hidden variable λ_1, we can find a subensemble E_1 of E in which all measurements of A will have $a(\lambda_1)$ as an outcome, so that:

$$\langle A^2 \rangle_{E_1} = \frac{1}{N} \sum_{i=1}^{N} (a(\lambda_1))^2 = (a(\lambda_1))^2 = \left(\frac{1}{N} \sum_{i=1}^{N} a(\lambda_1) \right)^2 = \langle A \rangle_{E_1}^2 . \tag{49}$$

As a result, this subensemble is *dispersion-free*:

$$\Delta A_{E_1} = \sqrt{\langle A^2 \rangle_{E_1} - \langle A \rangle_{E_1}^2} = 0. \tag{50}$$

Define an operator C:

$$C = A + B, \tag{51}$$

for which again

$$\langle B \rangle_{E_1} = b(\lambda_1), \langle C \rangle_{E_1} = c(\lambda_1). \tag{52}$$

Now, *assuming* that the expectation value of two noncommuting operators is the sum of the expectation values of each separate operator:

$$\langle C \rangle_{E_1} = \langle A \rangle_{E_1} + \langle B \rangle_{E_1}, \tag{53}$$

we find

$$c(\lambda_1) = b(\lambda_1) + a(\lambda_1). \tag{54}$$

However, it is easy to find a counterexample for this. For example, with σ_i the Pauli spin matrices:

$$A = \sigma_x, B = \sigma_y, C = \sigma_x + \sigma_y. \tag{55}$$

Now, $a(\lambda_1) = \pm 1$, $b(\lambda_1) = \pm 1$ and $c(\lambda_1) = \pm\sqrt{2}$. These values cannot possibly meet equation 54. Our assumption of the existence of hidden variables leads to a contradiction, hence it was wrong.

This is the first example of a "no go" theorem in quantum mechanics. It was later pointed out that the assumed sum rule (equation 53) applies for normal quantum mechanical states, but does not apply generally to hidden variable theories (Bell, 1966). After all, in a hidden variable theory, the expectation value for an observable must equal one of its eigenvalues: the eigenvalues for A and B are ± 1, and the eigenvalues for C are $\pm\sqrt{2}$, so the assumed sum rule cannot possibly hold.

Von Neumann's work provided an important basis for later "no go" theorems such as Bell's theorem (section 3.3) and the Kochen-Specker theorem (section 3.4).

1.7 Quantum foundations and society

Until now, we have taken a rather internalist approach to look at the development of quantum physics. However, there were undeniably also important external factors involved.

1.7.1 Quantum physics and Weimar Germany: the Forman thesis

Why was quantum physics, one of the most impressive scientific theories of all time, developed in Weimar Germany – a nation troubled by the treaty of Versailles, hyperinflation, an economic depression and poor academic funding – of all places?

In his 1971 paper *Weimar Culture, Causality and Quantum Theory, 1918 – 1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment* (Forman, 1971), Paul Forman tries to provide an answer.

He quotes the 1918 book *Der Untergang des Abendlandes* by Oswald Spengler to illustrate the revival of an existentialist *Lebensphilosophie* in the Weimar republic. This influential book, propagating the failure of exact sciences and rejecting determinism, was received favourably by a large portion of the German academically educated audience. The sentiment was developed to the extent that a number of notable physicists and mathematicians capitulated to Spenglerism, rendering determinism a controversial issue. Forman continues to describe a general sense of crisis in the intellectual milieu, and states:

> And while it is undoubtedly true that the internal developments in atomic physics were important in precipitating this widespread sense of crisis among German-speaking Central European physicists, and that these internal developments were necessary to give the crisis a sharp focus, nonetheless it now seems evident to me that these internal developments were not in themselves sufficient conditions. The *possibility* of the crisis of the old quantum theory was, I think, dependent upon the physicists' own craving for crises, arising from participation in, and adaptation to, the Weimar intellectual milieu. (Forman, 1971, ch. II.4)

Here Forman seems to imply that the intellectual milieu made possible the birth of the new formalism. However, it is not clear why the scientific arguments (the inconsistence of the existing mathematical formalism and the accumulation of experimental inaccuracies) as to why the Born-Sommerfeld model was failing and a new formalism for quantum theory was needed would not be sufficient. It is more reasonable to state that the Weimar culture made possible the acceptance of the Copenhagen interpretation of the formalism.

Forman proceeds by giving an impressive number of examples of physicists renouncing determinism as a fundamental physical notion in the early '20s, showing that the determinism-indeterminism debate in physics started well before the Born rule was introduced. However, it must be said that these examples are often given in the form of quotes from public speeches. It is not entirely clear if these "popular" sources provide a good foundation to elucidate the development of actual scientific thought.

Spenglerism might not have been the only spark for the debate on indeterminism. Bohr already introduced a certain randomness in the electron transitions of his 1913 atomic model. Also, Rutherford's very early 1900s mechanism to explain radioactive decay had an inherent (but inexplicit) randomness in it, using terms like "average lifetime". Moreover, Einstein's Annus Mirabilis paper on Brownian motion as well as Boltzmann's earlier works on statistical mechanics could be interpreted to imply a certain randomness in the behaviour of atoms. These were all important papers, but a fundamental indeterminism had not yet *explicitly* entered the foundations of quantum mechanics.

Born's 1926 introduction of the Born rule was the pivotal moment in the history of quantum mechanics where probability explicitly entered the stage[16]. After this, the discussion was to become more focused on the strongly related question of whether a fundamental role for randomness in the interpretation of the wave function is acceptable or not. The general opinion in the physics community was that it is acceptable indeed, but here the list of scientific arguments was absent:

> I myself am inclined to give up determinism in the world of atoms. But that is a philosophical question for which physical arguments alone are not decisive. (Born, 1926)

In order for the scientific community to steadily accept this explicit radical indeterminism, it must have been influenced in a way.

This is where Forman ends his discussion, but we find another strong case in 1935, when the EPR article showed that the Copenhagen interpretation is nonlocal. Bohr's response did not resolve nonlocality – and yet the general public took his side, showing itself to be prepared to hold on to these indeterministic ideas, even though it was now clear that the interpretation was also nonlocal.

Summarizing, I argue that the intellectual milieu in the Weimar period made possible the acceptance of the Copenhagen concept of indeterminacy, but I reject the stronger claim that the quantum formalism itself was born as a result of this milieu.

1.7.2 The philosopher-physicist

It is clear that the old generation of quantum physicists were very much involved in philosophy. Besides the obvious Bohr and Einstein, a clear 'case' of the philosopher-physicist is Heisenberg. Pauli writes to Bohr about Heisenberg in 1923:

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[16] Heisenberg did not renounce determinism with his matrix mechanics; he rejected the notion of classical movements on the atomic scale. The transition probability (radiation amplitude) term was included because it is *observable*. In wave mechanics, there was no such thing as a transition probability until Born's rule came about. (Consider an electron in initial state $|\psi(t_0)\rangle = |\psi_a\rangle$; the probability of a transition to state $|\psi_b\rangle$ is given by Born's rule: $p_{a\to b}(t) = |\langle\psi_b|\psi(t)\rangle|^2$.)

> I always feel very strange with him [. . .] For he is very unphilosophical [. . .] I was therefore very pleased that you have invited him to Copenhagen [. . .] Hopefully then Heisenberg, too, will return home with a philosophical orientation toward his thinking.(Cassidy, 1992)

Pauli's wish came true. After the mid '20s, Heisenberg committed himself to producing quite a lot of work about philosophy. He developed a neo-Kantian view, which is perhaps best exposed in his 1958 book *Physics and Philosophy.* He gives us a few good examples of the implications of the Copenhagen interpretation on the philosophical tradition (Heisenberg, 1958, ch. 5).

Firstly, Bohr and Heisenberg took the role of observation in quantum mechanics even beyond a heuristic principle: in the Copenhagen interpretation the observer is an integral part of the physical system. Heisenberg argued that this is incompatible with one of the most important ontological doctrines in philosophy, namely the Cartesian partition of the world in *res cogitans* (mind) and *res extensa* (matter). Quantum theory does not describe *nature* but rather *nature as observed by man*, thus rendering Descartes' sharp separation impossible.

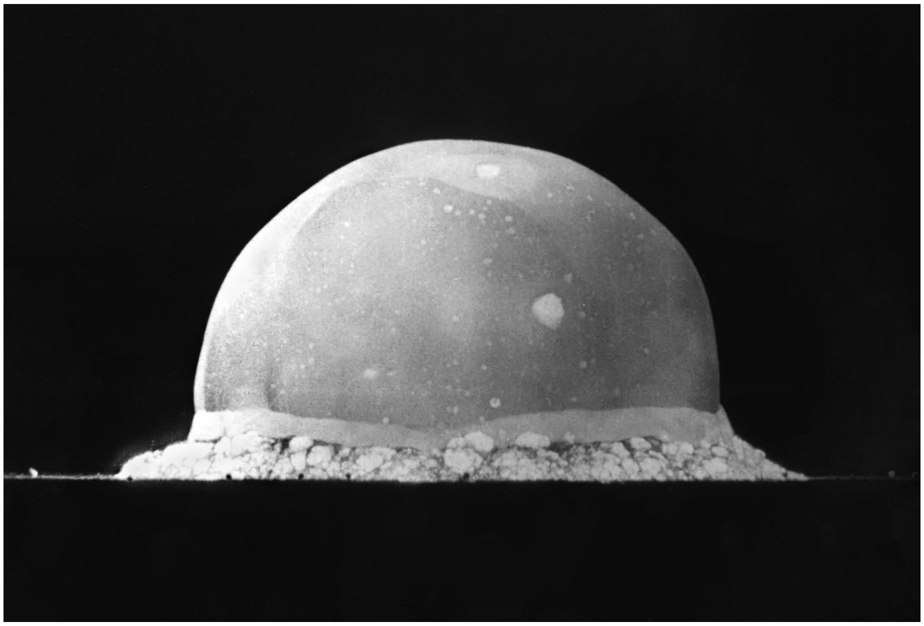
Secondly, where Spenglerism most likely played a part in its early acceptance, the notion of indeterminism in the Copenhagen interpretation conversely had an impact on philosophy. Heisenberg discussed Kant's *synthetic a priori judgements* and found that quantum mechanics complicates the Kantian presupposed law of causality (and analogously, relativity complicates our intuition of space and time). The Copenhagen interpretation describes the measurement apparatus in a classical way, in which the Kantian concepts of space, time and causality are necessary. However, these concepts have only a limited applicability as causality does not apply to the measurement itself. This is, according to Heisenberg, "the fundamental paradox of quantum theory that could not be foreseen by Kant". It changes the character of Kant's synthetic a priori judgements from metaphysical to practical.

Many of the physicists I have mentioned so far received a Nobel prize for their work on the foundations of quantum mechanics, including Planck, Einstein, Bohr, De Broglie, Schrödinger, Dirac, Pauli, Heisenberg and Born. The interpretive debates involved many of these big names of the first generation of quantum physicists that had reshaped the world of physics.

Essentially philosophical arguments were published in scientific papers: the EPR discussion, for example, was published in Physical Review. Philosophical discussions were held at scientific congresses: Bohr's Como lecture was given at the International Physical Congress, and many interpretive debates were held at the Solvay conferences.

The interpretation of quantum mechanics was considered a part of science and a hot topic in the scientific community in the '20s and '30s. As we shall see, after the second world war – and the rise of a new generation of quantum physicists – this would have *completely* changed.

2 World war two



Trinity Site explosion, 0.016 seconds after explosion, July 16, 1945. The little black dots at the bottom are trees. (Berlyn Brixner, Los Alamos Photo Gallery, PA-98-0520)

Adolf Hitler was appointed as chancellor of Germany in 1933. Upon visiting the country in that very same year, Von Neumann wrote to a friend:

> If these boys continue for only two more years (which is unfortunately very probable), they will ruin German science for a generation – at least. (Weiner, 1969, p. 205)

The political and scientific environments were changing rapidly in the home country of many leading scientists working on quantum theory. Three years later Goudsmit observed:

> Very few contributions to physics are coming from Germany nowadays, the main German export being propoganda of hatred. (Kragh, 1999, p. 233)

In 1939 the second world war broke out – and when it ended in 1945 the world had changed, not least in the way physics was practised and the role physics played in society.

2.1 The physics community

2.1.1 Europe

In April 1933 the Nazi party implemented a law that made it forbidden for Jews and socialists to hold positions as civil servants. A wave of dismissals and resignations of university teachers followed, leading to a drain of creativity and excellence. Amongst the thousands of intellectual emigrants were Einstein, Born, Szilárd, Frisch and Meitner (these last three names will be important in section 2.2.1). It has been estimated that Germany lost 25% of its physics community – the universities with progressive physics institutes like Göttingen and Berlin suffered most (Kragh, 1999, Ch. 16). Following the expansion of Nazi Germany, similar emigrations happened in other parts of Europe: the list of emigrants includes Schrödinger, Fermi and Bohr. Most of these scientists relocated to either the US or the UK.

In 1936 the German Nobel prize winner Philipp Lenard published a book entitled "Deutsche Physik". He attacked modern theoretical physics by rejecting relativity and "sterile mathematization," and advocated holism and intuitive physics. As this rejection of "Jüdische Physik" agreed with the Nazi ideology, he received political support and a number of proponents of his ideas were appointed professors. The majority of physicists found the movement ridiculous and modern theories were still taught at universities.

For those who were loyal to the Nazi regime or stayed to fight the decline of physics in Germany, the restrictions on German scientists caused isolation: they were forbidden to accept Nobel prizes and attend certain international meetings, and there were heavy restrictions on travelling (Kragh, 1999, Ch. 16). One of the physicists who stayed was Heisenberg. There is some controversy about a conversation he had with Bohr about nuclear energy and his role in developing nuclear weapons for the Nazis; the play *Copenhagen* by Michael Frayn is based on this.

2.1.2 The USA

In the US the physics community grew rapidly in the '20s. American physicists benefited from opportunities to study in Europe and the visits to America by distinguished European physicists, as well as the establishment of new physics institutes and departments, for which large sums of money were made available. By the early '30s American physics was basically on the same level as its European counterpart.

> [In the early 1920's] The prime effort in trying to clarify the mysteries of quantum theory was centered in Germany and Denmark. Our American journal, *The Physical Review*, was only so-so, especially in theory, [. . .] Then, fairly suddenly, at about the time these basic equations were established [in the last half of the 1920's] and many applications to specific problems were possible, America came of age

> in physics, for although we did not start the orgy of quantum mechanics, our young theorists joined it promptly. [...] One measure of a country's prowess in science is the stature of its journals. By 1930 or so, the relative standings of *The Physical Review* and *Philosophical Magazine* were interchanged as compared with the earlier period that I have cited. (Van Vleck, 1964)

In the early '30s the Great Depression brought a setback to American physics, but by 1935 things began to improve again. The influx of European physicists provided further expertise. Helghe Kragh writes:

> The number of industrial research laboratories rose from about 300 in 1920 to more than 2,200 in 1940. In a longer perspective, the years of depression were just a minor disruption of the general trend of growth in American physics. This growth and the general vigor of American physics were an essential factor in the country's ability to absorb the many European refugee physicists who arrived in the 1930s. (Kragh, 1999, Ch. 17)

Similarly, the American physicist John Slater recalls:

> In 1920 theoretical physics was something which had to be imported from Europe. By 1940 it was being exported. (Slater, 1967)

Schweber (Schweber, 1986) argues that the American universities had a different way of educating their physicists. Where theorists and experimentalists usually worked in different departments in European institutes, and theorists engaged in philosophical discussions, the American universities had a more unified tradition. Theorists and experimentalists belonged to the same department, and one of the main tasks for the theorist was to do detailed analyses of experiments. Hence, they had a more pragmatic outlook, and Schweber quotes De Tocqueville, who argues that this pragmaticism was a typically American trait.

2.2 Nuclear physics and the bomb

2.2.1 A very concise history

The field of nuclear physics was born with the discovery of radioactivity in 1896 and the subsequent research by Henri Becquerel and Marie and Pierre Curie. After Rutherford's discovery of the nucleus and his model of the atom around 1910, in which only protons were postulated, James Chadwick suggested the existence of the neutron in 1932. This new particle did not only resolve problems explaining the mass and spin of atoms, it also made possible calculations on binding energy and nuclear reactions, using Einstein's famous 1905 mass-energy equivalence

$$E = mc^2. \tag{56}$$

A fast development of the field of nuclear physics followed. In 1934 Fermi explained the weak nuclear force and in 1935 Yukawa introduced the strong force. At the same time, particle accelerators were being developed, so that physicists could collide particles at high energies in order to study these new theories.

