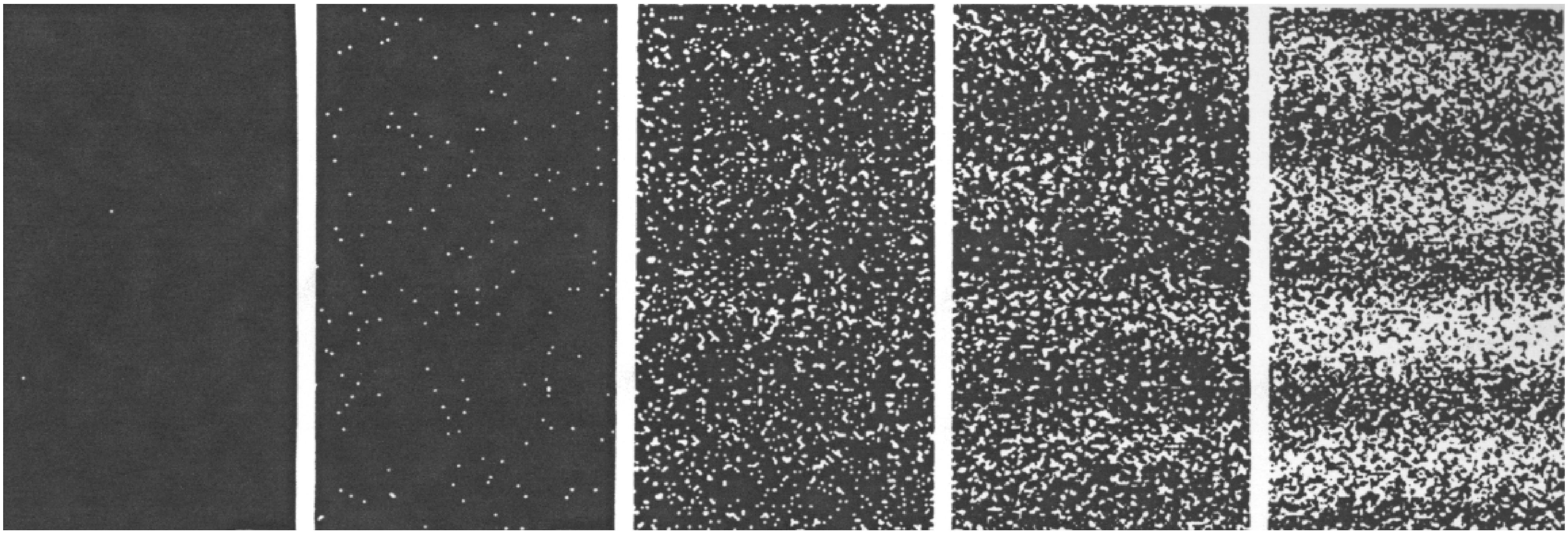
Pilot wave theory, Bohmian metaphysics, and the foundations of quantum mechanics

Lecture 7

Not even wrong. Why does nobody like pilot-wave theory?



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Acknowledgements

The material in this lecture is to a large extent a summary of publications by Peter Holland, Antony Valentini, Guido Bacciagaluppi, David Peat, David Bohm, Basil Hiley, Oliver Passon, James Cushing, David Peat, Christopher Norris, H. Nikolic, David Deutsch and the Daily Telegraph. Bacciagaluppi and Valentini's book on the history of the Solvay conference was a particularly vauable resource. Other sources used and many other interesting papers are listed on the course web page:

www.tcm.phy.cam.ac.uk/~mdt26/pilot_waves.html

MDT

Life on Mars

REVIEWS OF

MODERN PHYSICS

VOLUME 42, NUMBER 4 OCTOBER 1970

Editorial Comment

The subject of the following paper lies in the border area between physics, semantics and other humanities. The Editor does not feel called upon to draw the border of physics firmly or restrictively, the more so as the subject seems to maintain a broad appeal after 45 years of quantum mechanics.

Several of our referees have worked long, thoroughly and repeatedly on this paper. Their points of view and recommendations have diverged widely. Publication of the reviews together with the paper would have given our readers an impression of the current spectrum of opinions on the subject.

A recurrent historical question underlies much of the paper and of the reviewer arguments. The early conceptual basis of quantum mechanics is often referred to as the "Copenhagen Interpretation." Just what did Bohr and the other originators of this interpretation actually mean to convey by their statements and writings? Well-known textbooks regard the Copenhagen Interpretation as referring implicitly to ensembles of physical systems; indeed they formulate "standard" quantum mechanics on this basis. Professor Ballentine regards the Copenhagen Interpretation as very restrictive and emphasizes the need for broadening it into a "Statistical Interpretation."

One of our referees regards the Author's point as moot and in need of no further emphasis. Another referee regards instead the assessment of the "validity" of various interpretations of quantum theory as a legitimate and difficult problem of physics; he feels, in fact, that the Author leaves the issue unresolved.

The Editor is experimenting with this note, trying to convey the flavor of controversy surrounding an unusual paper. He might take a different attitude on future occasions.

Today we are apt to forget that - not so very long ago - disagreeing with Bohr on quantum foundational issues, or indeed just writing about the subject in general, was professionally equivalent to having a cuckoo surgically attached to the centre of one's forehead via a small spring. Here, for example, we encounter the RMP Editor feeling the need to add a remark on *editorial policy* before publishing Prof. Ballentine's (hardly very controversial) paper on the statistical interpretation, concluding with what seems very like a *threat*. One need hardly be surprised at Bohm's reception seventeen years earlier..

An intimidating atmosphere..

"The idea of an objective real world whose smallest parts exist objectively in the same sense as stones or trees exist, independently of whether or not we observe them. . . is impossible." [Heisenberg, 1958]

"[The quantum postulate] *implies a renunciation of the causal space-time coordinates."* [Bohr 1934]

"No concealed parameters can be introduced with the help of which the indeterministic description could be transformed into a deterministic one. Hence if a future theory should be deterministic, it cannot be a modification of the present one but must be essentially different. How this could be possible without sacrificing a whole treasure of well-established results I leave to the determinists to worry about." [Born 1949]

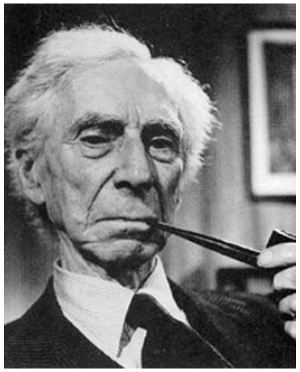
"It should be emphasized, however, that the probability function does not in itself represent a course of events in the course of time. It represents a tendency for events and our knowledge of events. The probability function can be connected with reality only if one essential condition is fulfilled: if a new measurement is made to determine a certain property of the system." [Heisenberg 1958]

"In contrast to ordinary mechanics, the new quantum mechanics does not deal with a space-time description of the motion of atomic particles. . . The difficulties · · · seem to require just that renunciation of mechanical models in space and time which is so characteristic a feature in the new quantum mechanics." [Bohr 1934]

..an attitude which propagated into more or less every modern textbook: *"It is clear that* [the results of the double slit experiment] *can in no way be reconciled with the idea that electrons move in paths. In quantum mechanics there is no such concept as the path of a particle."* [Landau and Lifshitz].

Einstein and Schrödinger remained incredulous at such certitude on these matters, and with good reason. We now know that the mere *existence* of de Broglie-Bohm theory - irrespective of whether it is has anything to do with 'reality' - shows all these statements to be incorrect; the quantum postulates do not *imply* any such thing. The Copenhagenists propagated circular arguments of *consistency* disguised as arguments of *inevitability*.

The value of philosophy



Engagement with QM interpretations debate can be a frustrating and disappointing business. Mutual visceral hatred of the various camps clearly an obstacle to progress. But what do we mean by 'progress' here? Let's ask Bertrand Russell. In his book *The Problems of Philosophy* (1912) he writes:

"What is the value of philosophy and why ought it to be studied? It is the more necessary to consider this question, in view of the fact that many men, under the influence of science or of practical affairs, are inclined to doubt whether philosophy is anything better than innocent but useless trifling, hair-splitting distinctions, and controversies on matters concerning which knowledge is impossible."

Indeed practically-minded physicists regard QM foundations as pointless since no direct applications follow from it. Russell continues: although philosophy does aim at 'knowledge which gives unity and system to the body of the sciences', it admittedly had little success in this respect and could only answer very few of its questions definitely. More important than the answers are the questions is asks:

"Philosophy is to be studied, not for the sake of any definite answers to its questions since no definite answers can, as a rule, be known to be true, but rather for the sake of the questions themselves: because these questions enlarge our conception of what is possible, enrich our intellectual imagination and diminish the dogmatic assurance which closes the mind against speculation."

Debate on QM interpretion thus a spectacular success story. Few questions have been settled ultimately, but every alternative interpretation enlarges 'our conception of what is possible'. Pilot-wave theory - and all the others - enrich our conception of what the quantum world may be (e.g. Bell's theorem..).

On pragmatism and instrumentalism

Engaging in these debates you eventually run into impatient people like **Mr. Logic** who - after rolling their eyes heavenwards and sighing - say things like the following (very slowly, as if talking to a child):

"*Look - you're obviously just some idiot who doesn't understand the scientific method. We wouldn't even care if your lunatic speculation was ontologically correct, whatever that really means. The goal of science is merely to improve our ability to predict experimental results and to create useful technology*."

