# The Doctrine of Classical Concepts in Historical Perspective

Conference for the History of Quantum Physics
Max Planck Institute for the History of Science, Berlin
29 June 2010

Kristian Camilleri

History and Philosophy of Science
University of Melbourne

# Why Classical Concepts?

You have repeatedly expressed you definite conviction that measurements must be described in terms of classical concepts… There must be clear and definite reasons which cause you repeatedly to declare that we must interpret observations in classical terms, according to their very nature… it must be among your firmest convictions – and I cannot understand what it is based upon.

Schrödinger to Bohr, 13 October 1935

# Bohr and the Indispensability of Classical Concepts

It lies in the nature of physical observation … that all experience *must* ultimately be expressed in terms of classical concepts.

N. Bohr, *Atomic theory and the description of nature* (1934)

The unambiguous interpretation of any measurement *must be* essentially framed in terms of classical physical theories, and we may say that in the sense the language of Newton and Maxwell will remain the language of physics for all time.

N. Bohr, 'Maxwell and modern theoretical physics' (1931)

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*. The argument is simply that by the word 'experiment' we refer to a situation where we can tell others what we have done and what we have learned and that, therefore, the account of the experimental arrangement and of the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics.

N. Bohr, 'Discussion with Einstein on epistemological problems in atomic physics' (1949)

# Outline

- Bohr's doctrine of classical concepts
- The Bohr-Heisenberg disagreement over the cut in 1935
- The evolution of the doctrine of classical concepts in the 1950s-60s

# Philosophical Interpretations

The alliance between Kantians and physicists was premature in Kant's time, and still is; in Bohr, we begin to perceive its possibility.
C. F. v. Weizsäcker, 1966

[Bohr's concept of complementarity] is the first example of a precise dialectical [materialist] scheme.
L. Rosenfeld, 1953

[Bohr's viewpoint] is entirely compatible with the formulations of logical empiricism.
P. Frank, 1947.

Traditional philosophy has accustomed us to regard language as something secondary and reality something primary. Bohr considered this attitude toward the relation between language and reality inappropriate.
A. Petersen, 1963

# Bohr and the Philosophy of Experiment

- Bohr as a *philosopher of experiment*
- Bohr's emphasis on the *functional*, as distinct from a *structural*, description of experiment
- The object-instrument divide
- The epistemological problem of quantum mechanics

# The Epistemological Problem

A still further revision of the problem of observation has since been made necessary by the discovery of the universal quantum of action… This circumstance, at first sight paradoxical, finds its elucidation in the recognition that it is no longer possible sharply to distinguish between the autonomous behavior of a physical object and its inevitable interaction with other bodies serving as measuring instruments, *the direct consideration of which is excluded by the very nature of the concept of observation itself*.

N. Bohr, 'Causality and complementarity', (1937)

We are faced here with an epistemological problem quite new in natural philosophy, where all description of experiences so far has been based on the assumption … *that it is possible to distinguish sharply between the behaviour of objects and the means of observation*.

[W]e are, therefore forced to examine more closely the question of *what kind of knowledge can be obtained concerning objects*. In this respect, we must on one hand, realize that the *aim of every physical experiment – to gain knowledge under reproducible and communicable conditions* – leaves us no choice but to use everyday concepts, perhaps refined by the terminology of classical physics, not only in accounts of the construction and manipulation of measuring instruments but also in the description of actual experimental results.

N. Bohr, 'Natural Philosophy and Human Cultures' (1937)

# Bohr's transcendental argument

- The object-instrument distinction is a condition of possibility for a device to serve its *epistemological purpose* as instrument, in spite of the fact that according to quantum mechanics, the instrument and object are in an entangled state.

- The *functioning* of the experimental apparatus is founded on the assumption that the interaction between an instrument and an object (even when making a measurement of a non-classical observable like spin) is a *causal* interaction taking place somewhere in *space* and *time* – essentially an exchange of energy/momentum in space-time.

- This is not a question about the nature of reality (ontology), but what are the conditions of possibility of experimental inquiry (epistemology).