In 1938 Otto Hahn bombarded uranium with neutrons, and Lise Meitner and Otto Frisch correctly interpreted his experimental results as nuclear fission. Leó Szilárd was the first to suggest using uranium in a nuclear chain reaction to produce energy for military purposes. He and Fermi worked together on exploring the possibility on a nuclear reactor (Szilárd, 1969).

Meanwhile, Szilárd informed the infamous Einstein, who realized the importance of these ideas and was prepared to sign a letter directed to President Roosevelt (the Einstein-Szilárd letter) that suggested the possibility of a nuclear bomb. A committee was put in place, but initially the response fell short.

2.2.2 The Manhattan project

Shortly after the attack on Pearl Harbour in December 1941 and the subsequent declaration of war by the US on Japan and Germany, the S-1 Uranium Committee set aside millions of dollars for the development of the nuclear program. An early landmark in this project was the world's first self-sustaining nuclear chain reaction in the Chicago Pile-1, supervised by Fermi in 1942.

The American physicist Robert Oppenheimer was appointed director of the group that would design and build the bomb. Their military laboratory was built in Los Alamos, New Mexico. Other laboratories and plants were constructed across the country.

Another central figure in the project was Von Neumann, who designed a part of the bomb and was involved in choosing the targeted Japanese cities. He also supervised the calculations of explosion simulations, for which he brought the electronic computer to Los Alamos in 1945.

After the first test bomb detonation (code named Trinity) in Juli 1945, this tremendous nationwide effort led to the bombing of Hiroshima and Nagasaki in which hundreds of thousands were killed – and the subsequent surrender of Japan.

2.2.3 The aftermath

The number of scientific publications obviously dramatically dropped during the war. Only a small fraction of physicists were still working on "ordinary" matters, including Dirac, Schrödinger and Born. The physics community needed a few years to recover and by 1950 the old number of publications was restored. However, it would not stop there – the success of the Manhattan project made clear that the development of physics was important for military purposes. It is not a surprise that the US, in the wake of the Cold War, continued to heavily fund physics education and research throughout the country (Forman, 1987).

Around this time the sense of a communist threat in the United States increased. Following the first atomic bomb test by the Soviet Union and the Chinese civil war in 1949, and the start of the Korean War in 1950, a period known as the Second Red Scare began. A central figure in this period was senator Joseph McCarthy, who was to a large extent responsible for the anti-communist crusade that ensued. Many actors, professors and government officials were investigated by the notorious House Un-American Committee (HUAC); the allegations were not always fair and well grounded.

A discussion of how these conditions affected the foundational debate in quantum physics will be given in chapter 4.

3 Foundational work after world war two

Hugh Everett (second from right) meets Niels Bohr (middle) at a seminar at Princeton University in 1954. ("Danish Savant At Princeton." Trenton Times, November 21, 1954. Archive, Box 1, Folder 5.)

The foundational issues of quantum mechanics had not yet been resolved. Yet, from the late '40s to the late '60s, the discussion on quantum foundations was less mainstream. In this section I will discuss two interesting interpretations from this era by Bohm and Everett. Today, these interpretations are seen as serious contenders in the interpretive debate. I will also cover two important "no go" theorems by Bell, and Kochen and Specker.

3.1 De Broglie-Bohm theory

In 1952 the American physicist David Bohm published a hidden variable interpretation, showing that Von Neumann's impossibility theorem did not hold in all cases. He presented a microstructure for quantum mechanics with deterministic particle trajectories, in which nonclassical effects are described by a quantum potential (Bohm, 1952).

> But in 1952 I saw the impossible done. It was in papers by David Bohm. [...] Long may Louis de Broglie continue to inspire those who suspect that what is proved, by impossibility proofs, is lack of imagination. (Bell, 1982)

This description is called *De Broglie-Bohm theory* as it can be seen as an extension to De Broglie's theory of the pilot wave (section 1.4.3).

3.1.1 The theory

To construct the equations of motion for De Broglie-Bohm theory, we look at a single particle with well-defined position $\mathbf{x}$ and velocity

$$\mathbf{v}(\mathbf{x}, t) = \frac{d\mathbf{x}}{dt}. \tag{57}$$

Now, we wish to find a Galilean covariant expression for the velocity vector.

The gradient of a scalar function transforms correctly under rotations, which suggests that the velocity field should go as

$$\mathbf{v}(\mathbf{x}, t) \sim \boldsymbol{\nabla}\psi(\mathbf{x}, t). \tag{58}$$

This is exactly what De Broglie did in his pilot wave formalism.

Moreover, we need time-reversal invariance: $\mathbf{v}(\mathbf{x}, t) \mapsto -\mathbf{v}(\mathbf{x}, -t)$. Now, let $\psi(\mathbf{x}, \mathbf{t}) \mapsto \psi^*(\mathbf{x}, -\mathbf{t})$, so then

$$\mathbf{v}(\mathbf{x}, t) \sim \mathrm{Im}\boldsymbol{\nabla}\psi(\mathbf{x}, t) \tag{59}$$

transforms correctly.

Now we look at boosts $\mathbf{v} \mapsto \mathbf{v}' = \mathbf{v} + \mathbf{u}$. The simplest way to do this is to take

$$\psi' = e^{i\mathbf{x}'\cdot\mathbf{u}/\alpha}\psi, \tag{60}$$

where α is some real constant, so that

$$\alpha\mathrm{Im}\frac{\boldsymbol{\nabla}'\psi'}{\psi'} = \alpha\mathrm{Im}\left[\frac{1}{e^{i\mathbf{x}'\cdot\mathbf{u}/\alpha}\psi}\left(\frac{i\mathbf{u}}{\alpha}e^{i\mathbf{x}'\cdot\mathbf{u}/\alpha}\psi + e^{i\mathbf{x}'\cdot\mathbf{u}/\alpha}\boldsymbol{\nabla}\psi\right)\right] = \alpha\mathrm{Im}\frac{\boldsymbol{\nabla}\psi}{\psi} + \mathbf{u}. \tag{61}$$

The velocity becomes

$$\mathbf{v}(\mathbf{x}, t) = \alpha\mathrm{Im}\frac{\boldsymbol{\nabla}\psi}{\psi}(\mathbf{x}, t) \tag{62}$$

For ψ we take the simplest extension of the Poisson equation, that behaves well under time reversal and boosts, which is the Schrödinger equation:

$$-\frac{h^2}{2m}\nabla^2\psi + V\psi = i\hbar\frac{\partial\psi}{\partial t}. \tag{63}$$

Also, we can now set $\alpha = \frac{\hbar}{m}$ to find the correct wave function phase factor, so we find the final form of the guiding equation

$$\boxed{\frac{d\mathbf{x}}{dt} = \frac{\hbar}{m}\mathrm{Im}\frac{\boldsymbol{\nabla}\psi}{\psi}(\mathbf{x}, t)} \tag{64}$$

We can compare these equations to Newtonian mechanics, by writing

$$\psi = Re^{iS/\hbar}, \tag{65}$$

where $R(\mathbf{x}, t)$ and $S(\mathbf{x}, t)$ are real functions. We find, using equation 64:

$$\mathbf{p} = m\mathbf{v} = \boldsymbol{\nabla}S. \tag{66}$$

We can define the probability density $\rho = R^2$ and the *quantum potential*

$$U = -\frac{\hbar^2}{2m}\frac{\nabla^2 R}{R}, \tag{67}$$

so that plugging equation 65 in equation 63 yields, after separating the real and the imaginary parts:

$$\frac{\partial\rho}{\partial t} + \boldsymbol{\nabla}\cdot\left(\rho\frac{\boldsymbol{\nabla}S}{m}\right) = 0, \tag{68}$$

$$\frac{\partial S}{\partial t} = -\frac{(\boldsymbol{\nabla}S)^2}{2m} - (V + U). \tag{69}$$

Now we can interpret equation 68 as a continuity equation with $\rho\frac{\boldsymbol{\nabla}S}{m} = \rho\mathbf{v} = \mathbf{j}$:

$$\frac{\partial\rho}{\partial t} + \boldsymbol{\nabla}\cdot\mathbf{j} = 0. \tag{70}$$

Because the same relation holds in regular quantum mechanics for $|\psi|^2$ instead of ρ, this makes sure that if $\rho(\mathbf{x}, t_0) = |\psi(\mathbf{x}, t_0)|^2$ at some time t_0, it will be $\rho(\mathbf{x}, t) = |\psi(\mathbf{x}, t)|^2$ at all t; a property that is called *equivariance*.

We also see that equation 69 is simply the Hamilton-Jacobi equation for a single particle, where S is the action and $V + U$ is the total potential energy:

$$\frac{\partial S}{\partial t} = -\frac{\mathbf{p}^2}{2m} - (V + U). \tag{71}$$

The equation of motion now is

$$\frac{d\mathbf{p}}{dt} = -\boldsymbol{\nabla}(V + U). \tag{72}$$

Quickly checking this with $U \to 0$ neatly gives us the expected $\frac{d\mathbf{p}}{dt} = -\boldsymbol{\nabla} V = \mathbf{F}$ in the classical limit. The theory has been presented in this Newtonian form by many authors, including in the original paper of Bohm, and equation 72 has been called the "quantum force". However, essentially this is only an analogy: the theory is not Newtonian and force is not an element of the theory; we only need equations 63 and 64 to fully describe the theory.

Summarizing, we see that the basic ontology of De Broglie-Bohm mechanics consists of particles with ("hidden") positions and wave functions. Equations 63 and 64 describe the evolution of the wave function and the way that the particle motion depends on the wave function.

In this theory, particles have well-defined position and velocity at any time. Hence, the uncertainty principle is an epistemic law as opposed to an ontic law. It is simply a practical limitation of our knowledge about the quantum system.

To complete the theory, and assure observational equivalence with regular quantum mechanics, Bohm postulated in his original papers (Bohm, 1952) that the positions of the particles satisfy the statistical distribution given by Born's rule:

$$\rho(\mathbf{x}, t) = |\psi(\mathbf{x}, t)|^2. \tag{73}$$

Work has been done on deriving this from De Broglie-Bohm theory, instead of postulating it. In 1953 Bohm gave an argument as to why $\rho \to |\psi|^2$, even if initially $\rho \neq |\psi|^2$, because of random interactions (Bohm, 1953). After all, when the particles happen to reach the equilibrium distribution at one point in time, equivariance dictates that they will stay there. Further justification of this *quantum equilibrium hypothesis*, in the case of an ensemble of systems with identical wave functions, is given in Dürr and Teufel's book *Bohmian Mechanics* (Dürr and Teufel, 2009, Ch. 11.4). The derivation of the quantum equilibrium hypothesis is still an active issue, but numerical simulations seem to indicate that systems in non-equilibrium reach equilibrium on short time scales, as expected (Towler et al., 2011).

Finally, to include spin, for example particles with spin $\frac{1}{2}$, we write

$$\frac{d\mathbf{x}}{dt} = \frac{\hbar}{m}\mathrm{Im}\frac{\psi^* \boldsymbol{\nabla}\psi}{\psi^*\psi} = \frac{\hbar}{m}\mathrm{Im}\frac{\sum_{i=1}^{2}\psi_i^* \boldsymbol{\nabla}\psi_i}{\sum_{j=1}^{2}\psi_j^*\psi_j}, \tag{74}$$

where ψ is some two-component wave function (Dürr and Teufel, 2009, Ch. 8.4).

Pauli wrote in a letter to Bohm about his work in 1951:

> I do not see any longer the possibility of any logical contradiction as long as your results agree completely with those of the usual wave mechanics and as long as no means is given to measure the values of your hidden parameters both in the apparatus and in the observed system. (von Meyenn, 2005, Band IV, Teil I, p. 436)

De Broglie-Bohm theory gives the same experimental predictions[17] as the conventional theory. As a result of this underdetermination, the arguments in

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[17] Only in the case that we drop the equilibrium hypothesis – if we do not give the system

favour of it and the objections against it pertain to which requirements for the nature of reality we find acceptable. This is precisely what Bohm wanted to show: when it comes to interpreting the quantum mechanical formalism, the Copenhagen interpretation is not necessarily the only viable one – which interpretation we choose depends on "philosophical tastes".

3.1.2 Measurement

In De Broglie-Bohm theory there is no collapse of the wave function – the Schrödinger equation gives us the only possible time evolution of the system. As a result there is no such thing as the measurement problem (Dürr and Teufel, 2009, Ch. 9.1).

In order to understand this, let us pose the measurement problem with a typical measurement setup. A quantum system and a measurement apparatus interact in the time interval t_0 to t_1. We assume that the measurement apparatus – which usually ends up being a *pointer* pointing at a value on a scale – also has a wave function, albeit a macroscopic one. Let the system wave function be $|\psi(\mathbf{x})\rangle$ and the pointer wave function be $|\phi(\mathbf{y})\rangle$, where $\mathbf{x}$ denotes the coordinates of the system particles and $\mathbf{y}$ those of the "pointer particles". The pointer has an initial ("null") wave function $|\epsilon_0\rangle$.

We look at a system in which the wave function is a simple superposition

$$|\psi\rangle = \alpha_1 |\psi_1\rangle + \alpha_2 |\psi_2\rangle\,, \quad |\alpha_1|^2 + |\alpha_2|^2 = 1. \tag{75}$$

In order for the measurement to give a result, at t_1 the pointer wave function has two possible final wave functions, $|\epsilon_1(\mathbf{y})\rangle$ (for $\mathbf{y}$ in some region Y_1) and $|\epsilon_2(\mathbf{y})\rangle$ (for $\mathbf{y}$ in some region Y_2 with $Y_1 \cup Y_2 = \emptyset$). The Schrödinger evolution during the interaction must be constructed in such a way that

$$|\psi_i\rangle |\epsilon_0\rangle \xrightarrow{\text{interaction}} |\psi_i\rangle |\epsilon_i\rangle\,, \quad i = 1, 2. \tag{76}$$

Now we find, using the Schrödinger evolution:

$$|\psi\rangle |\epsilon_0\rangle = \sum_{i=1,2} \alpha_i |\psi_i\rangle |\epsilon_0\rangle \xrightarrow{\text{interaction}} \sum_{i=1,2} \alpha_i |\psi_i\rangle |\epsilon_i\rangle\,. \tag{77}$$

In the orthodox interpretation[18], this situation (being quite analogous to Schrödinger's cat) gives rise to the measurement problem: is the (macroscopic) pointer simultaneously pointing at 1 and 2, until it is observed?

In De Broglie-Bohm theory this is not a problem, as the pointer value turns out to be either 1 or 2. After all, coordinate $\mathbf{y}$ follows the guiding equation, so it depends in a deterministic way on the initial coordinates $(\mathbf{x_0}, \mathbf{y_0})$. Note that

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enough time to reach quantum equilibrium – we may find different results. This means that De Broglie-Bohm theory might not be just an interpretation but an actual alternative theory after all. Work on quantum nonequilibrium has been done by Valentini. (Valentini, 1991)

[18] In the orthodox interpretation the measurement device is a classical entity and therefore does not have a wave function, and the coordinates of the particles are fuzzy, so this way of describing a measurement would technically not hold.

the dependence on $\mathbf{y_0}$ means that the theory is *contextual*: the measurement outcome will depend on the configuration of the pointer. In section 3.4 we will see that it is not possible to make a hidden variable theory that is noncontextual.

> However, in our suggested new interpretation, so-called 'observables' are, as we have seen [. . .], not properties belonging to the observed system alone, but instead potentialities whose precise development depends just as much on the observing apparatus as on the observed system. (Bohm, 1952)

For an ensemble of systems with identical wave functions and typical particle configurations we find pointer values 1 or 2 with respective probabilities $|\alpha_1|^2$ and $|\alpha_2|^2$, by the virtue of the quantum equilibrium hypothesis. Similarly, the system particles have ("hidden") initial coordinates $\mathbf{x_0}$ and the system wave function $|\psi\rangle$ stands for the statistical spread of particles in such an ensemble.