"*A theory makes a specific prediction about the quantitative value of some specific measurement in a proposed experimental setup. That experiment is then performed and the measured result either agrees or disagrees with the quantitative prediction to within some obtained accuracy. If the experimentally measured value agrees with the quantitative prediction then that is taken as evidence in support of the theory as a whole and the experiment is said to verify the theory. On the other hand, if the experimentally measured value disagrees with the quantitative prediction then that is taken as evidence in opposition to the theory and the experiment is said to falsify the theory. Get it now, moron?*"

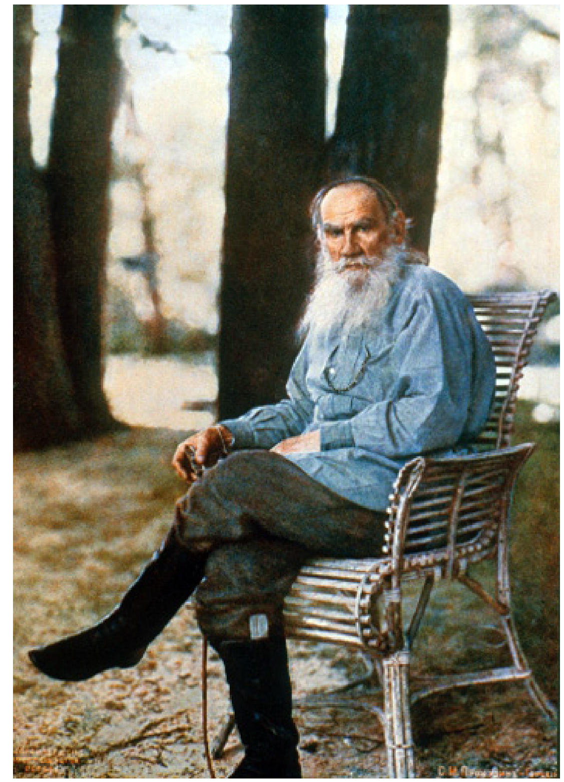
So goal of science is to catalogue experimental results. True surely, but you must remember two things: **1.** Have other goals if you like, such as understanding nature on a deeper conceptual level, and **2.** Instrumentalist bores like Mr. Logic are really no fun at parties (but bridges they build don't collapse).

Mathematical theories work for a reason, and speculating about how mathematical objects in equations map onto objects in real world can also lead to new developments of pragmatic aspects. What do our theories tell us about the world? An interesting question asked by workers in *every branch of science* - except pragmatic quantum physicists who are, of course, special.

Confirmation bias

Confirmation bias is a 'tendency to search for or interpret new information in a way that confirms one's preconceptions and to avoid information and interpretations which contradict prior beliefs'.

"The most difficult subjects can be explained to the most slow-witted man if he has not formed any idea of them already; but the simplest thing cannot be made clear to the most intelligent man if he is firmly persuaded that he knows already, without a shadow of doubt, what is laid before him."
L. Tolstoy, *The Kingdom of God is Within You* (1894)



"I know that most men, including those at ease with problems of the highest complexity, can seldom accept even the simplest and most obvious truth if it be such as would oblige them to admit the falsity of conclusions which they have delighted in explaining to colleagues, which they have proudly taught to others, and which they have woven, thread by thread, into the fabric of their lives."
L. Tolstoy, *What is Art?* (1897)

You know what? I do this, and so do you.. Having decided to give a course extolling the virtues of pilot-wave theory, I found myself reading a lot. I came to feel a small thrill when I read a supportive statement in the literature. When I read a negative statement, I felt a twinge of annoyance. Eventually one becomes irrational. Everything is framed through the spectacles of the preferred viewpoint.

"Any competent philosopher who does not understand something will take care not to understand anything else whereby it might be explained." [David Lewis]

Today's questions

Approximately *nobody* likes pilot-wave theory. Why?

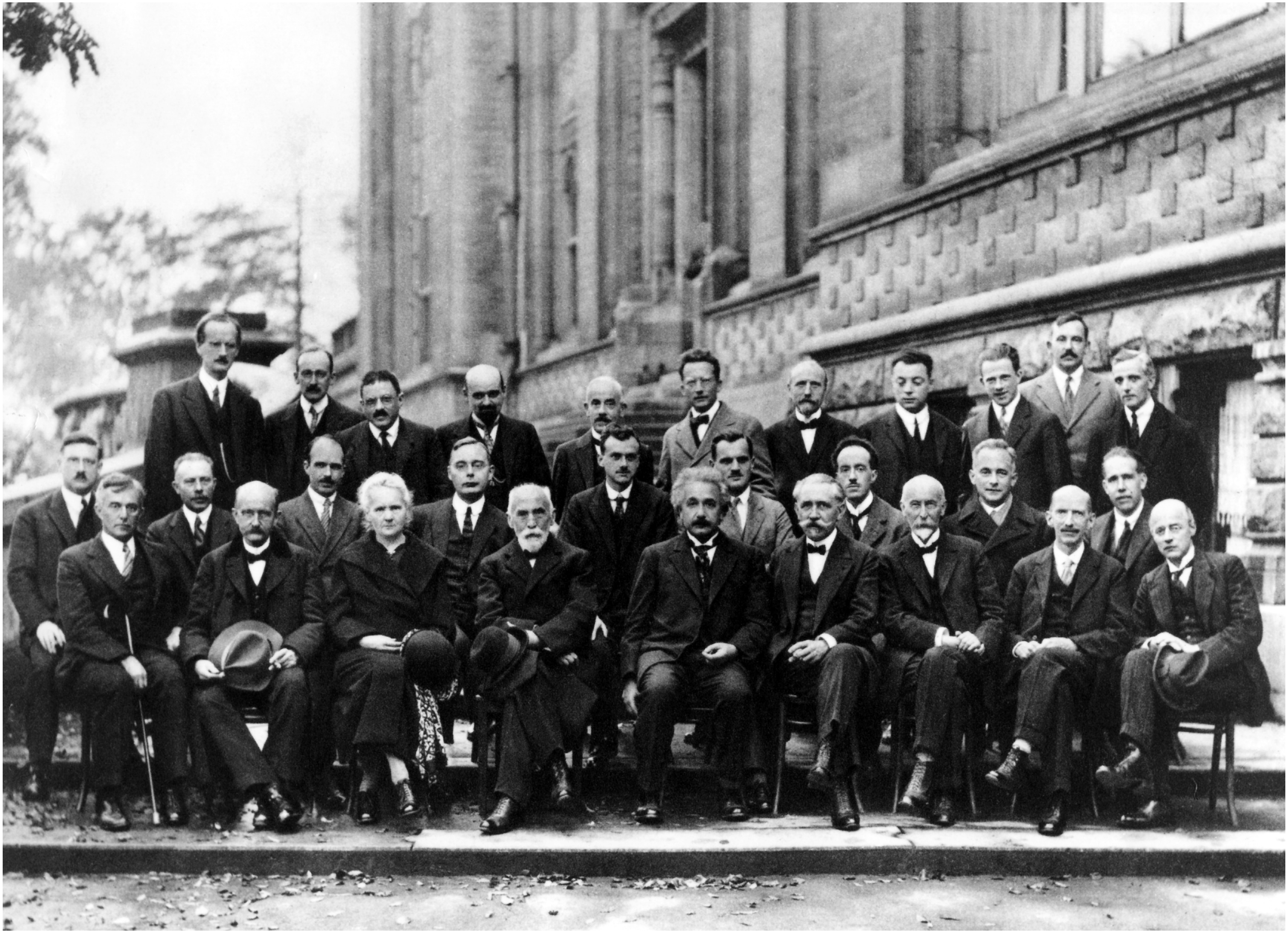
Well, 95%+ of physicists have never heard of it, because it is not taught in universities (there are very few courses like this currently offered worldwide - probably something like three). We have only just emerged from the era where students interested in pilot-wave theory were subject to career-ending threats from their professors. In many places, that attitude still exists. What is the reason for this?

Historical contingency: We first explore why the specific ideas proposed independently by de Broglie and Bohm were rejected by the establishment, given what was known at the time. Were these rejections fair? Inevitable? What would have happened if 'hidden variables theories' had been taken seriously?

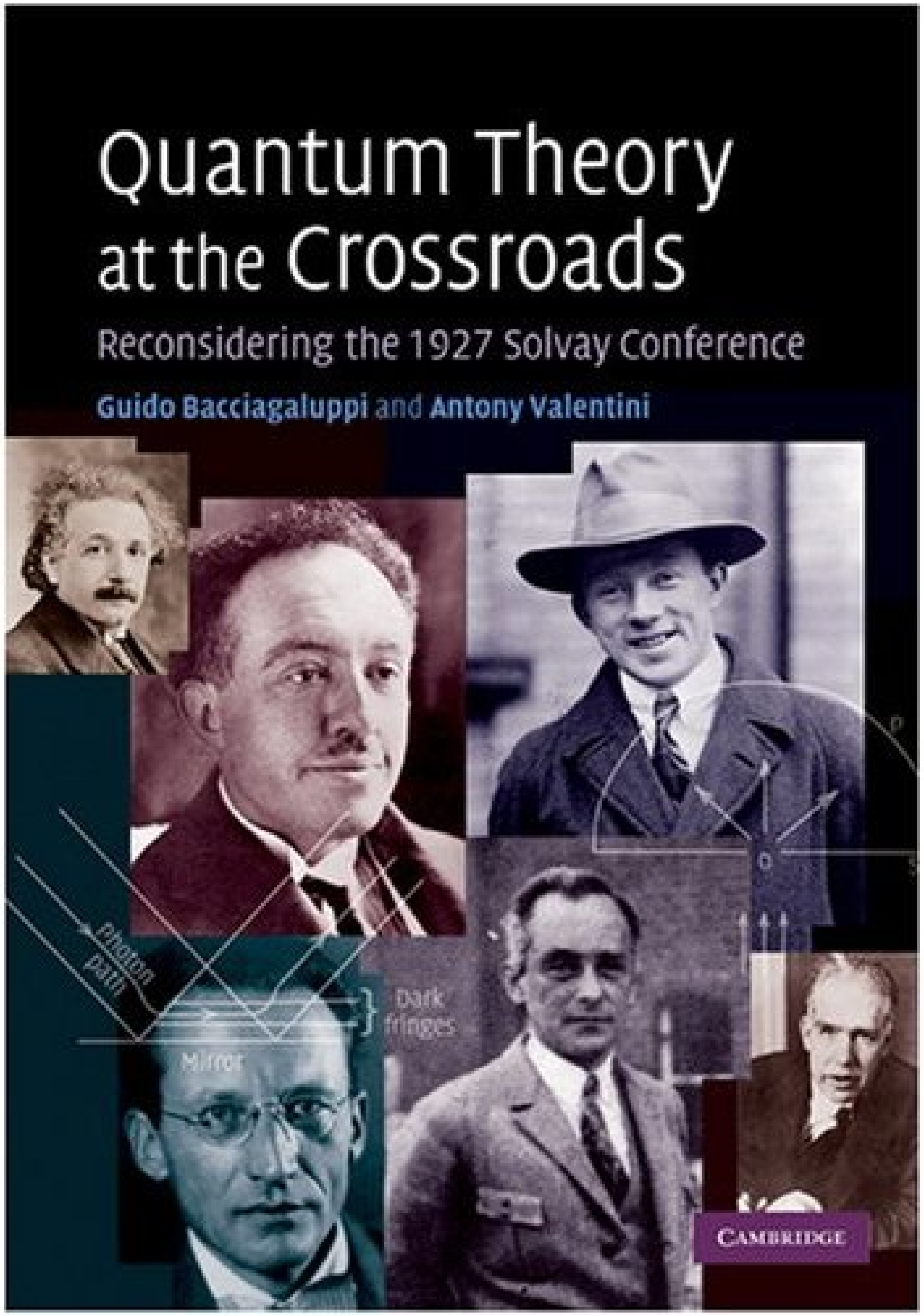
Objections: We then examine specific objections to pilot-wave theory, and to hidden-variables in general, raised in the literature in the modern era. We classify them into the following categories:

1. **Silly objections**: 10
   Statements about the theory which are simply and obviously incorrect.
2. **Apparently reasonable objections**: 9
   Sensible questions that one might reasonably raise - if one has not been taught the theory and is learning about it for the first time - but which may be explained or answered trivially.
3. **Reasonable objections**: 4
   Sensible objections to which the response is only partial or incomplete, but one might reasonably expect the objection to be overcome through further mathematical or conceptual development (for example, by having a larger number of clever people working on it).
4. **Definitive objections**: 0
   Objections which imply pilot-wave theories cannot be correct. No such issues are currently known.

The 5th Solvay conference, 1927



The book to read



Fascinating!

Pilot wave theory in 1927

- **May 1927**: De Broglie publishes 'Wave mechanics and the atomic structure of matter and of radiation', *Le Journal de Physique et le Radium*, **8**, 225 (1927) - the final development of a remarkable progression in thought that had begun in 1923. Paper contains full modern pilot-wave dynamics, though only for single particles.

  [See p. 61 Bacciagaluppi and Valentini (BV) for analysis of this paper.]

  "*One will assume the existence, as distinct realities, of the material point and of the continuous wave represented by the function Ψ, and one will take it as a postulate that the motion of the point is determined as a function of the phase of the wave by the equation $\mathbf{v} = -\frac{1}{m_0}\left(\nabla S + \frac{e}{c}\mathbf{A}\right)$. One then conceives the continuous wave as guiding the motion of the particle. It is a pilot wave.*"

- **August 1927**: "*...it is very rich in ideas and very sharp, and on a much higher level than the childish papers by Schrödinger...*" [Pauli, letter to Bohr of August 1927, referring to this paper of de Broglie].

- **October 1927**: 5th Solvay conference takes place in Brussels. De Broglie presents the main results of his *Journal de Physique et le Radium* paper, but now for a nonrelativistic system of N particles guided by a wave function Ψ in configuration space that determines the particle velocities according to de Broglie's law of motion. The theory published in the Proceedings is absolutely pilot-wave dynamics as we know it today, which is why it is usually called, er.., 'Bohmian mechanics'.

De Broglie's presentation at the Solvay conference

- Begins by reviewing his early Ph.D. thesis work: showed E and $\mathbf{p}$ determined by *phase* of an associated wave: $E = \partial S/\partial t$, $\mathbf{p} = -\nabla S$. Implies principles of least action and Fermat identical. Quantization conditions appear in natural way. Far reaching implications for statistical mechanics.
- Then outlines Schrödinger's work, who had set out to derive a wave equation for de Broglie's phase waves. (TISE amounts to a mathematical transcription into the language of the usual wave equation of de Broglie's expressions for frequency and phase velocity of electron waves - brought into the non-relativistic limit [BV, p.58-61]). Louis notes wave equation constructed so phase S of Ψ-function is solution of Hamilton-Jacobi equation in geometrical-optics limit (Lecture 2); Schrödinger identifies particles with localized wave packets.
- Problem with Schrödinger: it '*seems a little paradoxical to construct a configuration space with the coordinates of points that do not exist*'. Proposes they do exist, and motivates guidance equations since phase of Ψ obeys Hamilton-Jacobi equation in classical limit:

  "*We propose to assume by induction that this* [velocity] *formula is still valid when the old Mechanics is no longer sufficient, that is to say when S is no longer a solution of the Jacobi equation. If one accepts this hypothesis, which appears justified by its consequences, the* [relativistic guidance equation] *completely determines the motion of the corpuscle as soon as one is given its position at an initial instant. In other words the function S, just like the Jacobi function of which it is the generalization, determines a whole class of motions, and to know which of these motions is actually described it suffices to know the initial position.*"
- Shows distribution $|\Psi|^2$ of particles preserved in time ('equivariance'). Derives the Born rule for probabilities. His expression *'probabilité de presence'* makes it clear he means probability for electron *being* somewhere, not just probability of *finding* it there in a suitable measurement.

[See BV, p.75-84]

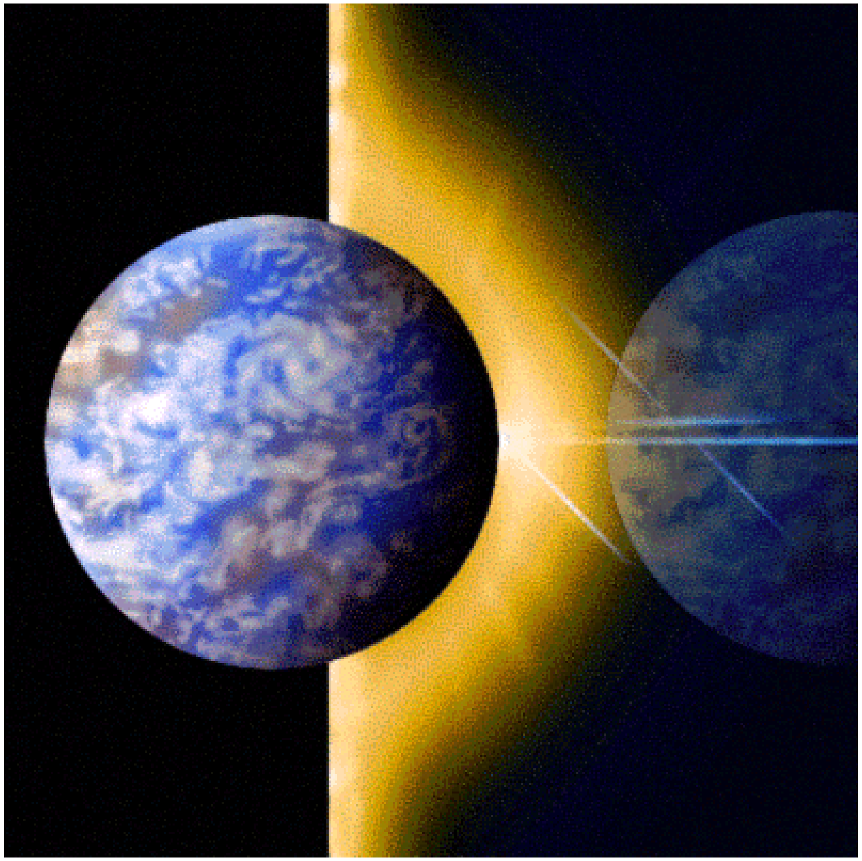
De Broglie's presentation at the Solvay conference, continued..

- Generalizes dynamics for many-body case. Says difficult to construct wave Ψ that can generate motion of *relativistic* many-body system (easy for one-body case). Thus difficulty of formulating fundamentally Lorentz-invariant many-body pilot-wave theory recognized from the beginning.
- Stresses wave Ψ appears as both a pilot wave and a probability wave. There are thus no grounds for abandoning determinism, and in this his theory differs from that of Mr. Born.
- Notes in passing can write dynamics in classical $\mathbf{F} = m\mathbf{a}$ form with quantum potential term (as Bohm did in 1952), but considers equation for *velocity* to be fundamental equation of motion.
- Applications: scattering of single particle by fixed-potential. Interference and diffraction of photons. Pressure exerted by light on a mirror. Stationary states of hydrogen atom. Calculate of electron velocity during atomic transition; such transitions visualizable without 'quantum jumps'.
- Presentation concludes with lengthy discussion of recent experiments involving diffraction, interference, and scattering of electrons. For diffraction of electrons by crystal lattice, points out scattered wave function has maxima in certain directions, and notes pilot-wave theory says electrons preferentially scattered in these directions. Referring to scattering maxima observed recently by Davisson and Germer, for electrons incident on a crystal, he says "*There is direct numerical confirmation of the formulas of the new Dynamics. . .*"

Discussion following de Broglie's (Tuesday) presentation extensive, detailed and varied. Our man replies satisfactorily to most queries. Conference concludes on Saturday with General Discussion, during which theory continues to be extensively debated. However, Pauli has an *objection..*

[See BV, p.75-84]

Utter madness



Let's try an experiment!

We shall believe the many-worlds interpretation, just for four slides..

Universe I: Pauli has a revelation..

The 1927 Solvay conference

MR DE BROGLIE: *. . . and so we conclude that the dualist representation by corpuscles and associated waves allows us now to see the non-relativistic quantum theory as just statistical mechanics with a different (quantum) dynamics. And with that, gentlemen, Madame Curie, I end my presentation. I thank you for your attention.*

Enthusiastic applause. Intermittent whooping from near the back of the hall.

Mr. Bohr is sitting in the centre of the front row. He applauds and flashes a beaming smile at Mr. de Broglie. However there appears to be something wrong with his eyes.

MR PAULI: *My dear de Broglie, I should - I think - like to congratulate you.. Since I read your very sharp article in the* Journal de Physique *I have been intrigued by this approach, as I have expressed several times to Mr. Bohr. With your apparently successful extension to the many-body case I begin now to see that much of what we have thought up to now is* ganz falsch *- not even wrong. I even see how some doubts I had about inelastic scattering could be resolved. Working alone in Paris away from our little circle has been good for you it seems - I had hitherto suspected the new mechanics would be a German creation..* [He nods in the direction of Mr. de Broglie and sits down.]