Note that the theory only gives a procedure for position measurements. It is argued, however, that every measurement is a position measurement. When we measure momentum, we simply measure position twice. When we measure spin, we measure the position of the particle after it has gone through a Stern-Gerlach apparatus. Similarly, every measurement result is given in terms of the position of a pointer or even the position of ink on paper.

3.1.3 Properties of the theory

3.1.3.1 Determinism

It is immediately clear that the theory is deterministic. Once the initial position x_0 of a particle in a certain quantum system is given, its trajectory $x(t)$ is fully specified: the dynamics of the system are fully defined if its initial configuration is known, meaning that the theory is deterministic.

3.1.3.2 Nonlocality

De Broglie-Bohm theory is nonlocal. This is perhaps the most controversial property of the theory – nonlocality has been a topic of discussion since Newton. It is clear however that the nonlocality of the theory cannot be used as an argument in favour of the orthodox interpretation, because the latter is also nonlocal, as pointed out in the EPR paradox.

In Bohm's work the nonlocality can be easily seen, if we consider two entangled particles with coordinates $\mathbf{x}_1(t)$ and $\mathbf{x}_2(t)$, where the wave function is not a product $\psi(\mathbf{x},\mathbf{y}) = \psi_1(\mathbf{x})\psi_2(\mathbf{y})$. We find

$$\frac{d\mathbf{x}_1}{dt} = \frac{\hbar}{m}\mathrm{Im}\frac{\frac{\partial}{\partial\mathbf{x}}\psi(\mathbf{x},\mathbf{x}_2(t))\big|_{\mathbf{x}=\mathbf{x}_1(t)}}{\psi(\mathbf{x}_1(t),\mathbf{x}_2(t))}. \tag{78}$$

In other words, $\mathbf{x}_1(t)$ depends on $\mathbf{x}_2(t)$, even in the case that the particles are far apart. However, this type of nonlocality, as mitigated by the wave function, does not enable transfer of information between two spacelike seperated observers in the same way that entanglement in the Copenhagen interpretation does not enable communication, so it does not violate relativity. This is easy to understand, as the Bohmian theory gives the same experimental predictions as regular quantum mechanics, and we have seen before that in the EPR case the expectation values for the second observer do not change.

The Bohmian nonlocality is however subtly different from the Copenhagen nonlocality, because in this case the effect is clearly *mechanical*, as we are talking about positions of actual particles.

I agree with proponents of Bohmian mechanics that nonlocality is an ideological issue, and not a conceptual one. Cushing states that the constraint of locality is simply a traditional or psychological construct.

> The origins of the uneasiness about nonlocality may be more psychological than logical. (Cushing, 1994, ch. 2.5.2)

David Bohm himself said the following about this subject:

> If the price of avoiding nonlocality is to make an intuitive explanation impossible, one has to ask whether the cost is not too great. (Cushing, 1994, ch. 2.5.2)

Since nonlocality is usually the major argument against Bohm's theory, it would be nice to construct a similar theory with hidden variables that is local. However, in section 3.3 we will see that this is in fact impossible.

3.1.3.3 No minimalism

De Broglie-Bohm theory introduces extra hidden variables in the theory. This is Everett's reason to prefer his own interpretation:

> Our main criticism of this view is on the grounds of simplicity – if one desires to hold the view that if ψ is a real field then the associated particle is superfluous since, as we have endeavored to illustrate, the pure wave theory is itself satisfactory. (DeWitt and Graham, 1973)

3.1.3.4 Contextuality

We have seen in the previous section about measurements that the theory is contextual: the configuration of the pointer influences the result of the measurement.

3.1.4 Common protests

The most common modern protest against De Broglie-Bohm theory, next to its ideological shortcomings, is that it does not recreate all experimental predictions of regular quantum mechanics. However, as far as I know, no conclusive proof of this statement has been published.

For example, the ground state of the electron in a hydrogen atom is given by

$$\psi(r, \theta, \phi) \sim e^{-r/a}, \tag{79}$$

where a is some real constant. This means that

$$\frac{d\mathbf{x}}{dt} = 0. \tag{80}$$

The electron is not moving at all! Some critics argue that this means the predictions of the theory are wrong. However, in an ensemble of hydrogen atoms, the electrons will have the expected distribution, as stated by the equilibrium hypothesis. Moreover, if we perform a measurement on a single electron in the ground state, we alter its velocity, due to the interaction with the measurement apparatus.

3.2 Relative state and the many worlds interpretation

3.2.1 Relative state formulation

In Hugh Everett's 1957 thesis, a number of questions were raised that the Copenhagen interpretation cannot answer. His dissatisfaction was based on the nonuniversality, or the classical observer-quantum object dualism, in the orthodox interpretation.

> How are a quantum description of a closed universe, of approximate measurements, and of a system that contains an observer to be made? These three questions have one feature in common, that they all inquire about the *quantum mechanics* that is *internal to an isolated system.* (Everett III, 1957)

He makes two basic assumptions in an attempt to answer these questions. Firstly, he postulates that the Schrödinger equation is the only possible time evolution for any isolated quantum system – there is no discontinuous collapse of the wave function. Secondly, he states that a system that is being observed externally (and is therefore not isolated) can be seen as part of a larger isolated system in which the observer is included.

This line of thought invites us to think about a *universal wave function* – a wave function that describes the whole universe, being the ultimate isolated system, including all the observers within it. This makes it an ideal starting point for quantum cosmology. All physics would follow from this single wave function. Everett's thesis was devoted to showing that this concept is logically self consistent, hence providing a complete (non dualistic) description for reality without supplementary hidden variables.

Consider again the description of a simple measurement with a system wave function $|\psi\rangle = \alpha_1 |\psi_1\rangle + \alpha_2 |\psi_2\rangle$ and possible post-measurement pointer wave functions $|\phi_1\rangle$ and $|\phi_2\rangle$, as stated in paragraph 3.1.2, with a Schrödinger evolution

$$|\psi\rangle |\phi_0\rangle = \sum_{i=1,2} \alpha_i |\psi_i\rangle |\phi_0\rangle \xrightarrow{\text{interaction}} \sum_{i=1,2} \alpha_i |\psi_i\rangle |\phi_i\rangle . \qquad (81)$$

In the orthodox formulation, the pointer superposition collapses into one of its states $|\phi_i\rangle$ with probability $|\alpha_i|^2$. For Everett, however, no collapse occurs and the post-measurement pointer state becomes a superposition of eigenstates. He states the resulting problem as follows:

> It seems as if nothing can ever be settled by such a measurement. [...] This behaviour seems to be quite at variance with our observations, since macroscopic objects always appear to us to have definite positions. Can we reconcile this prediction of the purely wave mechanical theory with experience, or must we abandon it as untenable? (DeWitt and Graham, 1973, p. 61)

According to Everett, each pointer eigenstate in the post-measurement superposition describes an observer perceiving a definite result. For each of these observers the usual wave function collapse *appears* to hold. The pointer has value 1 relative to $|\psi\rangle = |\psi_1\rangle$, and the pointer has value 2 relative to $|\psi\rangle = |\psi_2\rangle$, hence the name *relative state*.

More generally, an isolated system state $|\Psi(x_1, x_2, t)\rangle$ cannot define a unique state for a subsystem $|\psi(x_1, t)\rangle$ independent of the state of the remainder $|\phi(x_2, t)\rangle$. Hence, there is usually no way to write $|\Psi(x_1, x_2, t)\rangle = |\psi(x_1, t)\rangle\, |\phi(x_2, t)\rangle$. We can only define a unique state $|\psi_i(x_1, t)\rangle$ *relative* to a *specified* state $|\phi_i(x_2, t)\rangle$ of the remainder of the isolated system:

$$|\Psi\rangle = \sum_i a_i \, |\psi_i(x_1, t)\rangle \, |\phi_i(x_2, t)\rangle \, . \tag{82}$$

In the special case of a measurement, each of these $|\psi_i(x_1, t)\rangle$ stands for a branch in which the observer makes a definite measurement of the subsystem consisting of x_2.

In this account it is unclear how one of these branches is "chosen by actuality", so that the pointer will eventually point at a definite value. Everett writes in a footnote:

> The whole issue of the transition from "possible" to "actual" is taken care of in the theory in a very simple way – there is no such transition, nor is such a transition necessary for the theory to be in accord with experience. From the viewpoint of the theory *all* elements of a superposition (all "branches") are "actual," none any more "real" than the rest. [...] Arguments that the world picture presented by this theory is contradicted by experience, because we are unaware of any branching process, are like the criticism of the Copernican theory that the mobility of the earth as a real physical fact is incompatible with the common sense interpretation of nature because we feel no such motion. In both cases the argument fails when it is shown that the theory itself predicts that our experience will be what it in fact is.

In a letter to Norbert Wiener, he states:

> You also raise the question of what it means to say that a fact or a group of facts is actually realized. Now I realize that this question poses a serious difficulty for the conventional formulation of quantum mechanics, and was in fact one of the the main motives for my reformulation. The difficulty is removed in the new formulation, however, since it is quite unnecessary in this theory ever to say anything like "Case A is actually realized." (Everett, 1957)

There have been a number of attempts to explain determinate measurement records and clarify the ontological status of branches based on Everett's ideas, most notably the 1971 many-worlds interpretation by DeWitt, which we will discuss next. Similar interpretations are the 1988 many-minds interpretation by Albert and Loewer (Albert and Loewer, 1988) and the 1990 consistent histories interpretation by Gell-Mann and Hartle (Gell-Mann and Hartle, 1990).

3.2.2 Properties of the theory

3.2.2.1 Determinism

It is immediately clear that this theory is deterministic. No collapse occurs and no probabilistic interpretation is needed. Everett writes:

> Our theory in a certain sense bridges the positions of Einstein and Bohr, since the complete theory is quite objective and deterministic ("God does not play dice with the universe"), and yet on the subjective level, of assertions relative to observer states, it is probabilistic in the strong sense that there is no way for observers to make any predictions better than the limitations imposed by the uncertainty principle. (DeWitt and Graham, 1973)

3.2.2.2 Locality

We can understand that Everett's theory is local as follows. Consider the EPR argument, and suppose the initial wave function of the electron-positron pair is given by

$$|\psi\rangle = \frac{1}{\sqrt{2}}(|\uparrow\downarrow\rangle - |\downarrow\uparrow\rangle). \tag{83}$$

Now, the wave function of the whole system, including the two observers a and b, is

$$|\Psi\rangle_1 = \frac{1}{\sqrt{2}} |a\rangle \left(|\uparrow\downarrow\rangle - |\downarrow\uparrow\rangle\right) |b\rangle \,. \tag{84}$$

After observer a performs their measurement we find that

$$|\Psi\rangle_2 = \frac{1}{\sqrt{2}}(|a_\uparrow\rangle |\uparrow\downarrow\rangle - |a_\downarrow\rangle |\downarrow\uparrow\rangle) |b\rangle \,. \tag{85}$$

In other words, observer a has obtained a result, but b has not. Now b performs a measurement, after which both observers are in relative states:

$$|\Psi\rangle_3 = \frac{1}{\sqrt{2}}(|a_\uparrow\rangle |\uparrow\downarrow\rangle |b_\downarrow\rangle - |a_\downarrow\rangle |\downarrow\uparrow\rangle |b_\uparrow\rangle). \tag{86}$$

Now, only when a and b communicate their results, the entire system is branched.

3.2.2.3 Noncontextuality

Everett's proposal is noncontextual. The measurement apparatus does not affect the outcome of the measurement – in fact, every outcome is actualised.

3.2.2.4 Minimalism

Everett's theory is minimal. No extra hidden variables have been introduced. It is often argued that his proposal is the ultimate minimal interpretation, because only the existence of ψ and its time evolution have to be postulated. However, there is no consensus on whether the derivation of the Born rule actually holds (Landsman, 2009).

3.2.3 Many-worlds

The many-worlds interpretation by DeWitt is the most popular expansion of Everett's ideas. In a 1970 article in Physics Today (DeWitt and Graham, 1973, p. 155), DeWitt discusses the Copenhagen interpretation, De Broglie-Bohm theory and the relative state formalism and argues in favour of the latter.

> Of the three main proposals for solving this dilemma [the measurement problem], I shall focus on one that pictures the universe as continually splitting into a multiplicity of mutually unobservable but equally real worlds, in each of which a measurement does give a definite result. Although this proposal leads to a bizarre world view, it may be the most satisfying answer yet advanced. (DeWitt and Graham, 1973, p. 155)

In 1971 he elaborates on this notion of a "splitting into a multiplicity of worlds" in an extensive article (DeWitt and Graham, 1973, p. 167). According to DeWitt, every quantum interaction taking place in the universe is splitting the universal state vector into branches, each of which is equally physically real. These different worlds cannot interact with each other.

The difference between DeWitt and Everett is the interpretation of these branches. Where DeWitt saw a branch as a physical world that is causally isolated from the other branches, Everett's understanding of branches was operational – they form an adequate description of quantum mechanics, but are not necessarily real – and he said it is in principle possible for branches to interact. The question as to whether branches can interact is still very much unclear and an active topic of debate.

DeWitt admits that the idea is ontologically extravagant:

> The idea of 10^{100+} slightly different copies of oneself all constantly splitting into further copies, which ultimately become unrecognizable, is hard to reconcile with the testimony of our senses, (DeWitt and Graham, 1973, p. 179)

3.2.4 Conceptual issues

DeWitt's revival of Everett's interpretation made it more well-known, and the technical issues of the interpretation have been extensively explored since. Of these issues, the two most pressing are an inclusion or derivation of the Born rule and the *preferred base problem.*

The first issue asks: if every branch is realized, how can we assign probabilities to them and what do these probabilities mean? Also, is it possible to *derive* the Born rule from the many-worlds interpretation? Everett himself tried to derive it using an analogy of the Lebesgue measure in thermodynamics, and called the result a "quantitative statement about the relative frequencies of the different possible results of observation that are recorded in the memory of a typical observer". However, he used an "additivity requirement" for the measures corresponding to the state coefficients, and it is not clear how this requirement relates to actual physics.

The second issue can be stated as follows: how do we decompose the universal wave function in such a way that each term corresponds to a different world?

Consider again a measurement of the wave function

$$|\psi\rangle = \alpha_1 |\psi_1\rangle + \alpha_2 |\psi_2\rangle\,, \quad |\alpha_1|^2 + |\alpha_2|^2 = 1, \tag{87}$$

with an interaction with a pointer in an initial null state $|\epsilon_0\rangle$ so that

$$|\psi\rangle |\epsilon_0\rangle \xrightarrow{\text{interaction}} \alpha_1 |\psi_1\rangle |\epsilon_1\rangle + \alpha_2 |\psi_2\rangle |\epsilon_2\rangle\,. \tag{88}$$

According to the many-worlds interpretation, $|\psi_1\rangle$ and $|\psi_2\rangle$ stand for different worlds. Now, we can also write ψ as

$$|\psi\rangle = \frac{1}{2}(\alpha_1 + \alpha_2)(|\psi_1\rangle + |\psi_2\rangle) + \frac{1}{2}(\alpha_1 - \alpha_2)(|\psi_1\rangle - |\psi_2\rangle). \tag{89}$$

What allows us to say that the vectors of a certain basis $|\psi_1\rangle$ and $|\psi_2\rangle$ stand for different worlds, but the vectors of another basis $|\psi_1\rangle + |\psi_2\rangle$ and $|\psi_1\rangle - |\psi_2\rangle$ do not?

Zurek attempted to give an answer to this by using quantum decoherence in 1981 (Zurek, 1981). Decoherence has since become very important in the interpretive debate. For instance, the 1990 consistent histories theory is defined in terms of decoherence (Gell-Mann and Hartle, 1990). The idea is that the interaction of a quantum system with its environment (or in the case of equation 88, with the pointer) creates correlations between the system states and the states of the environment (Breuer and Petruccione, 2002, Ch 4.1).

Let us take another look at the system we discussed before (equation 87). The density matrix is

$$\begin{aligned} \rho &= |\psi\rangle \langle\psi| \\ &= |\alpha_1|^2 |\psi_1\rangle \langle\psi_1| + |\alpha_2|^2 |\psi_2\rangle \langle\psi_2| + \alpha_1^* \alpha_2 |\psi_2\rangle \langle\psi_1| + \alpha_2^* \alpha_1 |\psi_1\rangle \langle\psi_2|\,. \end{aligned} \tag{90}$$

The last two terms of this sum are the interaction terms.

Now we consider interactions with the environment ϵ,

$$(\alpha_1 |\psi_1\rangle + \alpha_2 |\psi_2\rangle) |\epsilon_0\rangle \xrightarrow{\text{interaction}} \alpha_1 |\psi_1\rangle |\epsilon_1\rangle + \alpha_2 |\psi_2\rangle |\epsilon_2\rangle = |\Psi\rangle\,. \tag{91}$$

The basis for which this is the case is called the einselected (environmentally induced selected) basis.

Also, by virtue of the large amount of degrees of freedom in the environment, we may assume the *decoherence condition*

$$\langle \epsilon_i | \epsilon_j \rangle = \delta_{i,j} \tag{92}$$

Now we obtain the (reduced) density matrix of the system by tracing over the environment

$$\rho_{\text{dec}} = Tr_\epsilon \sum_i \langle \epsilon_i | \Psi \rangle \langle \Psi | \epsilon_i \rangle = |\alpha_1|^2 |\psi_1\rangle \langle \psi_1| + |\alpha_2|^2 |\psi_2\rangle \langle \psi_2| \,. \tag{93}$$

The interference terms have disappeared in the einselected basis due to decoherence (Zurek, 2002).

Quantum decoherence is now mostly studied outside of the many-worlds framework and regarded as appropriate explanation for the preferred basis problem in a broader sense. It tells us that a quantum system evolves to a mixture of states that correspond to the states we measure, because of its interaction with the environment. It is often said that decoherence solves the measurement problem, but this is not immediately clear: we still have no knowledge about what the actual measurement outcome is. In many-worlds, of course, both measurement outcomes are actualized. Note that decoherence implies splitting in the subsystem that is being measured, but not splitting of the universal wave function. A common critique against the decoherence answer to the preferred basis problem, is that it is not sufficient in realistic situations.

3.3 Bell's theorem

Together, the EPR papers and Bohm's work provided a major motivation for John Bell's famous article, which was published in 1964 (Bell et al., 1964). His theorem says that quantum mechanical descriptions cannot simultaneously meet the following requirements:

(1) *Hidden Variables*: observables of any quantum system have definite values that can be specified with extra (hidden) variables;

(2) *Locality*: objects are influenced directly only by their immediate surroundings.

Consider the EPR-experiment, in which two entangled spin-$\frac{1}{2}$ particles are brought far apart before measuring their spin. However, this time observer A and B are free to choose which direction of the spin to measure, denoted by respectively $\mathbf{a}$ and $\mathbf{b}$, with $|\mathbf{a}| = |\mathbf{b}| = 1$. Their measurements are now denoted by

$$\sigma_{\mathbf{1}} \cdot \mathbf{a} = \pm 1, \quad \sigma_{\mathbf{2}} \cdot \mathbf{b} = \pm 1. \tag{94}$$

If $\mathbf{a}$ and $\mathbf{b}$ are parallel this experiment is of course the same as the EPR example, so that $\sigma_{\mathbf{1}} \cdot \mathbf{a} = -\sigma_{\mathbf{2}} \cdot \mathbf{b}$. If however $\mathbf{a}$ and $\mathbf{b}$ are perpendicular, the measurements of A and B will not be statistically correlated at all.

Quantum mechanics dictates that the expectation value of the product of the measurements is equal to

$$P_{QM}(\mathbf{a}, \mathbf{b}) = \langle (\sigma_{\mathbf{1}} \cdot \mathbf{a})(\sigma_{\mathbf{2}} \cdot \mathbf{b}) \rangle = -\mathbf{a} \cdot \mathbf{b} = -\cos\theta_{a,b}, \tag{95}$$

where $\theta_{a,b}$ is the angle between $\mathbf{a}$ and $\mathbf{b}$.

Now assume the existence of hidden variables λ that give a complete specification of the state of the quantum system, with $\rho(\lambda)$ the corresponding probability distribution. If we assume that these hidden variables are *local*, the measurement results are $A(\mathbf{a}, \mathbf{b}, \lambda) = A(\mathbf{a}, \lambda) = \pm 1$ and $B(\mathbf{a}, \mathbf{b}, \lambda) = B(\mathbf{b}, \lambda) = \pm 1$. The expectation value of their product is

$$P_{HV}(\mathbf{a}, \mathbf{b}) = \int A(\mathbf{a}, \lambda) B(\mathbf{b}, \lambda) \rho(\lambda) d\lambda. \tag{96}$$

In the original EPR situation we have seen that $A(\mathbf{n}, \lambda) = -B(\mathbf{n}, \lambda)$, which we can use to write equation 96 as

$$P_{HV}(\mathbf{a}, \mathbf{b}) = -\int \rho(\lambda) A(\mathbf{a}, \lambda) A(\mathbf{b}, \lambda) d\lambda. \tag{97}$$

Because $A(\mathbf{n}, \lambda)^2 = 1$, we can write

$$\begin{aligned} P_{HV}(\mathbf{a}, \mathbf{b}) - P_{HV}(\mathbf{a}, \mathbf{d}) &= -\int \left[A(\mathbf{a}, \lambda) A(\mathbf{b}, \lambda) - A(\mathbf{a}, \lambda) A(\mathbf{d}, \lambda) \right] \rho(\lambda) d\lambda \\ &= \int A(\mathbf{a}, \lambda) A(\mathbf{b}, \lambda) \left[A(\mathbf{b}, \lambda) A(\mathbf{d}, \lambda) - 1 \right] \rho(\lambda) d\lambda. \end{aligned} \tag{98}$$

This means, considering that $|A(\mathbf{a},\lambda)A(\mathbf{b},\lambda)| = 1$, that

$$|P_{HV}(\mathbf{a},\mathbf{b}) - P_{HV}(\mathbf{a},\mathbf{d})| \leq \int [1 - A(\mathbf{b},\lambda)A(\mathbf{d},\lambda)]\, \rho(\lambda) d\lambda. \tag{99}$$

As $\int \rho(\lambda)d\lambda = 1$, we find the original *Bell inequality*

$$\boxed{1 + P_{HV}(\mathbf{b},\mathbf{d}) \geq |P_{HV}(\mathbf{a},\mathbf{b}) - P_{HV}(\mathbf{a},\mathbf{d})|.} \tag{100}$$

If we assume that $P_{HV}(\mathbf{x},\mathbf{y}) = P_{QM}(\mathbf{x},\mathbf{y}) = -\cos\theta_{x,y}$, it is very easy to find values for $\mathbf{a}$, $\mathbf{b}$, $\mathbf{c}$ and $\mathbf{d}$ in which this inequality is violated.

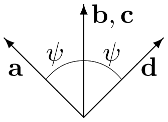

Figure 2: Example of a configuration in which Bell violation occurs. All the vectors are in the same plane and $\psi = \frac{\pi}{3}$.

Consider the configuration in figure 2. In this case we find

$$1 - \frac{1}{2} \geq | - \frac{1}{2} - \frac{1}{2}|, \tag{101}$$

which is obviously false, and this means that our assumption $P_{HV} = P_{QM}$ was wrong.

It is not immediately clear that the above argument also holds for contextual hidden variable theories like De Broglie-Bohm theory. Namely, in that case the measurement results also depend on some independent hidden variables of the measurement apparati μ and ν

$$A(\mathbf{a},\lambda,\mu), B(\mathbf{b},\lambda,\nu). \tag{102}$$

However, a Bell inequality can be derived for this case too. Moreover, Bell's theorem can be generalized to stochastic hidden variable theories, where λ indicates the probability of finding a certain outcome. Regular quantum mechanics can be understood this way. Hence, the Copenhagen interpretation must be nonlocal as well (Seevinck, 2014, Ch. 7).

Bell violations have been experimentally verified a number of times. The first Bell test was done by Freedman and Clauser in 1972 (Freedman and Clauser, 1972).

Interestingly, the many-worlds theory escapes from the Bell inequalities because measurements do not have a single outcome.

3.4 Kochen-Specker theorem

Bell had shown in 1966 that Von Neumann had made an unwarranted assumption in his impossibility proof (Bell, 1966). A year later Kochen and Specker published a theorem that refined the assumption. The theorem states that quantum mechanical descriptions cannot simultaneously meet the following requirements: (Kochen and Specker, 1967)

(1) *Hidden Variables*: observables of any quantum system have definite values that can be specified with extra (hidden) variables;

(2) *Noncontextuality*: values of observables are independent of the measurement arrangement.

Let H_d be a Hilbert space of dimension $d \geq 3$ and let A, B, C,... be a set of n observables. We now have to show that the following assumptions contradict eachother:

(I) All observables simultaneously have values, designated by $v(A)$, $v(B)$, $v(C)$,...;

(II) If A, B and C are compatible, then:

  (a) If $A + B = C$, then $v(A) + v(B) = v(C)$;

  (b) If $A \cdot B = C$, then $v(A) \cdot v(B) = v(C)$.

The first assumption (I) of *value definiteness* is of course a consequence of the hidden variables requirement (1). Note that the sum rule (IIa) and the product rule (IIb) relate the values of *compatible* observables, as opposed to Von Neumann's assumption. In the original proof, $d = 3$ and $n = 117$, making the process highly laborious. In 1996 a simpler but slightly weaker proof was given for $d = 4$ and $n = 18$ which I will discuss (Cabello et al., 1996).

Let u_1, u_2, u_3 and u_4 be four orthogonal vectors in H_4. Let P_1, P_2, P_3 and P_4 be (commuting) projector operators on these vectors ($P_i = |u_i\rangle\langle u_i|$), with the property that $v(P_i)$ is either 0 or 1. This follows from $P_i^2 = P_i \rightarrow v(P_i)^2 = v(P_i)$, using the product rule (IIb). We know

$$P_1 + P_2 + P_3 + P_4 = I, \tag{103}$$

so that

$$\boxed{v(P_1) + v(P_2) + v(P_3) + v(P_4) = v(P_1 + P_2 + P_3 + P_4) = v(I) = 1.} \tag{104}$$

The first part of this equation follows from the sum rule (IIa). Because $v(P_i)$ is either 0 or 1, it follows that in any set of projector operators there is *one* i for which $v(P_i)$ is 1; the other values must be 0.

Now consider the 9 setups for orthogonal vectors u_1, u_2, u_3 and u_4 in table 1. These have been chosen so that there are 18 vectors that each occur in the table

	setup 1	setup 2	setup 3	setup 4	setup 5	setup 6	setup 7	setup 8	setup 9
u_1	(0,0,0,1)	(0,0,0,1)	(1,-1,1,-1)	(1,-1,1,-1)	(0,0,1,0)	(1,-1,-1,1)	(1,1,-1,1)	(1,1,-1,1)	(1,1,1,-1)
u_2	(0,0,1,0)	(0,1,0,0)	(1,-1,-1,1)	(1,1,1,1)	(0,1,0,0)	(1,1,1,1)	(1,1,1,-1)	(-1,1,1,1)	(-1,1,1,1)
u_3	(1,1,0,0)	(1,0,1,0)	(1,1,0,0)	(1,0,-1,0)	(1,0,0,1)	(1,0,0,-1)	(1,-1,0,0)	(1,0,1,0)	(1,0,0,1)
u_4	(1,-1,0,0)	(1,0,-1,0)	(0,0,1,1)	(0,1,0,-1)	(1,0,0,-1)	(0,1,-1,0)	(0,0,1,1)	(0,1,0,-1)	(0,1,-1,0)

Table 1: 9 different setups for orthogonal vectors in four dimensions.

twice. We must now choose values for the corresponding projection operators, so that in every column there is only one vector for which the operator value is 1. In the literature this is usually translated into a colouring problem: in every column exactly one field must be coloured white, whilst the rest is coloured black, giving a total of 9 white fields. However, according to the noncontextuality requirement (2) the setup configuration cannot influence the value of a projection operator corresponding to a particular vector. So if we colour one field white, we must always colour the other field with the same vector white too: the number of white fields is always even and cannot be 9. This means our requirements cannot hold simultaneously.

3.5 A systematic comparison of interpretations

The only viable and complete interpretation of quantum mechanics before the war was the Copenhagen interpretation. We have seen that two new viable interpretations were proposed in the '50s. We are now ready to do a systematic comparitive study between these interpretations.

3.5.1 Conceptual issues

3.5.1.1 The Copenhagen interpretation

(a) *No well-defined measurement process:* it is not clear what physical process causes projection during measurement;

(b) *No universality:* a universal interpretation describes the entire universe – the key requirement for this property is that the observer is internal to the quantum system, as the universe cannot have an external observer. In the Copenhagen interpretation, however, observers are always external.

The Copenhagen interpretation does not have appropriate answers to either of these difficulties, because the quantum-classical duality is central in its description of the measurement process.

3.5.1.2 De Broglie-Bohm theory

(a) *Quantum equilibrium hypothesis:* can we prove the quantum equilibrium hypothesis, stating that an ensemble of particles reaches the expected $|\psi|^2$ distribution? Numerical calculations seem to indicate that it is so;

Most objections against De Broglie-Bohm theory seem to be about its ability to recreate all experimental predictions of regular quantum mechanics. However, this is usually based on a misunderstanding of the measurement process in the theory, and as far as I know no conclusive conflicting predictions have been found.

3.5.1.3 Many-worlds interpretation

(a) *Preferred basis problem:* in what representation of the wave function do the terms correspond to worlds? Most authors consider this problem solved by decoherence;

(b) *The Born rule:* it is said that the Born rule gives a "measure of existence of a world", but what does this mean exactly? Moreover, Everett's derivation of the Born rule is sometimes not accepted by critics, but the Born rule can also just be postulated.

3.5.2 Ontology

In the Copenhagen interpretation, the reality of quantum mechanical observables must always be considered within an experimental context. Verdict: unsafe.

De Broglie-Bohm theory describes particles with a well-defined position and momentum. Next to ontic particles, the wave function is also real, and describes both the time evolution of the system and the statistical spread of the particles. Verdict: safe.

In the many-worlds interpretation, the wave function is real. Each decohered branch of the theory stands for a seperate world. Verdict: unsafe.

3.5.3 Ideology

	determinism	locality	minimalism	noncontextuality
CI	no	no	yes	yes
DBB	yes	no	no	no
MW	yes	yes	yes	yes

Table 2: The interpretations and their ideologies.

3.5.4 Conclusions

The Copenhagen interpretation is the easiest interpretation to work with, because of its historical prominence and its use of ad hoc solutions to foundational issues. These ad hoc solutions cause its grave conceptual issues: its nontreatment of the measurement problem and its nonuniversality are very problematic. It is also both ontologically and ideologically unsatisfactory.

De Broglie-Bohm theory gives us the most intuitive world picture in terms of particle locations and trajectories, at the cost of extra variables and nonlocality. This is a good approach from an *ontological* point of view: it makes the underlying physics intuitive but does not give us the most satisfactory philosophical properties. Indeed, Bohm himself preferred to call the theory the Ontological interpretation.

The many-worlds interpretation brings back our "safe" pre-1920s deterministic and local properties of reality, and adds no extra structure to the formalism. The universality of the theory was the major reason for conceiving it. Be that as it may, the assumption that all alternative histories are real can be considered ontologically extravagant. This is a good approach from an *ideological* point of view: the underlying physics is unorthodox but the philosophical properties are satisfactory.

The no-go theorems show that it is not possible to give an interpretation that is both ontologically and ideologically satisfactory. The Kochen-Specker theorem illustrates the impossibility to go back to classical scientific realism, in which the world is described by beables, and measurement is the discovery of these beables. Moreover, the Bell theorem shows that any interpretation with such a safe ontology must be nonlocal.

4 From principles to problems

“A typical Feynman Lecture classroom” (feynmanlectures.info)

The reception of the interpretive work in the ’50s and ’60s contrasts sharply with the interpretive discussion in the ’20s and ’30s. The first section of this chapter is an introduction to the intellectual milieu of the postwar physics community. In the second section I discuss the reception of postwar interpretive work. In the third section I discuss a number of reasons for this attitude change, most of which are direct consequences of developments in the US during the second world war and the cold war. Then I discuss the rise of numerical methods. Finally, I show that the older generation of quantum physicists was fairly conservative when it came to interpretive issues, and the newer generation generally focused on problem solving rather than principles.

4.1 The postwar physics community

The general shift from *physics of principles* to *physics of problem solving* after the war is wonderfully summarized in a quote that is usually attributed to Richard Feynman, who was notoriously hostile to philosophy: “Philosophy of science is about as useful to scientists as ornithology is to birds.”