Mr. Heisenberg leans against one of the walls of the lecture theatre, smoking a cheroot. He does not smile or clap. His eyes are fixed on the back of Pauli's neck. A thin hiss of smoke escapes from his delicately pursed mouth.

Universe I: One year later, Bohr speaks

MR. BOHR: . . . *so I shall try to describe to you a certain general point of view, which I hope will be helpful in order to harmonize the apparently conflicting views taken by different scientists. I call it* [offstage, a trumpet sounds] *the 'Complementarity Principle'. It says, you see, that with there being this dual wave-particle nature of reality there is - we must all now agree - no logical picture that can simultaneously describe and be used to reason about all properties of a quantum system.* [General hilarity, then silence broken by occasional embarrassed coughing. Someone shouts 'Keep up!'.]

MR. EINSTEIN: *But my dear Bohr, is that not precisely what M. de Broglie has provided? It is, if I may say, now generally accepted that the de Broglian mechanics has lifted a corner of the Great Veil.*

MR. HEISENBERG: *Seen one of these so-called 'electrons' when you're not looking at it, have you? Eh?* [Heisenberg lunges at Bragg, who has foolishly called out 'Yes!'. He is restrained by Mr. Dirac.]

MR. SCHRÖDINGER: *What madness is this? These conflicts Bohr speaks of are in your head and his. You would replace de Broglie's beautiful, logical, comprehensible quantum theory - which so elegantly extends the theory of poor Boltzmann and Mr. Einstein - with such pettifogging mumbo-jumbo? This would lead us down the road to rats being at the same time both dead and alive..*

MR. BOHR: *But.. I am the Father of Quantum Mechanics. I have an Institute. You must listen to me..*

ALL: *Father of My Arse, mate. Hoo, hoo. Get back to Copenhagen..* [Enter men in white coats. Bohr and Heisenberg are put into straitjackets and dragged away.]

MR. HEISENBERG: [offstage] *But to hope for so-called hidden variables is like saying 2+2=5. OOoofffff!*

Soon afterwards, inspired by the physicists and instead of waiting until the 1960s as expected, the philosophers have all the logical positivists taken outside and shot. All the old problems in philosophy are opened again, and everyone has much more fun. Henceforth, quantum theorists are seen by the public - if seen at all - as rather dull on account of the lack of barking paradoxes. At least until Marie Curie discovers nonlocality in 1935..

Universe II: The one we live in..

The 1927 Solvay conference

MR. DE BROGLIE: *. . . and so we conclude that the dualist representation by corpuscles and associated waves allows us now to see the non-relativistic quantum theory as just statistical mechanics with a different (quantum) dynamics. And with that, gentlemen, Madame Curie, I end my presentation. I thank you for your attention.*

Polite applause. Some photon somewhere goes the other way.

MR. PAULI: *It seems to me that, concerning the statistical results of scattering experiments, the conception of Mr. de Broglie is in full agreement with Born's theory in the case of elastic collisions, but that it is no longer so when one also considers inelastic collisions. I should like to illustrate this by the example of the rotator, which was already mentioned by Mr. de Broglie himself. As Fermi has shown. . .* [there follows a technical argument†. . .] *. . . Mr. de Broglie's point of view does not then seem to me compatible with the requirement of the postulate of the quantum theory, that the rotator is in a stationary state both before and after the collision. · · · In Born's theory, agreement with the quantum postulate is realized thus, that the different partial waves in configuration space, of which the general solution of the wave equation after the collision is composed, are applicable* separately *in a statistical way. But this is no longer possible in a theory that, in principle, considers it possible to avoid the application of notions of probability to individual collision processes.*

† See BV p.511 for the full translation.

Universe II: de Broglie's response

MR. DE BROGLIE: *Fermi's problem is not of the same type as that which I treated earlier; indeed, he makes configuration space play a part, and not ordinary space. The difficulty pointed out by Mr. Pauli has an analogue in classical optics. One can speak of the beam diffracted by a grating in a given direction only if the grating and the incident wave are laterally limited, because otherwise all the diffracted beams will overlap and be bathed in the incident wave. In Fermi's problem, one must also assume the wave* Ψ *to be limited laterally in configuration space.*

MR. LORENTZ: *The question is to know what a particle should do when it is immersed in two waves at the same time.*

MR. DE BROGLIE: *The whole question is to know if one has the right to assume the wave* Ψ *to be limited laterally in configuration space. If one has this right, the velocity of the representative point of the system will have a constant value, and will correspond to a stationary state of the rotator, as soon as the waves diffracted by the* ϕ*-axis will have separated from the incident beam. One can say that it is not possible to assume the incident beam to be limited laterally, because Fermi's configuration space is formed by the superposition of* identical *layers of height* 2π *in the direction of the* ϕ*-axis; in other words, two points of configuration space lying on the same parallel to the* ϕ*-axis and separated by a whole multiple of* 2π *represent the* same *state of the system. In my opinion, this proves above all the artificial character of configuration spaces, and in particular of that which one obtains here by rolling out along a line the cyclic variable* ϕ.

And this little exchange is supposed to be why pilot-wave theory was rejected, and why de Broglie gave it up.. Here is how historians usually characterize what happened: "It was immediately clear that nobody accepted his ideas. . . In fact, with the exception of some remarks by Pauli. . . de Broglie's causal interpretation was not even further discussed at the meeting. Only Einstein once referred to it *en passant*." [Jammer, *The Interpretations of QM in Historical Perspective*]. This is factually incorrect on every level. The theory was *extensively* discussed by most of the participants, both after de Broglie's presentation, and in the General Discussion. The only critical remark (apart from a minor one by Kramers) was Pauli's, and as we shall see, Pauli's objection was more confused than is generally thought (it is *falsch* if not *ganz falsch*). Contrary to popular opinion, de Broglie's reply to Pauli above *does* contain the essential points required for a proper treatment of inelastic scattering.

Pauli's objection in modern terms

Background: look back at 'Are there quantum jumps?' and 'Collision of electron and hydrogen atom' from lecture 3. Full story in BV p.227-247.

Pauli's objection conceptually equivalent to scattering of particle by H atom (in turn related to measurement problem) - can be *elastic* (H left in ground state) or *inelastic* (H left in excited state).

Scattering

- Incident particle represented by freely evolving wave packet Ψ_{inc} - necessarily spatially finite (*laterally and longitudinally limited*). Wave function evolves into $\Psi = \Psi_{inc} + \Psi_{sc}$ where Ψ_{sc} is scattered wave. At large distances from atom, only Ψ_{sc} contributes to particle current density $\mathbf{j}$.
- Can simplify with *infinitely extended plane wave* $\mathrm{e}^{i\mathbf{k}\cdot\mathbf{x}}$ for Ψ_{inc}. Asymptotic wave function roughly $\Psi = \mathrm{e}^{ikz} + f(\theta, \phi)\frac{\mathrm{e}^{ikr}}{r}$ for z-axis parallel to $\mathbf{k}$. Scattering amplitude f gives differential cross section $\mathrm{d}\sigma/\mathrm{d}\Omega = |f(\theta, \phi)|^2$.
- Standard treatment: current density $\mathbf{j}$ gives rate of probability flow into solid angle $\mathrm{d}\Omega$. $\mathbf{j}$ is current associated with Ψ_{sc} only, though Ψ_{sc} overlaps with $\Psi_{inc} = \mathrm{e}^{i\mathbf{k}\cdot\mathbf{x}}$. Justified as infinite plane wave is abstraction: real incident wave spatially limited - obvious point well known to Solvay people.
- In pilot-wave theory terms, $\mathbf{v} = \frac{\mathbf{j}}{|\Psi|^2} = \frac{\nabla S}{m}$ interpreted as actual velocity field of ensemble of particles with positions distributed according to $|\Psi|^2$. Differential cross section $\mathrm{d}\sigma/\mathrm{d}\Omega$ measures fraction of incident particles whose actual trajectories end (asymptotically) in solid angle $\mathrm{d}\Omega$.
- For inelastic scattering, outgoing packets ψ_n expand with different speeds - eventually become widely separated. Scattered electron at $\mathbf{x}$ occupies only one such packet ψ_i which alone determines its velocity; atom left in energy eigenstate ϕ_i. Only thing unknown to de Broglie: effective 'collapse' to state $\phi_i\psi_i$ essentially *irreversible* due to interaction with many other degrees of freedom.
- If naively assume infinite incident plane wave corresponded to real situation, then outgoing electron would never reach a constant velocity as it would be guided by a superposition of overlapping terms.

Pauli's objection: what was wrong with it?

- Bohm discusses this point in one of his 1952 papers reintroducing pilot-wave theory. He concludes: *"Thus, Pauli's objection is seen to be based on the use of the excessively abstract model of an infinite plane wave."*. However this cannot be true. Not only is it highly unlikely that a physicist of Pauli's abilities would make such an elementary mistake, but Pauli states quite explicitly in his first sentence that Mr. de Broglie's conception is fine for *elastic* collisions (which one would expect to suffer from same problem).
- Real problem with Pauli's objection stems from his "As Fermi has shown.." remark. This refers to misleading optical analogy introduced by Fermi in a more restricted context: *(time-dependent) scattering of an electron in two spatial dimensions by a rotator - a model scattering centre with one rotational degree of freedom - is mathematically equivalent to (time-independent) scattering of a scalar light wave in three spatial dimensions by an infinite diffraction grating*. Unfortunately to be applied in this context one requires a *frequency-dependent* speed of light, and it cannot be applied to a real situation with a finite incident wave (see BV discussion).
- Clear from his answer that de Broglie understood general separation mechanism required to yield definite outcome, but was misled by false optical analogy and phrased his answer in terms of it.

Bohm continued downplaying de Broglie's contribution until his death, see e.g. the following rather naughty extract (from Bohm and Hiley's **1993** textbook). Given the existence of a clear question of priority (which Bohm would lose under any serious analysis) one would expect him to have paid more attention to finding out exactly what it was that de Broglie had done. However, this passage does express the common viewpoint:

"*The idea of a 'pilot wave' that guides the movement of the electron was first suggested by de Broglie in 1927, but only in connection with the one-body system. De Broglie presented this idea at the 1927 Solvay Congress where it was strongly criticised by Pauli. His most important criticism was that, in a two-body scattering process, the model could not be applied coherently. In consequence de Broglie abandoned his suggestion. The idea of a pilot wave was proposed again in 1952 by Bohm in which an interpretation for the many-body system was given. This latter made it possible to answer Pauli's criticism.*"

The green remarks are incorrect or misleading. Bohm's character was such that he was simply not interested in historical questions of priority.