An editorial in Physical Review Letters by Samuel Goudsmit illustrates how not only philosophy of science, but also discussions in theoretical physics were seen by some physicists as quite useless:

> It has been suggested that Physical Review Letters has ruined theoretical physics. [. . .] The next step might be to equip theorists with portable recorders so that all their statements about physics, including those uttered in their sleep, would be preserved on tape. The contents of the tapes would be transmitted electronically to interested colleagues via a distribution center; computers coded with key words could scan the tapes for information relevant to each user’s interests. Hopefully, such a system might result in such chaos as to make priority assignments impossible, and the great advances in theoretical physics would become anonymous, just like the great achievements in the art of ancient Egypt. (Goudsmit, 1965)

Goudsmit made an announcement in 1973 for Physical Review D (on particles and fields), in which he makes rather strong criteria for foundational papers that will be considered for refereeing. As a justification, he writes that “it should not be overlooked that physics is an experimental science.”

All this sounds very different from the ideas about philosophy and theoretical physics that Einstein and Bohr propagated in the ’20s and ’30s. As the influential philosopher of science Paul Feyerabend put it in 1969:

> The withdrawal of philosophy into a “professional” shell of its own has had disastrous consequences. The younger generation of physicists, the Feynmans, the Schwingers, etc., may be very bright; they may be more intelligent than their predecessors, than Bohr, Einstein, Schrödinger, Boltzmann, Mach and so on. But they are uncivilized savages, they lack in philosophical depth – and this is the fault of the very same idea of professionalism which you are now defending. (Motterlini, 1999)

However, some physicists were still working on philosophical matters.

> H. Margenau has pointed out that the conventional interpretation of quantum measurements which is associated with the name of von Neumann is currently no longer acceptable to a fair number of physicists. (Aharonov et al., 1964)

> Although the structure of the quantum theory in the opinion of almost all physicists is free from contradiction, questions about the

> consistency of its interpretation have been and continue to be posed.
> (DeWitt and Graham, 1973, p. 219 (Cooper and Van Vechten))

There was a small group of determined physicists working on unsolved foundational issues, but the majority of physicists simply did not care about these subjects.

4.2 Reception of postwar interpretive ideas

The mainstream quantum physics community from the '40s to the '60s is characterized by a conservative attitude. In this section I illustrate this by discussing the reception of the previously discussed ideas in this period. It generally seems to be the case that they were virtually ignored until the '70s or '80s, after which they were "rediscovered" and significant progress was made. The reception of the work of Bohm, Everett and Bell has been extensively researched and will be discussed here.

There is little to no literature on the reception of the Kochen-Specker theorem; however, a quick citation count gives a good indication. The 1967 paper received only a handful of citations the first five years after its publication. In the mid '70s the interest started increasing and today the paper has thousands of citations.

4.2.1 Bohm

In 1947 David Bohm became an assistant professor of physics at Princeton, where he worked closely with Einstein. He was called to appear before the House Committee on Un-American Activities in 1949 – at the very start of the McCarthy era – because of his ties to suspected communists. He decided to plead the Fifth Amendment, to use his right to refuse to testify against his colleagues. As a result he was arrested and suspended from Princeton. After the Supreme Court ruled that he had the right to refuse testimony, no crime had been committed and Bohm was to be released, his contract with Princeton had expired and he reluctantly moved to Brazil, as advised by Oppenheimer. Consequently, he was both physically and intellectually isolated from the physics community (Hiley and Peat, 1987).

The basic idea of the pilot wave had already been put forward by De Broglie in 1927. However, after Von Neumann's impossibility proof and Bohr's victory in the interpretive debates, the physics community (including De Broglie) was convinced that the Copenhagen interpretation was the only viable one. The previously discussed 1952 paper (Bohm, 1952) was meant to show that this is not the case, but the American physics community was virtually uninterested in it. Bohm's exile must have contributed to this lack of discussion, but it is also clear that most physicists of that time simply did not see the point of quibbling over interpretive issues in quantum mechanics. Bohm later turned to other communities to convey his message and became involved in philosophy, psychology and spirituality.

> Because the response to these ideas was so limited, and because I did not see clearly, at the time, how to proceed further, my interests began to turn in other directions. (Hiley and Peat, 1987, ch. 2, David Bohm)

De Broglie-Bohm theory usually remains unmentioned in standard textbooks on quantum mechanics – the Copenhagen interpretation is commonly presented as the only (acceptable) interpretation. In recent years however, a number of physicists and philosophers have committed themselves to developing Bohm's ideas.

> When even Pauli, Rosenfeld and Heisenberg, could produce no more devastating criticism of Bohm's version than to brand it as "metaphysical" and "ideological"? Why is the pilot wave picture ignored in text books? Should it not be taught, not as the only way, but as an antidote to the prevailing complacency? To show that vagueness, subjectivity and indeterminism, are not forced on us by experimental facts, but by deliberate theoretical choice? [...] There are surely other morals to be drawn here, if not by physicists then by historians and sociologists. (Bell, 1982)

4.2.2 Everett

DeWitt writes in his 1971 article on the many worlds interpretation:

> Let me turn immediately to my main purpose, which is to describe one of the most bizarre and at the same one of the most straightforward interpretations of quantum mechanics that has ever been put forward, and that has been unjustifiably neglected since its appearance thirteen years ago. (DeWitt and Graham, 1973, p. 167)

After graduation in 1956 Everett left the realm of theoretical physics to work for the Pentagon. Some take this as a sign that he was disappointed by the reception of his work. In fact, Everett had started working for the Pentagon even before his thesis was finished, so it can also be taken as one of the reasons for Everett's work to be underrepresented, as he left the community so rapidly after graduation.

Everett's mentor John Wheeler, a highly influential physicist at the time, initially provided backing for his thesis. He wrote in his assessment:

> No escape seems possible from this relative state formulation if one wants to have a complete mathematical model for the quantum mechanics that is internal to an isolated system. Apart from Everett's concept of relative states, no self-consistent system of ideas is at hand to explain what one shall mean by quantizing a closed system like the universe of general relativity. (DeWitt and Graham, 1973)

Mostly due to Wheeler's influence, the theory was discussed in the "higher circles" of quantum mechanics on a handful of occasions. Pre-prints were sent to distinguished physicists, some of which, including DeWitt, Wiener and Margenau, responded favourably. Wheeler took a draft of the thesis to Copenhagen to discuss it with Bohr. During the famous 1957 Chapel Hill conference on gravitation, Wheeler brought up Everett's universal wave function after which Feynman commented on it (DeWitt and Rickles, 2011, p. 270). However, the discussion in the physics community was rather short-lived.

It is a curious fact that Wheeler considered Everett's work to be an *extension* (as opposed to a competitor) of the Copenhagen interpretation. Wheeler had previously worked with Bohr and was strongly devoted to his approach.

Everett himself had informal discussions about his thesis with a few notable physicists of the old quantum generation, including Bohr, Rosenfeld and Podolski. Unsurprisingly, the work was met with considerable resistance by members of the Copenhagen group, because they were unable to reconcile it with some of the school's most fundamental doctrines. It was labeled as being metaphysical, lacking experimental context for its description of measurements and failing to recognize the fundamental role of irreversibility in the measurement process (Osnaghi et al., 2009). Everett wrote in a letter to Max Jammer in 1973:

> I was somewhat surprised, and a little amused, that none of these physicists had grasped one of what I considered to be the major accomplishment of the theory – the "rigorous" deduction of the probability interpretation of quantum mechanics from wave mechanics alone. [...] The unwillingness of most physicists to accept this theory, I believe, is therefore due to the psychological distaste which the theory engenders overwhelming the inherent simplicity of the theory as a way of resolving the apparent paradoxes of quantum mechanics as conventionally conceived. Thus, the theory was not so much criticized, as far as I am aware, but simply dismissed. (Everett, 1973)

As stated before, Everett was rather vague about certain aspects of his interpretation of quantum mechanics – but then, so was Bohr in his articles on complementarity in the '20s. The new views Everett advocated were highly stimulating and interesting, and shed new light on the most important philosophical difficulties of the orthodox interpretation. Even though Everett's work was properly spread through the physics community, it seems that the time was simply not right for his ideas to be taken seriously.

4.2.3 Bell

Bell submitted his now celebrated article to the rather new and obscure journal *Physics*[19] to avoid the publication fees of more established journals like *Physical Review* – he was too shy to ask his host university to pay for his unusual submission. The paper initially attracted no attention whatsoever: in the first four years, not a single citation to the article was made. Only in the late

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[19] The full name was *Physics Physique Fizika*; it lasted for only four years.

seventies the first few citations were made; by 1980 the total citation count was a respectable 160. Today the paper is in the top 0.01% of all physics papers ever published.

Bell was a strong supporter of De Broglie-Bohm theory (Bell, 1982). This makes it quite ironic that Bell's theorem has been misunderstood to disprove the possibility of the existence of hidden variables in general. In 1976, Eugene Wigner wrote:

> The proof he [von Neumann] published [...], though it was made much more convincing later on by Kochen and Specker, still uses assumptions which, in my opinion, can quite reasonably be questioned. [...] In my opinion, the most convincing argument against the theory of hidden variables was presented by J. S. Bell. (Wigner, 1976)

4.3 American cold war policies

In the so-called 'second' Forman thesis (Forman, 1987), Paul Forman argues that military funding was the main catalyst for the development of physics research from 1940 to 1960. The military influence was largely responsible for a drastic increase in the number of scientists. However, as the physicist Merle Tuve said in 1959:

> Regardless of the doubling and redoubling year by year of the announced annual expenditures [...] these large sums seem to contribute so little to the really basic core of scholarly accomplishment [...] I feel that we have directed most of our efforts toward the creation and support of large-scale activities essentially technological in character.(Forman, 1987, p. 219)

So not only did the scale of research change, its nature was also altered.

Where the number of awarded doctorates in physics and engineering in the US was lower than 5,000 per year in and before the early 40's, by the late sixties this number was about 35,000 (Thurgood et al., 2006). It is not a surprise that university teachers dropped the personal style and foundational approach to teaching, bearing in mind that it was now their job to train large numbers of physicists, most of whom would end up in applied industrial and government labs. Similarly, US textbooks focused on physics of problems rather than physics of principles (Kaiser, 2007).

We also observe an increase in the number of scientists at conferences. In a report about the 1962 meeting on the foundations of quantum mechanics, at which Aharonov, Dirac, Furry, Podolsky and Rosen were present (and, incidentally, where Everett presented his relative-state formalism), it is argued that this increased conference size hindered philosophical discourse.

> Years ago, when the number of physicists at a meeting was so small that all could easily fit into a single room, the spirit of free discussion

> so vital for the progress of physics was characteristic of most conferences. Today, with the large meetings attended by hundreds of people and with many sessions going on simultaneously, it is difficult to create an atmosphere conductive to free and thorough discussion. (Werner, 1964)

Following these developments, the number of publications in physics journals was unprecedentedly high. Goudsmit wrote in an editorial, a year before his humorous derogation of theoretical physicists:

> In 1925 the Zeitschrift für Physik published 367 articles by 285 authors. In 1963 The Physical Review printed some 1600 papers by about 2500 different authors. The average length of the articles has more than doubled. [...] We believe that these numbers prove that the pursuit of physics has changed drastically in the last few decades, (Goudsmit, 1964)

As a result of this, major physics journals had to reject most of the submitted papers, and in the selection process it was common to lean more towards technical and experimental papers than towards discussions on theoretical or philosophical issues.

4.4 Numerical problem solving

Before the war, the word "computer" meant a *person* who performed calculations on a desk calculator. The development of the digital computer that would radically change this notion started in the mid '30s, marked according to most computer historians by a 1936 paper of Alan Turing. The first reliable machines appeared during the war. In America, the general purpose ENIAC was produced in 1945, which was versatile enough to run a large range of programs. It was rapidly followed by other computers such as the Whirlwind, MANIAC, and the Ferranti Mark 1, all of which were also used for numerical calculations by scientists.

This rapid progress makes sense when one looks at the events in Los Alamos in the early '40s, that were about to change not only the political but also the scientific world. Scientists working on the atomic bomb found themselves in need for sheer calculational power. Their calculations involved phenomena with extremely high densities, pressures and temperatures. There were no analytic techniques for the nonlinear mathematics these calculations required. The only way to get results was through a computational approach, that was known to be lengthy and cumbersome (Metropolis and Nelson, 1982).

The electronic computer was simultaneously developed in Germany[20], the UK[21] and the US. At the time, the US was the only country close to producing

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[20] The German Z3 was one of the first electronic computers. It was built by Konrad Zuse in 1941 in complete intellectual isolation. Government funding for further development was denied because it was considered to be not strategically important. The machine was destroyed in the war.

[21] The British used their Colossus machines to break German ciphers.

an atomic bomb and therefore needing a lot of computational power for their scientific research. Impressed by several developments – most notably the ENIAC (designed to speed up calculations for the United States Army's Ballistic Research Laboratory (Rojas and Hashagen, 2000)) and the Harvard Mark I (built for the U.S. Navy Bureau of Ships) – Von Neumann[22] brought the electronic computer to Los Alamos in early 1945:

Besides the famous and well documented ENIAC case, similar machines were developed during and after the war in other centers such as Harvard, MIT and Manchester. As many notable physicists worked with computers during the war and saw their huge potential for science, the new possibilities of electronic computing quickly became apparent in the scientific society.

As a consequence, physicists who were not working on anything bomb related started asking for computer time. So finally in 1951 the first commercially available Ferranti Mark 1 electronic computers appeared. After this, the number of publications using numerical methods on electronic computers steadily increased.

The way that computers were used in science changed from a way to simply do calculations, to a way of actually producing new knowledge. An early example: in a well known 1955 paper, Fermi, Pasta and Ulam (Fermi et al., 1965) (FPU) used numerical iteration techniques to study a nonlinear system on the Maniac I. This particular problem was not an existing problem that needed an answer – it was a problem inspired by the computer. Instead of only using it as a tool, the authors tried to explore its epistemological possibilities.

> He [Fermi] held many discussions with me [Ulam] on the kind of future problems which could be studied through the use of such machines. [. . .] Fermi expressed often a belief that future fundamental theories in physics may involve non-linear operators and equations, and that it would be useful to attempt practice in the mathematics needed for the understanding of non-linear systems. The plan was then to start with the possibly simplest such physical model and to study the results of the calculation of its long-time behaviour. Then one could gradually increase the generality and the complexity of the problem calculated on the machine. (Fermi et al., 1965)

The FPU paper yielded surprising results: the time evolution of the nonlinear system showed an unexpected periodicity and seemed to be greatly dependent on starting conditions. Fermi found these results quite interesting, and other scientists seemed to be impressed too:

> I [Ulam] presented the results of the original paper on several occasions at scientific meetings; they seemed to have aroused considerable interest among mathematicians and physicists. (Fermi et al., 1965)

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[22] Von Neumann became highly involved with the development of computers and proposed the Von Neumann architecture that is still used in computers today.

And thus, the use of a computer could result in, as Fermi put it, “little discoveries”. Fermi, Pasta and Ulam had caught a first glimpse of a new field for which the electronic computer was of key importance: chaos theory. In the ’60s Edward Lorenz pioneered it after famously accidentally discovering chaos in a weather prediction simulation on a simple digital computer (Lorenz, 1963).

The electronic computer gave scientists the possibility to effectively use numerical methods. These methods proved to be very fruitful during and shortly after the war for a large range of problems. An increasing number of scientists started using these new methods, developing a more pragmatic way of advancing science. These new methods proved to be a successful way of making new discoveries, and this success can explain – at least partly – why physicists were drawn away from foundational matters. Where physicists tried to advance science by looking at foundations in the ’20s and ’30s, now they used a more technical approach.

4.5 Quantum generations

We have seen that we can characterize the leading scientists in quantum theory of the ’20s and ’30s as philosopher-physicists (see section 1.7.2). From the ’40s to the ’60s this first generation of quantum physicists was still very much authoritative in the physics community. However, they had a very conservative attitude: they had invested decades in the development of the Copenhagen ideas, which now formed their basis for physics. And thus novel interpretations were met with resistance.