Why did de Broglie give up?

Often see commentary like: "The Copenhagenists were of course so clever that they immediately saw the flaws in de Broglie's theory and he was forced to give up his heretical views in shame shortly after Solvay." Clearly not true, as their (few) criticisms are not in fact correct, and he didn't give it up immediately. In his book *An Introduction to the Study of Wave Mechanics* (1930) de Broglie gives three (measurement-related) reasons why he now considers pilot-wave concept unsatisfactory:

1. *For particle incident on imperfectly reflecting mirror, he says that if particle found in transmitted beam then reflected part of the wave must disappear (this being 'a necessary consequence of the uncertainty principle')*.

   Here he did not understand how pilot-wave theory accounts for effective (and practically irreversible) collapse of wave packet, by means of a separation into non-overlapping branches involving many degrees of freedom.

2. *Particle in free space guided by superposition of plane waves would have rapidly varying velocity and energy. Can't see how this could be consistent with outcomes of quantum energy measurements, which would coincide strictly with the energy eigenvalues present in the superposition.*

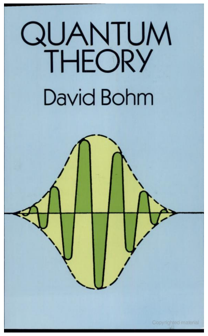
   To resolve this question de Broglie would have had to apply pilot-wave dynamics to quantum measurement process itself - including apparatus in the wave function if necessary (as done much later by Bohm).

3. *In applying pilot-wave theory to photons, de Broglie finds that in some circumstances the photon trajectories have superluminal velocities, which he considers unacceptable*.

   Though not a problem, we shall not discuss this here.

De Broglie's book does not mention Pauli's criticism.

Bohm - a quarter of a century later



In 1951 Bohm published his first textbook "Quantum Theory" which presented the *standard Copenhagen version* of the theory. It was widely praised and was adopted by many universities for their courses. It is acknowledged as one of the best books espousing the Copenhagen approach (though in typical Bohm style - the Schrödinger equation doesn't appear until p. 191 following extensive discussion). Pauli wrote warmly to Bohm approving of the way he had woven mathematics and physics together and addressed philosophical questions. Einstein (by then widely and unfairly considered a boring old reactionary desperate to hang on to determinism and an objective reality) told Bohm that he enjoyed that the book, but asked Bohm to visit him. The visit resulted in Bohm being 'turned'!

Murray Gell-Mann reports Bohm as saying: "*He [Einstein] talked me out of it. I'm back where I was before I wrote the book.*" One year later, around the time he was sacked from Princeton and exiled to Brazil for suspected communist sympathies, we get the following two papers:

'A suggested interpretation of the quantum theory in terms of "hidden" variables I', Phys. Rev **85**, 166 (1952)

Basic theory. 'The mere possibility of such an interpretation proves it is not necessary for us to give up a precise, rational, and objective description of individual systems at a quantum level of accuracy.'

'A suggested interpretation of the quantum theory in terms of "hidden" variables II', Phys. Rev **85**, 180 (1952)

Theory of measurement. Treatment of electromagnetic field (photoelectric and Compton effects), Appendix added in proof: discussion of interpretations of quantum theory proposed by de Broglie..

Strictly speaking, main thing done by Bohm not done before by de Broglie - other than stating the matter more clearly - was adding proper measurement theory (effectively inventing 'decoherence' concept). Thus calling the theory 'Bohmian mechanics'†, the 'Bohm interpretation', the 'Bohm theory', and its adherents 'Bohmians' is unfair to our French friend, and the practice should be stopped - in favour of 'de Broglie-Bohm theory', or better 'pilot-wave theory' (since no-one born outside France can pronounce de Broglie's surname). It is not an interpretation either, but effectively a new theory.

† According to his biographer, Bohm "was horrified when his own hidden variable theory was later called 'Bohmian mechanics' by some physicists".

What was the response to Bohm's theory?

Massive indifference, and outright hostility, just as Bohm predicted.

Witness Bohr fanboy Leon Rosenfeld writing to Bohm (in tone of St. Paul writing to an errant church): "*I shall certainly not enter into any controversy with you or anybody else on the subject of complementarity, for the simple reason that there is not the slightest controversial point about it.*" In their work together, Rosenfeld points out, he and Bohr had already made all the errors that could conceivably be made before arriving on solid ground: "*It is just because we have undergone this process of purification through error that we feel so sure of our results.*" Therefore, when Rosenfeld made assertions of infallibility, he said, he was not being dogmatic, for "*there is no truth in your suspicion that we may just be talking ourselves into complementarity by a kind of magical incantation. I am inclined to return that it is just among your Parisian admirers that I notice some disquieting signs of primitive mentality.*"

*　　*　　*

Max Dresden reports a visit to Oppenheimer's group at the Princeton Institute, where he enquired about Bohm. '*We consider it juvenile deviationism*," Oppenheimer replied. No, no one had actually read the paper - "*we don't waste our time.*" When Dresden gave a short seminar on Bohm's work, he was shocked by the response. Reactions were based less on scientific grounds than on accusations that Bohm was a fellow traveller, a Trotskyite, and a traitor. It was suggested that Dresden himself was stupid to take Bohm's ideas seriously. The overall reaction was that the scientific community should '*pay no attention to Bohm's work*'. As Dresden recalled, Abraham Pais also used the term '*juvenile deviationism*'. Another physicist said Bohm was a '*public nuisance*'. Oppenheimer went so far as to suggest that "*if we cannot disprove Bohm, then we must agree to ignore him.*"

*　　*　　*

Hints of Princeton reaction reach Bohm in exile in Brazil. He writes to a friend: "*As for Pais and the rest of the 'Princetitute' what those little farts think is of no consequence to me. In the past 6 years, almost no work at all has come out of that place.. I am convinced that I am on the right track.*"

Pauli's opinion in the 1950s

Letter to Bohm (1951): "*I just received your long letter of 20th November, and I also have studied more thoroughly the details of your paper. 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 measuring apparatus and in the observe* [sic] *system. As far as the whole matter stands now, your 'extra wave-mechanical predictions' are still a check, which cannot be cashed.*"

Letter to Fierz (1951): Bohm keeps writing me letters "*such as might have come from a sectarian cleric trying to convert me particularly to de Broglie's old theory of the pilot wave.*" In the last analysis Bohm's whole approach is "*foolish simplicity,*" which "*is of course beyond all help*".

What did Feynman think of Bohm and his work?

Despite the 19 pages of *The Character of Physical Law* (1964) devoted to the double slit experiment - "*Nobody knows any machinery. Nobody can give you a deeper explanation of this phenomenon than I have given; that is, a description of it.*"(!) - Feynman was actually a good friend of Bohm:

"*When I met Feynman he thought that the idea was crazy but after enough talk I convinced him that it is logically consistent.*" Feynman now agreed "*there might be something to my interpretation of quantum theory.*" He was "*convinced that it is a logical possibility and it may lead to something new.*"

* * *

Several months after their meeting at Belo Horizonte, Bohm wrote, "*Feynman was terrifically impressed with it, and now I think he is my friend for life.*" He was right; even as late as the 1980s, he always visited Feynman during his visits to the United States. Bohm valued their discussions saying [to David Peat], "*Feynman is a very clever fellow,*" his highest compliment. Yet even though Feynman took Bohm's hidden variables theory seriously, he was not willing to work on the theory himself. The reason, Bohm explained, was that Feynman "*could not see a problem in it.*"

* * *

Feynman arrived for the bongo drumming, and he, Yevick and Bohm spent several days at Copacabana Beach. In the light of Feynman's considerable reputation, Yevick was surprised that Bohm took the role of intellectual leader, doing most of the talking while Feynman walked beside him asking questions.

* * *

When [Feynman] mentioned his own lack of interest in the philosophical issues of science, one of the Ojai group, David Moody, joked, "*Dave knows a little bit about both.*" Feynman became angry, saying "*I can tell you one thing. David Bohm knows a lot more than just a little about physics.*" Booth Harris, a teacher at the Ojai school, remembered Feynman saying, "*You probably don't know how great he is,*" and noticed the considerable respect Feynman showed toward Bohm.

Quotations from David Peat's 1997 biography of Bohm

A reminder: why we like pilot-wave theory

- It preserves a realist ontology wherein particles possess determinate values of space-time location and momentum.
- They continue to possess such values *between* various acts of observation-measurement, rather than acquiring them only in consequence of being measured with respect to this or that parameter.
- This allows for greater continuity with certain components of classical (pre-quantum) physics such as the conservation laws respecting matter-energy and angular momentum.
- The pilot-wave hypothesis produces results in perfect accordance with those obtained in standard QM by means of the Schrödinger-derived wave probability function..
- ..while avoiding any recourse to mysterious ideas of the wavepacket collapse as somewhow brought about by observer intervention or only on the instant - in Schrödinger's parable - when the box is opened up for inspection and the cat thus release from its supposed 'superposed' (dead-and-alive) state.
- Pilot-wave theory also seeks to explain quantum effects such as photon deflection or multipath interference without proposing a massively expanded ontology of parallel worlds, shadow universes, multiple intersecting realities, etc..

What are the objections to this? Let's find out..

Silly objections..

..which nevertheless have appeared in print.

Pilot-wave theory..

- ..makes predictions about results of experiments different from those of standard QM.
- ..is mathematically equivalent to standard QM so it's not really an alternative at all.
- ..requires the postulation of a mysterious and undetectable quantum potential.
- ..requires the addition to quantum theory of a mysterious pilot wave.
- ..has been ruled out by the work of Bell.
- ..produces trajectories which are crazy, since they may be curved even when no classical forces are present.
- ..is unintuitive.
- ..is a childish regression to discredited classical modes of thought.
- ..makes the same predictions about results of experiments as standard QM so it is untestable and therefore meaningless.
- ..is in violation of Ockham's razor.

If you need to ask why they're silly, you've not been paying attention..

Apparently reasonable objections..

..which I shall discuss no further (see earlier lectures).

> "*Mr. Born can doubt the real existence of the trajectories calculated by Mr. de Broglie, and assert that one will never be able to observe them, but he cannot prove to us that these trajectories do not exist. There is no contradiction between the point of view of Mr. de Broglie and that of the other authors.*"
>
> **L. Brillouin**, discussion following de Broglie's 1927 Solvay talk.

Pilot-wave theory..

- ..is more complicated than standard QM since it involves an extra equation†.
- ..trajectories can't be correct, since a Bohmian particle may be at rest in stationary quantum states.
- ..is incompatible with quantum randomness on account of it being deterministic.
- ..is wrong, since it requires waves to move in configuration space.

† Objection based on surprisingly common misconception that standard QM defined solely by Schrödinger's equation, and does not actually need as part of its formulation any of the measurement postulates found in textbook quantum theory. It is only within a many-worlds framework that this view could begin to make sense, but it is very doubtful it makes sense even there.

I shall now go into 'apparently reasonable objections' that I want to discuss further, then look at the 'reasonable objections'. Boundary between these is elastic and I'll leave you to conclude where it is.

1. Pilot wave theory is inelegant..

Here is a confession from a pilot-wave recidivist.

"*In* [1965], *I rejected Bohm's interpretation for several reasons which no longer seem good to me. Even today, if you look at the Wikipedia encyclopaedia on the Web, you will find it said that Bohm's theory is mathematically inelegant. Happily, I did not give that reason in* [1965], *but in any case it is not true. The formula for the velocity field is extremely simple: you have the probability current in the theory anyway, and you take the velocity vector to be proportional to the current. There is nothing particularly inelegant about that; if anything, it is remarkably elegant!*"

Hilary Putnam (2005)

2. Impossibility proofs

Paul Feyerabend was visiting Bohr in Copenhagen (1953). At the time he knew nothing of Bohm's hidden variable papers, but had been impressed by the discussion of quantum measurement in [Bohm's] *Quantum Theory*. "*For the first time all this business about measurement made some kind of sense.*"

The first Feyerabend heard of Bohm's new theory was during a seminar given by Niels Bohr. Following the lecture he asked Bohr to clarify certain points. The Danish physicist's reaction was, "*Have you read Bohm*"? As Feyerabend put it, "*It seemed that, for him, the sky was falling in. . . Bohr was neither dismissive, nor shaken. He was* **amazed**."

In the midst of explaining to Feyerabend why Bohm's paper so disturbed him, Bohr was called away. The discussion continued without him for two more hours. Some of those present argued that the objections to Bohm's theory were not at all conclusive. As Feyerabend put it, the orthodox Copenhagen supporters tried to reply "*in the Bohrian fashion*". When this attempt was not successful, they said, "*But von Neumann has proved*. . . " which ended the discussion. Feyerabend noted, however, that Bohr himself did not use von Neumann's supposed proof as a crutch in that fashion.

What exactly was von Neumann supposed to have proved? In the early days of quantum theory, the mathematician John von Neumann presented a proof that quantum theory could never be reduced or transformed to any theory employing mechanical hidden variables. While most physicists had never bothered to read it, they paid lip service to von Neumann, assuming that his conclusion was true. Bohm, however, knew that the 'proof' was based upon such restrictive assumptions that it did not rule out a hidden variables theory at all. [From David Peat's *Infinite Potential*]

For decades von Neumann's work of 1932 was widely believed to constitute a logically irrefutable proof that any type of hidden-variables theory that gave *all* of the same predictions as standard QM was *impossible*. However, it was simply incorrect. He was able to obtain this result only with the mistaken assumption that one of standard QM's rules for statistical ensembles could be *extended* to the dispersion-free ensembles of any hidden-variables theory. See J.S. Bell. *On the problem of hidden variables in quantum mechanics* Rev. Mod. Phys. **38**, 447 (1966) - reprinted in *Speakable and Unspeakable*, Ch. 1. For a good modern discussion and summary in the pilot-wave context, see J.T. Cushing's *Quantum mechanics: historical contingency and the Copenhagen hegemony*, p. 131-134.

3. Bohm nicked the Schrödinger equation from Schrödinger

"*There is an air of contrivance about it that makes it unappealing. For example, the hidden wave has to satisfy a wave equation. Where does this equation come from? The frank answer is out of the air or, more accurately, out of the mind of Schrödinger. To get the right results, Bohm's wave equation must be the Schrödinger equation, but this does not follow from any internal logic of the theory and it is simply an* ad hoc *strategy designed to produce empirically acceptable answers. . . . It is on these grounds that most physicists find the greatest difficulty with Bohmian ideas . . . the* ad hoc *but necessary appropriation of the Schrödinger equation and the equation for the Bohmian wave has an unattractively opportunistic air to it.*"

J. Polkinghorne, Quantum theory: a very short introduction (2002)

[Brian Josephson made a similar point the first time I spoke about this theory here in the Cavendish.]

Unfortunately, as a matter of historical fact, the Schrödinger equation did follow from the internal logic of de Broglie's original theory. De Broglie invented the idea of a wave function. Schrödinger set out in the first place to find the general wave equation for de Broglie's waves, and his derivation of that equation owed much to the optical-mechanical analogy, which was a key component of de Broglie's approach to dynamics. It cannot be said that de Broglie 'appropriated' the Schrödinger equation for a purpose foreign to its origins, when the original purpose of the Schrödinger equation was in fact to describe de Broglie's waves.

4. No action-reaction symmetry

In classical physics there is an interplay between particle and field - each generates the dynamics of the other. In pilot wave theory Ψ *acts on positions of particles but, evolving as it does autonomously via Schrödinger's equation, it is not acted upon by the particles.*

- One may think this is unaesthetic, but while it may be reasonable to require reciprocity of actions in classical theory, this cannot be regarded as a *logical requirement* of all theories that employ the particle and field concepts, especially one involving a nonclassical field.
- Some people think this implies, from a deeper perspective than afforded by standard Bohmian mechanics or quantum theory, that Ψ should be regarded as '*nomological*' i.e. as an object for conveniently expressing the law of motion somewhat analogous to the Hamiltonian in classical mechanics. From this (deeper) cosmological level, the TDSE is merely phenomenological. Clearly this is highly speculative.
- Holland has explored some deeper ideas related to this question in his work on a possible Hamiltonian formulation of pilot-wave theory (outside the scope of this course). See the following papers:

  Hamiltonian theory of wave and particle in quantum mechanics I: Liouville's theorem and the interpretation of the de Broglie-Bohm theory (2001).

  Hamiltonian theory of wave and particle in quantum mechanics II: Hamilton-Jacobi theory and particle back-reaction (2001).

  Quantum back reaction and the particle law of motion (2005).

5. Asymmetry between position and momentum

Objection used in particular by Pauli and Heisenberg. But:

1. Pilot-wave theory gives position different ontological status than all other 'observables' to achieve clear ontology and solve conceptual problems of standard QM. Symmetry not an end in itself.
2. Hamiltonian in orthodox QM not invariant under general unitary transformations, though it is under usual space-time symmetries. Can say choosing a basis in Hilbert space is the symmetry, but you may as well Fourier transform Maxwell fields. That doesn't mean there is a symmetry in classical electrodynamics between physical (position) space and Fourier space.
3. And anyway, Hiley, has shown how to obtain a Bohm approach in the momentum representation, thus solving the problem:

   '*Non-commutative quantum geometry: a reappraisal of the Bohm approach to quantum theory*', B.J. Hiley (2005).

6. Nonuniqueness of guidance equation

QM probability current can be 'gauged' by a divergence-free vector field: $\mathbf{j}' = \mathbf{j} + \mathbf{a}$ with $\nabla \mathbf{a} = 0$. Corresponding guidance equation $\mathbf{v}' = \mathbf{j}'/|\Psi|^2$ yields same statistical predictions but individual trajectories differ from standard pilot-wave ones. Thus problematic to regard Bohm trajectories as representing 'actual motion' of quantum particles?

Given Lorentz covariance fixes uniquely the current and the associated guidance law for spin-$\frac{1}{2}$ particles, Holland and Philippidis show that this uniquely fixes the guidance law in the nonrelativistic limit. Holland also looks at this question by considering dynamic implications of guiding wave suffering no 'back reaction' from the particle. See the following:

Implications of Lorentz covariance for the guidance equation in two-slit quantum interference P. Holland and C. Philippidis (2003)
Quantum back reaction and the particle law of motion P. Holland (2005).

7. Spin, nonlocality, and relativity

Spin

- All objections concerning spin turn out not to appreciate that in pilot-wave theory spin not property of particles (see discussion of Stern-Gerlach experiment in Lecture 1). Spin-based effects emerge naturally using spinorial wave functions. However, discussion of this in literature for examples other than SG (say with chemistry-type problems) very limited, and could be improved.
- What we do in computational electronic structure theory using our low-tech spin-zero Schrödinger theory is to introduce spin by the back door using spin orbitals and Slater determinants and all that. Spin/antisymmetry effects come entirely from the structure of nodal surface, and particles being repelled from nodes etc.

Nonlocality and relativity

- Can reasonable state nonlocality of pilot-wave theory more explicit (i.e. dynamical) than 'non-separability' of standard QM but this is a hair-splitting distinction. In both cases non-local correlations can't be used for superluminal signalling (except for nonequilibrium matter).
- Pilot-wave theory allows relativistic generalizations by either relaxing requirement of Lorentz invariance to apply only to observations, or by introducing extra theoretical structure (Lecture 5).
- In relativistic treatments like Bohm-Dirac theory, essential property of many-particle generalization is requirement for preferred reference frame (all particles have same time variable). But predictions agree with standard theory and preferred reference frame can be made unobservable.
- Additional structure restores fundamental Lorentz invariance for Bohm-Dirac theory. Counter example to claim that non-locality and Lorentz invariance in strict opposition. Theories not fully developed - research is ongoing - but so what, maybe there is a preferred frame.

8. Quantum field theory

Widespread suspicion that pilot-wave concepts cannot be sustained in realm of quantum field theories. Thorough discussion beyond the scope of this course but the suspicion is certainly incorrect.