This we can see in reactions from the Copenhagen school against the new interpretations (Hiley and Peat, 1987, p. 7) (Freire, 2005). Bohr said about Bohm’s theory: ‘We may hope that it will later turn out that sometimes $2+2 = 5$.’ Born proclaimed that ‘Pauli has come up with an idea that slays Bohm,’ but the best counter-argument that Pauli gave was that ‘Bohm is metaphysical’ – even though three decades earlier he had accused Heisenberg of being not philosophical enough! Rosenfeld wrote about Everett:

> With regard to Everett neither I nor even Niels Bohr could have any patience with him, when he visited us in Copenhagen more than 12 years ago in order to sell the hopelessly wrong ideas he had been encouraged, most unwisely, by Wheeler to develop. He was undescribably stupid and could not understand the simplest things in quantum mechanics. (Rosenfeld, 1972)

About the postwar generation of physicists, Schweber writes:

> All the young theoreticians at the wartime laboratories learned that physics is about numbers and about the results of experiments. Good theories yield numbers, explain numbers, and help design good apparatus. [...] Here it was the pragmatic, utalitarian outlook – which had been reinforced by the wartime experiences – that gave the philosophical and ideological underpinning. (Schweber, 1994, Ch. 3)

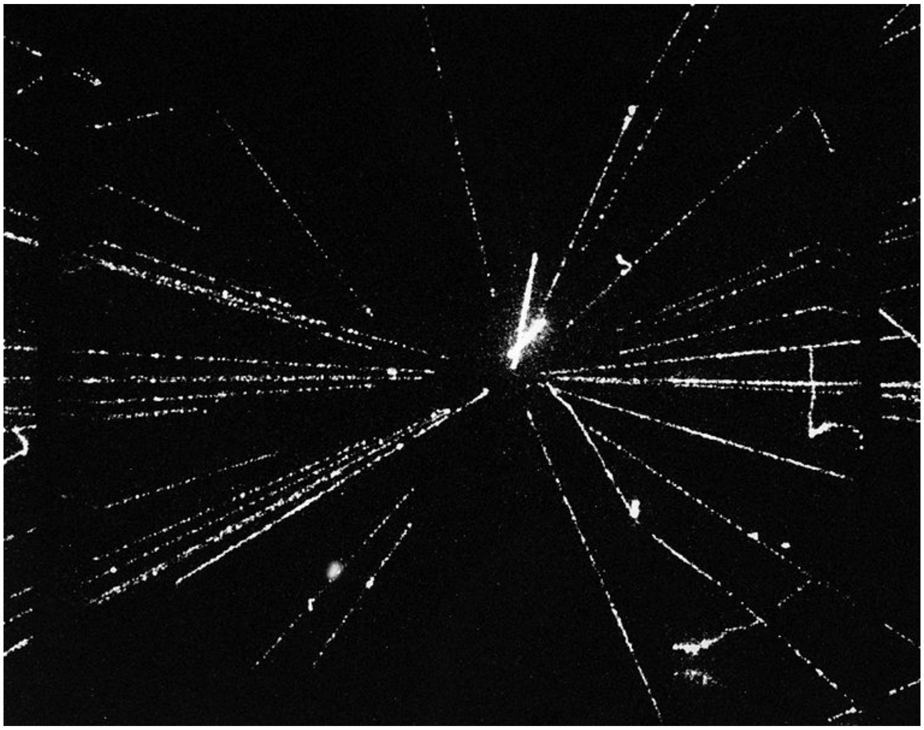
We have seen earlier that American universities tended to integrate theoreticians and experimentalists under one roof, and produce theoreticians who were involved with the analysis of experiments, rather than the analysis of foundations. The wartime successes of number crunching and experimental physics at Los Alamos and other laboratories such as the MIT Rad Lab showed the results of doing physics the American way. After the war, the new generation of physicists brought these wartime experiences with them to the academic world. They had been tought to pay less attention to foundations – they had been trained to "shut up and calculate". Hence, a pragmatic, less metaphysical form of the Copenhagen interpretation became their "working philosophy", and its conceptual issues were seen as unimportant.

This rather sharp distinction between generations has been made before:

> The second generation, those who were students of the founding fathers in the early postrevolutionary period, seem firmly – at times even ferociously – committed to the position that there is really nothing peculiar about the quantum world at all. Far from making *bons mots* about dizziness, or the opposite of deep truths being deep truths, they appear to go out of their way to make quantum mechanics sound as boringly ordinary as possible. (Mermin, 1989)

Until a third generation came about in the seventies and eighties, those who were foundationally inclined were stuck with the conservative founding fathers and their pragmatic pupils.

5 Quantum field theory



A proton-antiproton interaction at 540 GeV, showing particle tracks in a streamer chamber. Photo taken in 1982 at CERN's Super Proton Synchrotron. (Wikimedia Commons)

Quantum field theory (QFT) is a framework for relativistic quantum theories. In the first section of this chapter I outline its early development. In the second section I discuss how the introduction of renormalisation, a crucial process for the further development of QED, was facilitated by the postwar way of thinking. Then I discuss difficulties with the possible ontologies for the QFT framework. Finally, I investigate whether De Broglie-Bohm theory and the many-worlds interpretation are still viable in QFT.

5.1 Birth of the formalism

If an electron in a hydrogen atom jumps from one state to another, it either emits or absorbs a photon. To describe this class of phenomena, Paul Dirac introduced (Dirac, 1927b) quantum electrodynamics (QED): a description of the interaction between matter and light. By using occupation numbers as a basis (a procedure called *second quantization*) he gave a mechanism for particle creation and annihilation.

To index the degrees of freedom in a quantum system with multiple identical particle-states $|\phi_1\rangle, |\phi_2\rangle, \ldots$, we can switch to a description in terms of occupation numbers as follows:

$$|N_1, N_2, \ldots\rangle, \tag{105}$$

where N_j gives the number of particles in the state $|\phi_j\rangle$. The space in which these states live is called Fock space. Now we can define creation and annihilation operators. In the case of bosons:

$$\hat{a}_j |N_1, \ldots, N_j, \ldots\rangle = \sqrt{N_j}\, |N_1, \ldots, N_j - 1, \ldots\rangle, \tag{106}$$

$$\hat{a}_j^\dagger |N_1, \ldots, N_j, \ldots\rangle = \sqrt{N_j + 1}\, |N_1, \ldots, N_j + 1, \ldots\rangle. \tag{107}$$

As their name suggests, these operators allow us to describe the creation and annihilation of particles. They obey the commutation relations

$$[\hat{a}_i, \hat{a}_j^\dagger] = \delta_{ij}\mathbb{I}, \tag{108}$$

$$[\hat{a}_i, \hat{a}_j] = [\hat{a}_i^\dagger, \hat{a}_j^\dagger] = 0. \tag{109}$$

For the fermionic case, replace the commutators by anti-commutators.

One year later, he found the Dirac equation (see appendix 7.2), making possible a relativistic treatment of QED. This was a necessary step: classical electrodynamics is relativistically invariant and therefore incompatible with nonrelativistic quantum mechanics.

The wave-particle duality – not so much as a physical phenomenon, but as a distinction between two traditions in theoretical approaches – persisted (Schweber, 1994, introduction)(Pais, 1986, Ch. 15).

The most prominent physicist on the particle side was Dirac, with his electron-hole theory. The Dirac equation called for an interpretation of negative energy states. Dirac himself proposed the Dirac sea in 1929 (Dirac, 1930): in a vacuum all the negative energy states are filled. The particle corresponding to one of the negative energy states *not* being filled is called a hole or positron. Electron-positron pair creation is simply a jump from a negative energy state to a positive energy state. The existence of the positron was experimentally verified in 1932 by Carl Anderson. We can do this for the Dirac equation because it is a fermionic equation, so we can use the Pauli exclusion principle to fill up states. This interpretation does not hold for the bosonic Klein-Gordon equation, which also has negative energy states (see appendix 7.2).

Heisenberg and Pauli (Heisenberg and Pauli, 1929), however, advocated an approach in which fields and waves were the fundamental entities. Following Pascual Jordan's suggestion that second quantization was the quantization of a *field*, they constructed a continuous extension of the Lagrangian method in classical mechanics. In the classical Lagrangian formalism, we use a generalized coordinate $\mathbf{q}$ to define the Lagrangian

$$L(\mathbf{q}, \dot{\mathbf{q}}, t) = T - V, \tag{110}$$

where T is the kinetic energy and V the potential energy. Hamilton's variation principle leads us to the Lagrange equations

$$\frac{d}{dt}\left(\frac{\partial L}{\partial \dot{\mathbf{q}}}\right) = \frac{\partial L}{\partial \mathbf{q}}. \tag{111}$$

We can switch to *field* theory by replacing the generalized coordinates by a continuous generalized coordinate $\phi(x)$, where x is a spacetime four-vector. We replace the Lagrangian L by a Lagrangian density

$$\mathcal{L}(\phi(x), \partial_\mu \phi(x)). \tag{112}$$

Here, $\partial_\mu = \frac{\partial}{\partial x^\mu}$ and $\mu = 0, 1, 2, 3$, where (x^0, x^1, x^2, x^3) correspond to (ct, x, y, z). From now on I shall also be using the standard Einstein summation convention, where $\sum_{\mu,\nu} g_{\mu\nu} x^\mu x^\nu$ is reduced to $x^\mu x_\mu$, using $g_{\mu\nu} = diag(1, -1, -1, -1)$ as metric. Again, using the variation principle, we find the Euler-Lagrange equation:

$$\partial_\mu \left(\frac{\partial \mathcal{L}}{\partial(\partial_\mu \phi)}\right) = \frac{\partial \mathcal{L}}{\partial \phi}. \tag{113}$$

This equation holds for any field ϕ_a we introduce into our theory. The conjugate field is defined as

$$\pi_a = \frac{\partial \mathcal{L}}{\partial \dot{\phi}_a}. \tag{114}$$

Now the coordinates and conjugate momenta become operators that satisfy canonical commutation relations, to quantize the theory. In the Schrödinger picture, this reads for bosonic fields:

$$\left[\hat{\phi}_j(\mathbf{x}), \hat{\pi}_k(\mathbf{y})\right] = i\hbar \delta_{jk} \delta(\mathbf{x} - \mathbf{y}) \hat{1}, \tag{115}$$

$$\left[\hat{\phi}_j(\mathbf{x}), \hat{\phi}_k(\mathbf{y})\right] = \left[\hat{\pi}_j(\mathbf{x}), \hat{\pi}_k(\mathbf{y})\right] = 0. \tag{116}$$

Again, for fermionic fields, we replace the commutators with anti-commutators.

The occupation number and field procedures give us different but equivalent ways of performing second quantization. Eventually, the field operator is the Fourier transform of creation and annihilation operators. For example, in the free Klein-Gordon case

$$\mathcal{L} = \frac{1}{2}\hbar^2 c^2 (\partial_\mu \phi)(\partial^\mu \phi) - \frac{1}{2} m^2 c^4 \phi^2, \tag{117}$$

the Euler-Lagrange equation is the Klein-Gordon equation

$$(\Box + \frac{m^2c^2}{\hbar^2})\phi(x) = 0. \tag{118}$$

The most general solution to this equation is a superposition of harmonic oscillators. To quantize our theory, we can simply quantize the oscillators in terms of raising and lowering operators labeled by the momentum $\mathbf{p}$:

$$\hat{\phi}(\mathbf{x}) = \int \frac{d\mathbf{p}}{(2\pi)^3} \frac{\hat{a}_{\mathbf{p}} + \hat{a}^{\dagger}_{-\mathbf{p}}}{\sqrt{2\omega_{\mathbf{p}}}} e^{i\mathbf{p}\cdot\mathbf{x}} \tag{119}$$

$$\hat{\pi}(\mathbf{x}) = -i \int \frac{\omega_{\mathbf{p}} d\mathbf{p}}{(2\pi)^3} \frac{\hat{a}_{\mathbf{p}} - \hat{a}^{\dagger}_{-\mathbf{p}}}{\sqrt{2\omega_{\mathbf{p}}}} e^{i\mathbf{p}\cdot\mathbf{x}}, \tag{120}$$

where $\omega_{\mathbf{p}} = \sqrt{\mathbf{p}^2 + m^2}$ is the frequency, and with the commutation relations

$$[\hat{a}_{\mathbf{p}}, \hat{a}^{\dagger}_{\mathbf{p}'}] = (2\pi)^3 \delta(\mathbf{p} - \mathbf{p}')\hat{1}, \tag{121}$$

$$[\hat{a}_{\mathbf{p}}, \hat{a}_{\mathbf{p}'}] = [\hat{a}^{\dagger}_{\mathbf{p}}, \hat{a}^{\dagger}_{\mathbf{p}'}] = 0. \tag{122}$$

This yields the correct commutation relations for $\hat{\phi}$ and $\hat{\pi}$. We can now see the raising and lowering operators as creation and annihilation operators, if we interpret the associated energy quanta as particles.

Finally, to add interactions, we simply add an interaction term to the Lagrangian, rendering the Euler-Lagrange equation nonlinear:

$$\mathcal{L} = \frac{1}{2}\hbar^2 c^2 (\partial_\mu \phi)(\partial^\mu \phi) - \frac{1}{2} m^2 c^4 \phi + \mathcal{L}_{int}. \tag{123}$$

5.2 Renormalisation

In order to make predictions, QED must be treated as a perturbation theory. In the early days, lowest-order calculations were quite successful, but both the particle and field approaches had major difficulties with divergences in higher-order calculations. These so-called UV divergences appear in integrals that take arbitrarily high momenta (or short distances) into account.

Very little progress in solving these problems was made by physicists in the '30s. The cut-off method, in which high momentum states are just left out of the integral, was introduced, but was seen as deeply problematic. At this stage, physicists emphasised that the theory of QED should provide a deeper understanding of the quantum world and accomplish unification of quantum mechanics and relativity theory. In other words, there was an emphasis on theoretical success, instead of on predictive ability. Physicists expected that only further conceptual revolutions would solve their divergence problems. Heisenberg, for example, wrote:

> It does not seem likely that there could be a consistent quantum theory of waves that precisely does not include the domain of large energy and momentum transfers and that does not determine the ratio of the rest masses of the particles. (Rueger, 1992)

Even though the necessary tools were already there in the '30s, the difficulties were only resolved in the late '40s with a technical and conservative method called renormalisation, by a new generation of quantum physicists, most notably Schwinger, Feynman, Dyson and Tomonaga. These physicists had an interest in empirical results, as opposed to an ambition for a perfect unified theory. For example, in 1948 the British physicist Rudolph Peierls, who had taken part in the Manhattan project, said:

> But at any rate a satisfactory theory of this self-energy, if it is possible, would not represent the last word and one is, therefore, more inclined to accept formalisms which will contain arbitrary parameters or arbitrary functions which at this stage do not come out of the theory but have to be taken from experiment. (Rueger, 1992)

In other words: at that moment, the ambition for a unified theory without infinities that determines physical parameters was unrealistic. So practically speaking it made sense to stick to the formalism they had and try to extract as much information from it as possible. The postwar generation tried to show that the dependence on high momentum cut-offs could be eliminated, by expressing results in experimentally observable quantities. This approach proved to be highly successful in further developing the field. Even though the procedure still seemed magical and unfounded to many physicists, it was an accepted practice.

The conclusion that cultural influences have made possible the success of the renormalisation method is shared between Schweber (Schweber, 1986) and Rueger (Rueger, 1992). Schweber gives a broad overview of the pragmatic tradition in the US, and the way that this attitude in particle physics was reinforced by the wartime experiences. Rueger has a more internalist approach, and shows that the mathematical methods were already there before the war, but these methods were not generally seen as an acceptible way to improve understanding. Together, these articles make a compelling case.

QED became the model theory for the development of other QFTs. With the technological advances of the '50s, it was possible to observe more and more new particles called hadrons in collider experiments. In the '60s it became clear that hadrons consisted of quarks, and quantum chromodynamics (QCD) was born. Where in QED the photon mediates the electromagnetic force between matter particles with electric charge, in QCD the gluons transmit the strong interaction between matter particles that carry colour, called quarks. The third force of the standard model, called the weak interaction, is carried by the Z and W bosons and affects all fermions. It can be described in a field theory called quantum flavourdynamics (QFD), but is in practice best understood in the electro-weak theory (EWT). These developments eventually led to the introduction of the standard model in the '70s, which describes the electromagnetic, weak and strong forces. The recent observation of the Higgs boson, which the theory predicted, was a great experimental verification of the theory.