- When generalizing to relativistic/QFT domain, must agree what is meant by 'Bohm-like' theory. Does it mean 'having (deterministic) trajectories'? Would like generalizations to (i) reproduce predictions of QFT, and (ii) include non-relativistic pilot-wave theory as limiting case (since no strict boundary between non-relativistic and relativistic physics). Also want clear *ontology* - attribution of 'being' to certain entities.
- Objectively existing things can be particles but can also be *fields* - quantum state then a *wavefunctional* which guides real field. Current best bet seems to be field ontology for bosons and particle ontology for fermions.
- Existing models have 'cooked up' flavour, but work for all practical purposes. Ambition of Bohm-like reformulations not to extend predictive power but to put on conceptually firm basis. While important questions remain open it seems premature to reject PWT for QFT-based reasons.

Additional provocative remarks

[See nice discussion in 'Quantum mechanics: myths and facts', H. Nikolic (2007).]

- Second quantization convenient mathematical trick - no new physical information. Is QFT really more fundamental theory than first-quantized QM of particles? If so should be able to reproduce all good results of less fundamental theory, e.g. in relativistic limit $\Psi^*\Psi$ should represent probability in space of particle positions. But this cannot be done. Instead of saying QFT solves problems of relativistic QM, more honest to say it merely sweeps them under the carpet.
- Is QFT a theory of particles? Elementary particle physicists would say particles actually more fundamental physical objects, while QFT is mathematical tool that describes - the particles.

9. Psycho-physical parallelism

Donald vs. Valentini, round 2

See MD problems on `www.bss.phy.cam.ac.uk/~mjd1014`.

MD: There are significant problems in understanding psycho-physical parallelism in [Bohm's] theory, in as far as it is not clear what role the hidden variables (the particle positions) should play. Particle trajectories are determined by the positions of other particles and by the guiding wave function, and the physical constitution of an observer could depend on facts about both. Indeed, it could even be that the particle positions are entirely irrelevant to psycho-physical parallelism and that, despite the existence of those definite positions, we are aware of a stochastic process of patterned structures in the global guiding wave (which is Everett's universal wave-function). The spirit of the Bohm interpretation, however, would seem to suggest that we are aware, at any moment, either of the positions of some family of particles associated with our brain (Bell 1981), or that we have just enough information about some such family to be able to assign them an effective local wavefunction (Albert 1992). And yet it is unclear which family of particles is involved, nor how that family is updated as the brain changes. Moreover, the information is of a type which seems quite remote from biologically significant information. In my opinion, this makes the Bohm interpretation an example of a theory in which the existence of consciousness, although possible, is hardly 'natural'.

AV: In pilot-wave theory, systems, apparatus and experimenters are 'built out of' the configuration $\mathbf{q}$ alone. If we were 'built out of' the guiding wave Ψ, then there would be other copies of us in different branches, and the theory reduces to many worlds (with a superfluous configuration appended to one of the branches). How exactly does a configuration Ψ map to a state of our consciousness? No one knows even classically. But certainly, there must be some coarse-graining. Moving a few nucleons around in my brain by a distance of a fraction of a Fermi won't change my conscious state, and nor will moving the deBB particle positions by a similar amount. People rarely mention this point, but I think that in pilot-wave theory there has to be an assumption of coarse-graining in the brain, otherwise we would be directly aware of nonlocality. (The only place I've seen this discussed is in my PhD thesis, pp. 25–26.) But to try to elaborate on this seems misplaced. We barely understand the brain or consciousness, and there's no special reason why we have to consider such details here, any more than we do when discussing eg Newtonian mechanics. Furthermore, there are more elementary questions that are still murky regarding the classical limit generally (eg for chaotic systems) in any version of quantum theory. So this all seems a bit unfair.

On the other hand, in what Matthew writes there is a separate thread of argument to the effect that, in deBB, perhaps one should think of ourselves as 'built out of Ψ' (or, perhaps out of both Ψ and the particles), in which case deBB is really a theory of many worlds. The argument has been put forward in particular by Deutsch, Zeh, Brown and Wallace. Deutsch says that pilot-wave theory is many worlds 'in denial'. [I have responded to this in a recent paper.]

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Parallel universe proof boosts time travel hopes

By Roger Highfield
Last Updated: 10:46AM BST 24 Sep 2007

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Science fiction looks closer to becoming science fact, reports Roger Highfield

Parallel universes really do exist, according to a mathematical discovery by Oxford scientists that sweeps away one of the key objections to the mind boggling and controversial idea.

According to quantum mechanics, unobserved particles are described by "wave functions" representing a set of multiple "probable" states. When an observer makes a measurement, the particle then settles down into one of these multiple options.

But the many worlds idea offers an alternative view. Dr Deutsch showed mathematically that the bush-like branching structure created by the universe splitting into parallel versions of itself can explain the probabilistic nature of quantum outcomes. This work was attacked but it has now had rigorous confirmation by David Wallace and Simon Saunders, also at Oxford.

"So," said Dr Deutsch, "the problems of probability, which were until recently considered the principal objection to the otherwise extremely elegant theory of Everett (which removes every element of mysticism and double-talk that have crept into quantum theory over the decades) have now turned into its principal selling point."

Quantum worlds without end

The multiverse according to Deutsch

- Many-worlds theories state all possible outcomes of every measurement somehow simultaneously 'realized' in different worlds which 'branch off' when wave function collapses. Thereafter they coexist as infinite multitude of parallel universes with just occasional quantum interference effects to signal their 'shadow reality'. Every universe equally 'real' to those living in it.
- Deutsch - like me, a sufferer from CBD (Confirmation Bias Disease) - explains 'absolute *necessity*' of adopting many-worlds as answer to QM paradoxes:

  "*We do not need deep theories to tell us that parallel universes exist - single-particle interference phenomena tell us that. What we need deep theories for is to explain and predict such phenomena: to tell us what the other universes are like, what laws they obey, how they affect one another..*"

  "*If the best theory available to physics did not refer to parallel universes, it would merely mean that we needed a better theory . . . in order to explain what we see.*"
- Seems utterly bizarre and counter-intuitive when judged by hitherto prevailing standards of realism and rational argument in the physical sciences - involves baroque proliferation of universes both astonishing and unprecedented in its sheer ontological extravagance (but proponents get very angry when people uses the word 'bizarre'). Deutsch says it is the *only theory* that can reasonably be upheld by anyone who accepts reality of quantum phenomena and who seeks a genuine explanation for them rather than a handy instrumentalist escape route.
- Instrumentalist Copenhagen QM is - he says - "*an idea for making it easier to evade the implications of quantum theory for the nature of reality*" - a point which seems fair enough.. The positivist belief that empirical adequacy plus a formalized proof procedure was the best any theory could properly aspire to is also bizarre.

See Deutsch's book - *The Fabric of Reality*. Note that other many-worlds variants exist - e.g. Saunders and Wallace's decoherence based approach.

Deutsch on pilot-wave theory

All right then - if parallel universes are the only way to understand single-electron interference - how does he dispose of the pilot-wave approach? It gets little more than a single paragraph in his book:

"Bohm's theory is often presented as a single-universe variant of quantum theory. But, according to Dr. Johnson's criterion, that is a mistake. Working out what Bohm's invisible wave will do requires the same computations as working out what trillions of shadow photons will do. Some parts of the wave describe us, the observers, detecting and reacting to the photons; other parts of the wave describe other versions of us, reacting to photons in different positions. Bohm's modest nomenclature - referring to most of reality as a 'wave' - does not change the fact that in his theory reality consists of large sets of complex entities, each of which can perceive other entities in its own set, but can only indirectly perceive entities in other sets. These sets of entities are, in other words, parallel universes."

Look how far Deutsch has to reinterpret Bohm to twist the theory into line with his fixed preconceptions. By '*Dr. Johnson's criterion*' ('I refute it thus!') he means 'kick-back' of other universe photons causing interference: "*if the complex motions of shadow photons . . . were mere possibilities that did not in fact take place, then the interference phenomena we see would not, in fact, take place*".

Nobody says Bohm avoids complexity of basic QM equations - pretext for saying just overly complicated version of many-worlds. Ignores Bohm's motivating premise - physical theories should describe/explain features of objective reality. Note translation of Bohm into Deutsch's idiom: 'entities perceiving other entities'. Assumes photons and 'shadows' exist across many universes interacting when such effects perceived to occur. Presupposes multiverse interpretation self-evidently true and all other theories which respect QM evidence bound to acknowledge that truth (against their own professed intent!).

- Deutsch very ingenious to represent pilot-wave theory as far-fetched metaphysical or speculative construct and many-worlds as matter of straightfoward inference from known phenomena!
- No need for bizarre excursions into realm of quantum hyperreality. PWT makes (in equilibrium) same predictions as many-worlds but gives more detailed, realistic, intuitively plausible, and explanatorily adequate account of observed quantum phenomena. Can't get away from that.

But Deutsch's idea that PWT is 'many-worlds in denial' merits further discussion. Next slide.

Many-worlds in denial?

Pilot-wave theory is really a many-worlds theory with a superfluous configuration attached to one of the worlds - it is not really a physically distinct formulation of QM.

"*The [pilot-wave] corpuscle's role is minimal indeed: it is in danger of being relegated to the role of a mere epiphenomenal 'pointer', irrelevantly picking out one of the many branches defined by decoherence, while the real story - dynamically and ontologically - is being told by the unfolding evolution of those branches. The 'empty wavepackets' in the configuration space which the corpuscles do not point at are none the worse for its absence: they still contain cells, dust motes, cats, people, wars and the like.*" [Brown and Wallace 2005]

Must evaluate PWT on its own terms without assumptions that make sense only in rival theories. It has its own subquantum theory of measurement, and *in general* describes nonequilibrium state violating Born rule. Note difference between ontological and mathematical structures e.g. family of trajectories in Hamilton-Jacobi theory - only one of them is real for a given system. String oscillation written as sum of modes - eigenfunctions and eigenvalues have mathematical significance only.

It is found that in realistic models of the classical limit, one does *not* obtain localized pieces of an ontological pilot wave following alternative macroscopic trajectories: 'empty packets' behave differently from packets containing the actual configuration. From a pilot-wave perspective, alternative trajectories are merely mathematical and not ontological, and many worlds are an illusion.