The modern understanding of renormalisation was born in the '70s, when Kenneth Wilson published his renormalisation group insights. He investigated how theories should be ajusted to retain their predictions for low energies, when one varies the cut-off. As a result, renormalisation became a theoretical tool for studying the behaviour of QFTs in different energy regimes, and renormalised theories are now seen as effective field theories.

5.3 Ontological issues

A particle theory describes the properties of individual entities, whereas a field theory describes properties of space-time points. We have seen that QFT gives us a unified framework for describing both fields and particles. The question remains as to whether particles or fields are the fundamental entities of QFT.

In fact, Weinberg (Weinberg, 1986) has said that we can characterise the history of particle physics as a "story of oscillation" between these two viewpoints. However, these viewpoints must be seen as supporting a strategy (either S-matrix theory (particles) or QFT (fields)) for the best way to make future progress for predictions, and not as a philosophical discussion on ontology. This philosophical discussion has only surfaced in more recent years.

In practice, QFT is used in particle physics to describe high energy collisions of particles. In this context it seems natural to think of QFT as a theory about particles. We have seen that the quanta in Fock space, in the free theory at least, seem a good justification for this approach. However, defining what actually constitutes a particle in interacting theories is problematic. In QED, for example, an electron is never alone: it is always "decorated" by a swarm of photons and electron-positron pairs.

More formal arguments against a particle interpretation originate from a field called *Algebraic Quantum Field Theory*, in which QFT is formulated in terms of algebras of local observables. The Haag, Reeh-Schlieder, Malament and Hegerfeldt theorems are generally interpreted to mean that particles cannot be properly counted and localized (Lupher, 2010).

Haag's theorem, for example, states that interacting theories are not unitarily equivalent to a Fock space representation for a free field. The theorem was introduced in 1955, and did not get the attention some thought it deserved, as was the case with earlier discussed interpretations and no-go theorems in the '50s and '60s:

> There is a wide spread opinion that the phenomena associated with Haag's Theorem are somehow pathological and irrelevant for real physics. In this section I make one more attempt to explain why that is not the case. (Wightman, 1967)

Instead we could take fields as the fundamental ontological entities in QFT. However, fields are operator-valued, so it is not clear in what way these fields give physical properties to space-time. There is some recent work on this problem.

Teller argues that we need states, as well as the field operator, to obtain a field configuration in terms of the expectation value $\langle\psi|\hat{\phi}(x)|\psi\rangle$ (Teller, 1995). The most popular proposal is called the wavefunctional interpretation, in which QFT states are represented as superpositions of classical fields. However, Baker argues that this interpretation rests again on a representation in Fock space, and suffers from the same no-go theorems as the particle interpretation (Baker, 2009).

As a consequence other ontologies such as ontic structural realism, in which underlying mathematical structures (in the case of QFT, these are group structures) are seen as real, have been proposed.

5.4 Many-worlds in QFT

The many-worlds interpretation is applicable to all quantum theories with states in Hilbert space and a linear wave equation, and works in exactly the same way as explained in the nonrelativistic case. This goes well for asymptotic free states, which are the states that we generally observe in particle detectors. In interacting field theories the Euler-Lagrange equation is strictly speaking nonlinear, but during the interaction we cannot speak of any relative states between observer and quantum system, as we cannot interfere on the scale of the process itself.

5.5 Bell-type QFTs: an extension of De Broglie-Bohm theory

Another way of finding an ontology for QFT, would be to find a suitable relativistic extension of De Broglie-Bohm theory in terms of particle trajectories. Moreover, it would counter a common protest against De Broglie-Bohm theory, namely that it cannot be extended to a relativistic theory.

However, in 1984 Bell published an article (Bell, 1984) in which he tried to do just that. He made a relativistic quantum theory in which he replaced the space-continuum with a lattice and implemented a stochastic creation-annihilation mechanism. This approach has some serious problems. Firstly, the theory is not Lorentz invariant, as it treats space and time on entirely different footing. Secondly, it introduces a stochastic element into quantum mechanics:

> The introduction of a stochastic element, for beables with discrete spectra, is unwelcome, for the reversibility of the Schrodinger equation strongly suggests that quantum mechanics is not fundamentally stochastic in nature. However I suspect that the stochastic element introduced here goes away in some sense in the continuum limit.

Also, one of the perks of De Broglie-Bohm theory was its determinism, which is now dropped.

More recently, Dürr et al. introduced Bell-type quantum field theories in a continuum (Dürr et al., 2004). In this theory, the particle world lines consist of pieces of Bohmian trajectories, that can begin and end. The resulting diagrams look like Feynman diagrams, but they are not merely a calculational tool: they describe actual particle paths. The state of a system is described by a vector in Fock space Ψ_t and a particle configuration Q_t, living in the configuration space of possible positions for a variable number of particles $\mathcal{Q}$. The continuous motion is described by the guiding equation

$$\frac{dQ_t}{dt} = Re \frac{\Psi_t^*(Q_t) \left([H_0, \hat{q}] \, \Psi_t \right) (Q_t)}{\Psi_t^*(Q_t) \Psi_t(Q_t)}, \tag{124}$$

where H_0 is the free hamiltonian and $\hat{q}$ is the Heisenberg position operator. The probability of jumping to a different particle configuration volume dq in $\mathcal{Q}$ during the time interval dt is given by

$$\sigma^{\Psi}(dq|Q_t)dt = \frac{2}{\hbar} \frac{\max(\mathrm{Im} \Psi^*(q) \, \langle q|H_I|Q_t \rangle \, \Psi(Q_t), 0)}{\Psi_t^*(Q_t) \Psi_t(Q_t)} dq dt, \tag{125}$$

where H_I is the interaction Hamiltonian and Q_t the present particle configuration. This defines a Markov process on $\mathcal{Q}$.

Contrary to what Bell believed, this theory still has the same problems as its lattice predecessor. It is not Lorentz invariant and particle jumps are stochastic. The authors write:

> But we note that though the theories we present here require a preferred reference frame, there can be no experiment that would allow an observer to determine which frame is the preferred one, provided the corresponding QFTs are such that their empirical predictions are Lorentz invariant. (Dürr et al., 2004)

In other words, to retain the safe Bohmian ontology, we have to drop even more ideological properties, namely determinism and Lorentz invariance.

Moreover, the theory has been successfully applied to relativistic fermions, but does not work yet for bosons. Other pilot-wave approaches to QFT exist, with both field and particle ontologies (for an overview, see (Struyve, 2011)), but none of these theories have yet acquired maturity. In other words, no satisfactory relativistic Bohmian approach exists yet.

5.6 Conclusions

For the relativistic case, we can expand upon our analysis in the following way, where we must keep in mind that it is still unclear whether the relativistic extension of the De Broglie-Bohm theory is actually viable or not.

5.6.1 Ontology

In the standard interpretation of QFT, it is not clear as to whether particles or fields are fundamental. Most philosophers and physicists lean towards fields, but it is not clear how exactly these fields can be considered physical. Verdict: unresolved.

The relativistic extension of the De Broglie-Bohm theory describes particles with a well-defined position and momentum. Verdict: safe.

As before, in the many-worlds interpretation, each decohered branch of the wave function stands for a seperate world. Verdict: unsafe.

5.6.2 Ideology

To our list of ideological statements we can now add Lorentz invariance, to get the following table:

	determinism	locality	minimalism	noncontextuality	Lorentz inv.
CI	no	no	yes	yes	yes
DBB	no	no	no	no	no
MW	yes	yes	yes	yes	yes

Table 3: The interpretations and their ideologies.

6 Conclusion

I have tried to answer the question: to what extent have social and cultural influences affected the interpretive debate in quantum mechanics between the '20s and the '60s?

In the first decade after the quantum revolution in the mid '20s, the foundational discussion was a part of mainstream science. A large variety of interpretations and philosophical issues were discussed in mainstream scientific circles. The Copenhagen view, in which an unsafe ontology and ideology were accepted, became the most popular school of thought, even though it had serious conceptual issues. The popularity of the Copenhagen ideas can be explained by the simple fact that it was the first complete interpretation of quantum mechanics, by the established authority of Bohr, and by the intellectual milieu at the time.

The political tensions that started in the mid '30s changed the scientific community significantly. Many European physicists left their country; most of them fled to the US, which was rapidly becoming the center of the scientific community. They played a major role in the war effort and in the '40s the US government started investing heavily in scientific research.

In the '50s two new viable interpretations saw the light of day, followed by two important "no go" theorems in the '60s. Where the Copenhagen interpretation had major conceptual difficulties regarding universality and measurements, the new interpretations did not. De Broglie-Bohm mechanics gives us an ontologically safe approach, and many-worlds gives us an interpretation with ideologically adequate properties. The no-go theorems made clear that it is not possible to create an interpretation of quantum mechanics that fully conforms to the realist's dream. However, the new foundational work did show that it was possible to formulate viable alternative interpretations, that are ontologically or ideologically more appealing than the Copenhagen interpretation and have answers to its main conceptual issues.

In contrast to the prewar attitude, philosophical discourse was not so common any more in the physics community throughout the '40s, '50s and '60s. There were a number of reasons for this attitude change. American universities became the largest centers for research and had a more pragmatic tradition than their European counterparts. The cold war was responsible for further increasing the focus on applied physics, and larger classes and conferences. Moreover, new pragmatic and numerical methods proved to be very successful and replaced a more analytic and foundational way of thinking. Finally, new interpretive ideas were met with resistance by the authorative first generation of quantum physicists. As a consequence, the Copenhagen ideas dominated and the new interpretations remained obscure.

The more pragmatic approach to physics is reflected in the success of the renormalisation program. In QFT, the main interpretive issue is the ontology of the theory, but this philosophical discussion has only emerged in recent years. The many-worlds interpretation is trivially applicable to QFT, but it is not yet clear whether a Bohmian approach is viable.

7 Appendices

7.1 The generalized uncertainty principle

We have, for Hermitian operators A and B:

$$\sigma_A^2 = \langle (A - \langle A \rangle)^2 \rangle = \langle \psi | (A - \langle A \rangle)(A - \langle A \rangle) | \psi \rangle = \langle f | f \rangle . \tag{126}$$

Similarly

$$\sigma_B^2 = \langle g | g \rangle , \quad | g \rangle = (B - \langle B \rangle) | \psi \rangle . \tag{127}$$

Now

$$\sigma_A^2 \sigma_B^2 = \langle f | f \rangle \langle g | g \rangle \geq | \langle f | g \rangle |^2 \geq \left(\frac{1}{2i} [\langle f | g \rangle - \langle g | f \rangle] \right)^2 , \tag{128}$$

using the Schwartz inequality and the fact that for a complex number $z = \langle f | g \rangle$ we have $|z|^2 \geq |\mathrm{Im}(z)|^2 = \left(\frac{1}{2i} [z - \bar{z}] \right)^2$.

Using the definitions of $|f\rangle$ and $|g\rangle$, we find that $\langle f|g\rangle - \langle g|f\rangle$ gives us

$$\langle f | g \rangle - \langle g | f \rangle = \langle AB \rangle - \langle BA \rangle = \langle [A, B] \rangle , \tag{129}$$

so that we end up with the generalized uncertainty principle

$$\boxed{\sigma_A^2 \sigma_B^2 \geq \left(\frac{1}{2i} \langle [A, B] \rangle \right)^2 .} \tag{130}$$

For x and p we have

$$[x, p] = i\hbar, \tag{131}$$

so that

$$\sigma_x^2 \sigma_p^2 \geq \left(\frac{\hbar}{2} \right)^2 . \tag{132}$$

Because standard deviations are positive that means

$$\sigma_x \sigma_p \geq \frac{\hbar}{2}. \tag{133}$$

Similarly, for $E = i\hbar \frac{\partial}{\partial t}$ and t we find

$$[E, t] = i\hbar, \tag{134}$$

so that

$$\sigma_E \sigma_t \geq \frac{\hbar}{2}. \tag{135}$$

7.2 A relativistic wave equation

The Schrödinger equation for a free particle

$$-\frac{\hbar^2}{2m}\nabla^2\psi = i\hbar\frac{\partial}{\partial t}\psi, \tag{136}$$

is not compatible with relativity theory. Time and space are not treated equally: the combination of a first order time derivative and a second order space derivative makes the equation not Lorentz invariant.

Of course, we could start by simply plugging the quantum operators for energy and momentum

$$\hat{E} = i\hbar\frac{\partial}{\partial t}, \quad \hat{\mathbf{p}} = -i\hbar\boldsymbol{\nabla} \tag{137}$$

into the expression for the free energy of a relativistic particle

$$E = \sqrt{m^2c^4 + \mathbf{p}^2c^2}, \tag{138}$$

where m is the particle mass and c is the velocity of light. However, it would be necessary to expand this square root in an infinite series, which is highly cumbersome.

7.2.1 The Klein-Gordon equation

Oskar Klein and Walter Gordon proposed an alternative approach in 1926. They mistakenly claimed it described the spin-$\frac{1}{2}$ electron; it is now considered to be the correct relativistic field equation for a spin-0 particle.

We start by plugging the quantum operators for energy and momentum into the *squared* expression for the free energy of a relativistic particle

$$\frac{E^2}{c^2} - p^2 = m^2c^2. \tag{139}$$

This gives us

$$-\frac{\hbar^2}{c^2}\frac{\partial^2}{\partial t^2} + \hbar^2\boldsymbol{\nabla}^2 = m^2c^2, \tag{140}$$

so that we find the Klein-Gordon equation:

$$-\frac{\hbar^2}{c^2}\frac{\partial^2}{\partial t^2}\psi + \hbar^2\boldsymbol{\nabla}^2\psi = m^2c^2\psi. \tag{141}$$

Using the d'Alembert operator $\Box = \frac{1}{c^2}\frac{\partial^2}{\partial t^2} - \boldsymbol{\nabla}^2$ and $\mu = \frac{mc}{\hbar}$, this is

$$(\Box + \mu^2)\psi = 0. \tag{142}$$

This is neat, because the d'Alembert operator is a Lorentz scalar so this expression remains valid for any inertial frame. However, when we try to find plane wave solutions for this equation, we find corresponding energy values

$$E_\pm = \pm c\sqrt{m^2c^2 + \mathbf{p}^2}. \tag{143}$$

The energy spectrum does not seem to have a lower boundary!

7.2.2 The Dirac equation

Paul Dirac found a *first order* relativistic equation in 1928 (Dirac, 1928).

Besides the fact that the Klein-Gordon equation was problematic, Dirac was unsatisfied with another aspect of quantum mechanics: spin. The concept of electron spin was introduced in 1925 by Uhlenbeck and Goudsmit, two students of Paul Ehrenfest in Leiden. The Austrian physicist Wolfgang Pauli formulated the exclusion principle in that same year. He also developed a rather pragmatic formalism, known as the Pauli spin matrices, to incorporate spin in wave mechanics. Dirac wrote:

> The question remains as to why Nature should have chosen this particular model for the electron instead of being satisfied with the point-charge. (Dirac, 1928)

This might seem a peculiar question; for a theoretical physicist it is not.