This is discussed in detail in A. Valentini, `quant-ph/0811.0810v2` (2008)

Many-worlds in trouble

Valentini's counter claim: *Many-worlds unlikely to be true, as it is ultimately motivated by puzzle of quantum superposition, which arises from a belief in eigenvalue realism, in turn based on intrisically unlikely assumption that quantum measurements should be modelled on classical measurements* .

Recall we say an experiment is 'a measurement of A' only because it formally resembles what *would have been* a correct measurement of A in a classical system. Thus system-apparatus interaction Hamiltonian $H = gAp_y \to \hat{H} = g\hat{A}\hat{p}_y$ (Lecture 4) and quantum 'measurements' in effect modelled on classical measurements. Pilot wave theory makes clear this is a mistake.

- Everett's initial motivation for introducing many-world was the puzzle of quantum superposition (in particular transfer of superposition from microscopic to macroscopic scales during measurement).
- Puzzle of superposition stems from 'eigenvalue realism': assignment of ontological status to eigenvalues of linear operators acting on Ψ. If $\Psi(x) = \sum_n c_n \phi_n(x)$ - superposition of eigenfunctions of $\hat{A}$ with eigenvalues a_n - then might appear as if all values a_n should be regarded as simultaneous attributes of single system. Ultimately due to belief that 'quantum measurement' is true measurement of observable A (due to formal resemblance to classical measurement).
- Assumption that classical physics provides reliable guide to measurement for nonclassical systems is intrinisically unlikely (we are outside domain of validity of classical theory) so subsequent conclusions - eigenvalue realism, superposition of properties, multiplicity of worlds - in turn intrinsically unlikely.

Einstein to Heisenberg: "*. . . it is quite wrong to try founding a theory on observable magnitudes alone. In reality the very opposite happens. It is the theory which decides what we can observe.*" Long complicated path underlying any observation, which runs from phenomenon, to production of events in our apparatus, and from there to the production of sense impressions. "*Along this whole path . . . we must be able to tell how nature functions . . . before we can claim to have observed anything at all. Only theory, that is, knowledge of natural laws, enables us to deduce the underlying phenomena from our sense impressions*" And finally "*. . . your theory will one day get you into hot water. When it comes to observation, you behave as if everything can be left as it was, that is, as if you could use the old descriptive language.*"

How to distinguish interpretations: the time operator

Time in relativistic and nonrelativistic quantum mechanics, H. Nikolic (2009)

- Problem of time operator in QM can be simply stated as follows: 'if the Hamiltonian $\hat{H}$ is bounded from below, there can be no Hermitian time operator $\hat{T}$ satisfying the canonical commutation relation $[\hat{T}, \hat{H}] = -i\hbar$'.
- In fact can define time operator as follows: act on wave function $\Psi(\mathbf{x}, t)$ by multiplying by parameter t (!) and enlarge Hilbert space of functions $f(\mathbf{x})$ to space of functions $f(\mathbf{x}, t)$. Quantity $|\Psi(\mathbf{x}, t)|^2 \, \mathrm{d}^3x \, \mathrm{d}t$ then naturally interpreted as probability that particle found in infinitesimal spacetime volume $\mathrm{d}^3x \, \mathrm{d}t$. However in standard QM eigenstates of the time operator cannot be constructed from physical solutions of the dynamical equations of motion.
- Nikolic points out this is not a problem for pilot-wave theory. Unlike standard QM, pilot-wave theory acceptable since we don't detect wave functions, but particles that move deterministically through spacetime. The spacetime position of a particle makes physical sense even without eigenstates of the time operator. Can measure time by measurements of 3-space positions of particles of the measuring apparatus - which also applied to measurement of time by a real clock.

New proposal for distinguishing interpretations

"In an acceptable interpretation the fact that time eigenstates are not physical should not imply that time itself is unmeasurable."

Quantum theory: interpretation cannot be avoided

> "*In recent years the debate on these ideas has reopened, and there are some who question what they call 'the Copenhagen interpretation' of quantum mechanics' - as if there existed more than one possible interpretation of quantum mechanics.*" **Rudolf Peierls** (1979)

First blow of combination that often constitutes entirety of what students are taught about quantum foundations. If any wicked student enquires, he is told 'interpretation is merely philosophical bias, and therefore no part of *physics*.' Peierls goes on to state Ψ represents knowledge of system i.e. central dynamical object refers exclusively to a human mind. Problem is the rather preposterous notion that this interpretation is uniquely unburdened by any prior philosophical world view.

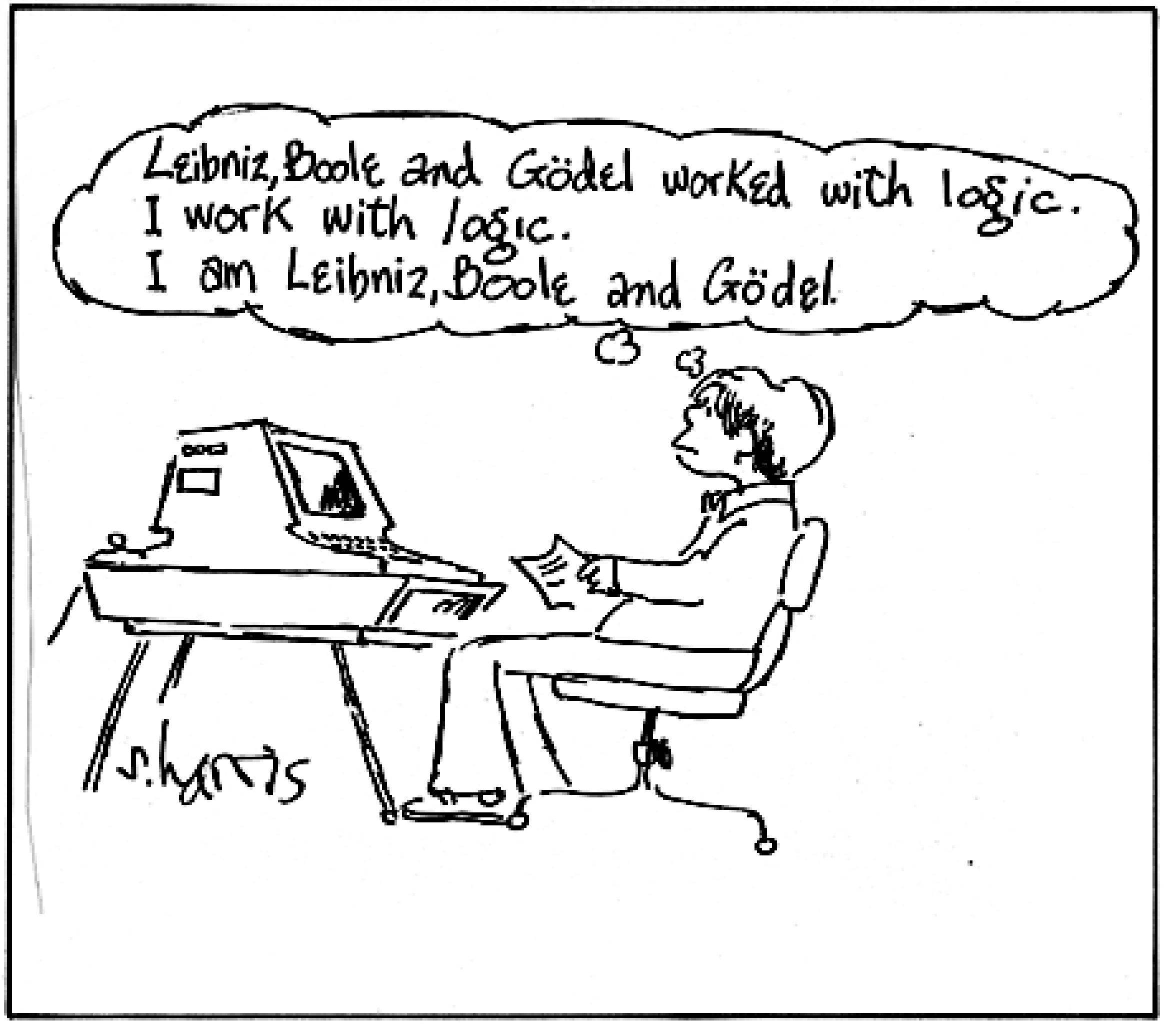
One often hears 'standard view perfectly consistent and free of extraneous metaphysical concepts other interpretations attach to theory. Encumbering QM with hidden variables, multiple worlds, or spontaneous collapse, without any improvement in its predictive power, only gives illusion of a better understanding'. This is just different, equally philosophical view involving unsupported metaphysical and fundamentally anti-scientific assumptions.

But standard QM not self-consistent due to measurement problem (Lecture 4). Solvable only by granting real physical existence to theory objects. Standard QM thus fundamentally anti-realist stance - wave function just about probabilities, but probabilities of what? Something does travel - of course - along different paths in interference experiment; to refuse to call it 'real' is merely to play with words.

Radical anti-realism can pretend to resolve interpretative paradoxes in virtually any context, e.g. Mach's rejection of grounding 'pressure' and 'temperature' in terms of real microscopic entities obviates need to understand, say, convergence to thermodynamic equilibrium. More broadly, philosophical doctrine of solipsism can 'solve' every problem in history of science by just denying that anything but one's own mental experiences exist. Ludicrously distant from kind of solution we are interested in as scientists.

Quantum Logic

Thus far I haven't mentioned the 'quantum logic' approach.
This is apparently how it works..



Rest of course

Lecture 1: 21st January 2009
An introduction to pilot wave theory

Lecture 2: 28th January 2009
Pilot waves and the classical limit. Derivation and justification of the theory

Lecture 3: 4th February 2009
Elementary wave mechanics and pilot waves, with nice examples

Lecture 4: 11th February 2009
The theory of measurement and the origin of randomness

Lecture 5: 18th February 2009
Nonlocality, relativistic spacetime, and quantum equilibrium

Lecture 6: 25th February 2009
Calculating things with quantum trajectories

Lecture 7: 4th March 2009
Not even wrong. Why does nobody like pilot-wave theory?

Lecture 8: 16th March 2009
Bohmian metaphysics : the implicate order and other arcana
Followed by a GENERAL DISCUSSION.

Slides/references on web site: `www.tcm.phy.cam.ac.uk/~mdt26/pilot_waves.html`