His wave equation for spin-$\frac{1}{2}$ particles was fully consistent with the principles of both special relativity and quantum mechanics, and incorporated spin in a natural way:

> It appears that the simplest Hamiltonian for a point-charge electron satisfying the requirements of both relativity and the general transformation theory leads to an explanation of all duplexity phenomena without further assumption. (Dirac, 1928)

Upon investigating equation 140

$$-\frac{1}{c^2}\frac{\partial^2}{\partial t^2} + \boldsymbol{\nabla}^2 = \frac{m^2c^2}{\hbar^2}, \tag{144}$$

Dirac realized one could rewrite the left hand side as follows (using $\partial_x = \frac{\partial}{\partial x}$, and so on):

$$-\frac{1}{c^2}\partial_t^2 + \partial_x^2 + \partial_y^2 + \partial_z^2 = \left(\frac{i}{c}A_t\partial_t + A_x\partial_x + A_y\partial_y + A_z\partial_z\right)^2, \tag{145}$$

where, in order for all the terms $\partial_i\partial_j$ with $i \neq j$ to vanish

$$A_iA_j + A_jA_i = 0 \text{ for } i \neq j, \text{ where } i, j = t, x, y, z \tag{146}$$

and

$$A_i^2 = 1. \tag{147}$$

He realized that A_i have to be matrices and the wave function has to have multiple components:

$$\psi(x) = \begin{pmatrix} \psi_1(x) \\ \vdots \\ \psi_N(x) \end{pmatrix} \tag{148}$$

This explained spin as an internal degree of freedom that is a consequence of unifying quantum mechanics with relativity. It is now possible to write

$$\left(\frac{i}{c}A_t\partial_t + A_x\partial_x + A_y\partial_y + A_z\partial_z\right)\psi = \frac{mc}{\hbar}\psi. \tag{149}$$

If we take $A_t = \beta$ and $A_j = i\beta\alpha_j$ for $j = x, y, z$ we arrive at the Dirac equation as it usually appears:

$$(c\boldsymbol{\alpha}\cdot\hat{\mathbf{p}} + \beta mc^2)\psi = i\hbar\frac{\partial\psi}{\partial t}. \tag{150}$$

The usual representations of β and $\boldsymbol{\alpha}$ for the lowest value $N = 4$ are

$$\beta = \begin{pmatrix} I & \varnothing \\ \varnothing & -I \end{pmatrix}, \quad \boldsymbol{\alpha} = \begin{pmatrix} \varnothing & \boldsymbol{\sigma} \\ \boldsymbol{\sigma} & \varnothing \end{pmatrix}, \tag{151}$$

where I is the 2×2 unit matrix and $\boldsymbol{\sigma}$ are the 2×2 Pauli spin matrices

$$\sigma_x = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix}, \quad \sigma_y = \begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix}, \quad \sigma_z = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix}. \tag{152}$$

The Dirac equation can also be demonstrated to be relativistically invariant. However, upon finding a solution for the Dirac equation of a free particle in its inertial rest frame, we again find negative energy values

$$E_\pm = \pm mc^2. \tag{153}$$

This problem can be solved with two approaches. Dirac himself developed electron-hole theory; others used a quantum theory of fields to resolve the issues.

References

Aharonov, Y., Bergmann, P. G., and Lebowitz, J. L. (1964). Time symmetry in the quantum process of measurement. *Physical Review*, 134(6B):B1410.

Albert, D. and Loewer, B. (1988). Interpreting the many worlds interpretation. *Synthese*, 77(2):195–213.

Bacciagaluppi, G. and Valentini, A. (2009). *Quantum Theory at the Crossroads.* Cambridge University Press.

Baker, D. (2009). Against field interpretations of quantum field theory. *British Journal for the Philosophy of Science*, 60:585–609.

Bell, J. (1982). On the impossible pilot wave. *In: Speakable and Unspeakable in Quantum Mechanics, John Bell, Cambridge University Press, 1987.*

Bell, J. (1984). Beables for Quantum Field Theory. *Cern-TH.4035/84.*

Bell, J. S. (1966). On the problem of hidden variables in quantum mechanics. *Reviews of Modern Physics*, 38(3):447.

Bell, J. S. et al. (1964). On the einstein-podolsky-rosen paradox. *Physics*, 1(3):195–200.

Bohm, D. (1952). A suggested interpretation of the quantum theory in terms of" hidden" variables. i. *Physical Review*, 85(2):166.

Bohm, D. (1953). Proof that probability density approaches $|\psi|^2$ 2 in causal interpretation of the quantum theory. *Physical Review*, 89(2):458.

Bohm, D. and Hiley, B. (1982). The de broglie pilot wave theory and the further development of new insights arising out of it. *Foundations of Physics*, 12(10):1001–1016.

Bohr, N. (1913). I. on the constitution of atoms and molecules. *The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science*, 26(151):1–25.

Bohr, N. (1928). The quantum postulate and the recent development of atomic theory. *Nature*, 121:580–590.

Born, M. (1923). Quantentheorie und störungsrechnung. *Naturwissenschaften*, 11(27):537–542.

Born, M. (1926). Quantenmechanik der stoßvorgänge. *Zeitschrift für Physik*, 38(11-12):803–827.

Born, M. (1954). The statistical interpretation of quantum mechanics. *Nobel Lecture*, 11:1942–1962.

Born, M., Heisenberg, W., and Jordan, P. (1926). Zur quantenmechanik. ii. *Zeitschrift für Physik*, 35(8-9):557–615.

Born, M. and Jordan, P. (1925). Zur quantenmechanik. *Zeitschrift für Physik*, 34(1):858–888.

Breuer, H. and Petruccione, F. (2002). *The theory of open quantum systems.* Oxford University Press.

Cabello, A., Estebaranz, J., and García-Alcaine, G. (1996). Bell-kochen-specker theorem: A proof with 18 vectors. *Physics Letters A*, 212(4):183–187.

Cassidy, D. (1992). *Uncertainty: The Life and Science of Werner Heisenberg.* Freeman, New York.

Cushing, J. (1994). *Quantum Mechanics, Historical Contingency and the Copenhagen Hegemony.* The University of Chicago Press.

De Broglie, L. (1924). Recherches sur la théorie des quanta. *Annales de Physique*, 10.

De Broglie, L. (1927). La mécanique ondulatoire et la structure atomique de la matière et du rayonnement. *Journal de Physique et du Radium*, 8:255–241.

DeWitt, B. and Graham, N. (1973). *The Many-Worlds Interpretation of Quantum Mechanics.* Princeton University Press.

DeWitt, C. M. and Rickles, D. (2011). *The Role of Gravitation in Physics, Report from the 1957 Chapel Hill Conference.* epubli.

Dirac, P. A. (1927a). The physical interpretation of the quantum dynamics. *Proceedings of the Royal Society of London. Series A*, 113(765):621–641.

Dirac, P. A. (1927b). The quantum theory of the emission and absorption of radiation. *Proceedings of the Royal Society of London. Series A, Containing Papers of a Mathematical and Physical Character*, pages 243–265.

Dirac, P. A. (1928). The quantum theory of the electron. *Proceedings of the Royal Society of London. Series A, Containing Papers of a Mathematical and Physical Character*, pages 610–624.

Dirac, P. A. (1930). A theory of electrons and protons. *Proceedings of the Royal Society of London. Series A, Containing papers of a mathematical and physical character*, pages 360–365.

Dürr, D., Goldstein, S., Tumulka, R., and Zanghi, N. (2004). Bohmian Mechanics and Quantum Field Theory. *Phys. Rev. Lett.*, 93.

Dürr, D. and Teufel, S. (2009). *Bohmian Mechanics.* Springer.

Einstein, A. (1905). Über einen die erzeugung und verwandlung des lichtes betreffenden heuristischen gesichtspunkt. *Annalen der Physik*, 322(6):132–148.

Einstein, A. (1916). Zur quantentheorie der strahlung. *Physikalische Gesellschaft Zürich*, 18:47–62.

Einstein, A., Podolsky, B., and Rosen, N. (1935). Can quantum-mechanical description of physical reality be considered complete? *Physical review*, 47(10):777.

Everett, H. (1957). Hugh Everett letter to Norbert Wiener, 31-May-1957. *UCIspace*.

Everett, H. (1973). Hugh Everett letter to Max Jammer, 19-September-1973. *UCIspace*.

Everett III, H. (1957). "relative state" formulation of quantum mechanics. *Reviews of modern physics*, 29(3):454.

Fermi, E., Pasta, J., and Ulam, S. (1965). Studies of Non Linear Problems. *The University of Chicago Press*. Including "Introduction no. 266" by S. Ulam.

Fine, A. (1996). *The shaky game: Einstein, realism, and the quantum theory*, volume 2. Cambridge Univ Press.

Forman, P. (1971). Weimar culture, causality, and quantum theory, 1918-1927: Adaptation by german physicists and mathematicians to a hostile intellectual environment. *Historical studies in the physical sciences*, pages 1–115.

Forman, P. (1987). Behind quantum electronics: National security as basis for physical research in the united states, 1940-1960. *Historical Studies in the Physical and Biological Sciences*, pages 149–229.

Freedman, S. J. and Clauser, J. F. (1972). Experimental test of local hidden-variable theories. *Physical Review Letters*, 28(14):938.

Freire, O. (2005). Science and exile: David bohm, the cold war, and a new interpretation of quantum mechanics. *Historical Studies on the Physical and Biological Sciences*, 36:1–34.

Gell-Mann, M. and Hartle, J. (1990). Alternative Decohering Histories in Quantum Mechanics. *Proceedings of the 25th International Conference on High Energy Physics, Singapore*.

Goudsmit, S. (1964). Editorial. *Physical Review Letters*, 13(15).

Goudsmit, S. (1965). Editorial. *Physical Review Letters*, 15(13).

Hackermüller, L., Uttenthaler, S., Hornberger, K., Reiger, E., Brezger, B., Zeilinger, A., and Arndt, M. (2003). Wave nature of biomolecules and fluorofullerenes. *Physical Review Letters*, 91(9):090408.

Heisenberg, W. (1925). Über quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen. *Zeitschrift für Physik*, 33:879–893.

Heisenberg, W. (1927). Über den anschaulichen inhalt der quantentheoretischen kinematik und mechanik. *Zeitschrift für Physik*, 43(3-4):172–198.

Heisenberg, W. (1930). *The Physical Principles of the Quantum Theory*. Dover Publications.

Heisenberg, W. (1931). Die rolle der unbestimmtheitsrelationen in der modernen physik. *Monatshefte für Mathematik*, 38(1):365–372.

Heisenberg, W. (1958). *Physics and Philosophy, the Revolution in Modern Science*. Harper & Brothers Publishers.

Heisenberg, W. (1969). *Der Teil und das Ganze*. R. Piper & Co. Verlag.

Heisenberg, W. and Pauli, W. (1929). Zur quantendynamik der wellenfelder. *Zeitschrift für Physik A Hadrons and Nuclei*, 56(1):1–61.

Hiley, B. and Peat, F. D. (1987). *Quantum Implications: Essays in honour of David Bohm*. Routledge & Kegan Paul Ltd.

Howard, D. (2004). Who invented the copenhagen interpretation? a study in mythology. *Philosophy of Science*, 71(5):669–682.

Jammer, M. (1966). *The Conceptual Development of Quantum Mechanics*. McGraw Hill.

Jammer, M. (1974). *The Philosophy of Quantum Mechanics*. John Wiley & Sons.

Kaiser, D. (2007). Turning physicists into quantum mechanics. *Physics World (May 2007)*, pages 28–33.

Kennard, E. (1927). Zur quantenmechanik einfacher bewegungstypen. *Zeitschrift für Physik*, 44(4-5):326–352.

Kochen, S. and Specker, E. (1967). The problem of hidden variables in quantum mechanics. *Journal of Mathematics and Mechanics*, 17:59–87.

Kragh, H. (1999). *Quantum Generations*. Princeton University Press.

Kramers, H. A. (1920). Über den einfluß eines elektrischen feldes auf die feinstruktur der wasserstofflinien. *Zeitschrift für Physik A Hadrons and Nuclei*, 3(4):199–223.

Kuhn, T. (1978). *Black-body theory and the quantum discontinuity, 1894-1912*. Oxford University Press.

Landsman, N. P. (2009). Born rule and its interpretation. *Compendium of quantum physics*, pages 64–70.

Lorenz, E. N. (1963). Deterministic nonperiodic flow. *Journal of the atmospheric sciences*, 20(2):130–141.

Lupher, T. (2010). Not particles, not quite fields: An ontology for quantum field theory. *Physics and Metaphysics*, 13:155–173.

Mermin, N. D. (1989). What's wrong with this pillow? *Physics Today*, 42:9.

Metropolis, N. and Nelson, E. C. (1982). Early computing at los alamos. *Annals of the History of Computing*, 4(4):348–357.

Motterlini, M. (1999). *For and Against Method*. The University of Chicago Press.

Osnaghi, S., Freitas, F., and Freire Jr, O. (2009). The origin of the everettian heresy. *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 40(2):97–123.

Pais, A. (1986). *Inward bound*. Clarendon Press.

Planck, M. K. E. L. (1900). Zur theorie des gesetzes der energieverteilung im normalspectrum. *Verhandl. Dtsc. Phys. Ges.*, 2:237.

Quine, W. (1951). Ontology and Ideology. *Philosophical Studies*, 2:11–15.

Robertson, H. P. (1929). The uncertainty principle. *Physical Review*, 34(1):163.

Rojas, R. and Hashagen, U. (2000). *The First Computers: History and Architectures*. The MIT Press.

Rosenfeld, L. (1972). Leon Rosenfeld letter to FJ Belinfante, 22-June-1972. *UCIspace*.

Rueger, A. (1992). Attitudes towards infinities: responses to anomalies in quantum electrodynamics, 1927-1947. *Historical studies in the physical and biological sciences*, pages 309–337.

Scheibe, E. (1973). *The Logical Analysis of Quantum Mechanics*. Pergamon Press.

Schlipp, P. A. (1959). *Albert Einstein, Philosopher-Scientist*. Harper & Brothers Publishers.

Schrödinger, E. (1926). Quantisierung als eigenwertproblem. *Annalen der physik*, 385(13):437–490.

Schrödinger, E. (1926). Über das Verhältnis der Heisenberg-Born-Jordanischen Quantenmechanik zu der meinen. *Annalen der Physik*, 79:734.

Schrödinger, E. (1935). Die gegenwärtige situation in der quantenmechanik. *Naturwissenschaften*, 23(49):823–828.

Schweber, S. S. (1986). The empiricist temper regnant: Theoretical physics in the united states 1920-1950. *Historical Studies in the Physical and Biological Sciences*, pages 55–98.

Schweber, S. S. (1994). *QED and the men who made it.* Princeton University Press.

Seevinck, M. (2014). *Grondslagen van de Quantummechanica.* Instituut voor Geschiedenis en Grondslagen van de Natuurwetenschappen van de Universiteit Utrecht.

Slater, J. (1967). Quantum physics in america between the wars. *International Journal of Quantum Chemistry*, 1(S1):1–23.

Sommerfeld, A. (1919). *Atombau und Spektrallinien.* Friedr. Vieweg & Sohn.

Struyve, W. (2011). Pilot-wave approaches to quantum field theory. *Journal of Physics: Conference Series*, 306.

Szilárd, L. (1969). Reminiscences. *In: The Intellectual Migration: Europe and America, 1930-1960, Donald Fleming and Bernard Bailyn (eds.), Harvard University Press.*

Teller, P. (1995). *An Interpretive Introduction to Quantum Field Theory.* Princeton University Press.

Thurgood, L., Golladay, M. J., and Hill, S. T. (2006). Us doctorates in the 20th century. *Arlington, VA: National Science Foundation.*

Towler, M., Russell, N., and Valentini, A. (2011). Time scales for dynamical relaxation to the born rule. *Proceedings of the Royal Society A: Mathematical, Physical and Engineering Science*, page rspa20110598.

Valentini, A. (1991). Signal-locality, uncertainty, and the sub-quantum H-theorem. *Physics Letters A*, 158:1–8.

van der Waerden, B. (1967). *Sources of Quantum Mechanics.* North-Holland Publishing Co.

Van Vleck, J. (1964). American Physics Comes of Age. *Physics Today*, 17:22.

von Meyenn, K. (2005). *Wolfgang Pauli: Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.* Springer.

von Neumann, J. (1932). *Mathematische Grundlagen der Quantenmechanik.* Springer.

Weinberg, S. (1986). Particle physics: Past and future. *International Journal of Modern Physics A*, 1(01):135–145.

Weiner, C. (1969). A New Site for the Seminar: the Refugees and American Physics in the Thirties. *In: The Intellectual Migration: Europe and America, 1930-1960, Donald Fleming and Bernard Bailyn (eds.), Harvard University Press.*

Werner, F. (1964). The Foundations of Quantum Mechanics. *Physics Today (Jan. 1964)*, pages 53–60.

Wheeler, J. and Zurek, W. (1983). *Quantum Theory and Measurement.* Princeton University Press.

Wightman, A. S. (1967). Introduction to some aspects of the relativistic dynamics of quantized fields. In *High energy electromagnetic interactions and field theory*, volume 1, page 171.

Wigner, E. (1976). Interpretation of Quantum Mechanics. *In: Quantum Theory and Measurement, John Wheeler and W. Zurek (eds.), Princeton University Press, 1983.*

Zurek, W. H. (1981). Pointer basis of quantum apparatus: Into what mixture does the wave packet collapse? *Physical Review D*, 24(6):1516.

Zurek, W. H. (2002). Decoherence and the transition from quantum to classical-revisited. *Los Alamos Science*, 27:86–109.