# Challenges in the 1930s


- Laue, M. von. (1932). Zu den Erörterungen über Kausalität. *Naturwissenschaften*, **20**, 915–16.
- Laue, M. von. (1934). Über Heisenbergs Ungenauigzeitbeziehungen und ihre erkenntnistheoretische Bedeutung. *Naturwissenschaften*, **26**, 439–41.
- Schrödinger, E. (1934). Über die Unanwendbarkeit der Geometrie im Kleinen. *Naturwissenschaften*, **31**, 518–20.
- Einstein, A., Podolosky, B. and Rosen, N. (1935). Can a quantum-mechanical description of physical reality be considered complete? *Physical Review*, **47**, 777–80.

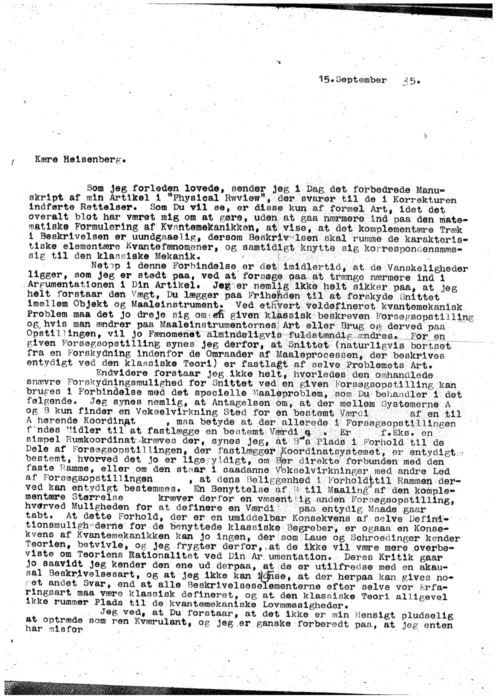
# Heisenberg and the Quantum-Classical Divide

[I]n a mathematical treatment of the process, a dividing line must be drawn between, on the one hand, the apparatus … and on the other hand, the physical system we wish to investigate. The latter we represent mathematically as a wave function. This function, according to quantum theory, consists of a differential equation which determines any future state from the present state of the function…The dividing line between the system to be observed and the measuring apparatus is immediately defined by the nature of the problem but it obviously *signifies no discontinuity of the physical process*. For this reason there must, within certain limits, exist complete freedom in choosing the position of the dividing line.

W. Heisenberg, 'Prinzipielle Fragen der modernen Physik' (1936)

# The Bohr-Heisenberg Correspondence

W. Heisenberg, *Ist eine deterministische Ergänzung der Quantenmechanik möglich?* (1935)



15.September 35.

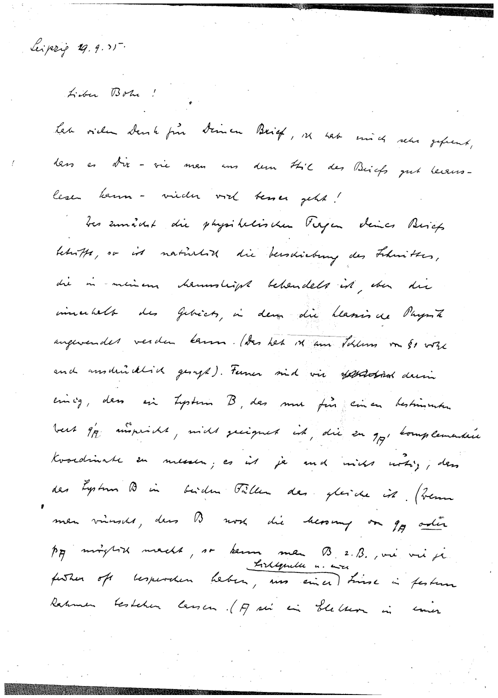
Kære Heisenberg.

Som jeg forleden lovede, sender jeg i Dag det forbedrede Manuskript af min Artikel i "Physical Rwview", der svarer til de i Korrekturen indførte Rettelser. Som Du vil se, er disse kun af formel Art, idet det overalt blot har været mig om at gøre, uden at gaa nærmere ind paa den matematiske Formulering af Kvantemekanikken, at vise, at det komplementære Træk i Beskrivelsen er uundgaaelig, dersom Beskrivelsen skal rumme de karakteristiske elementære Kvantefænomener, og samtidigt knytte sig korrespondensmæssig til den klassiske Mekanik.

Netop i denne Forbindelse er det imidlertid, at de Vanskeligheder ligger, som jeg er stødt paa, ved at forsøge paa at trænge nærmere ind i Argumentationen i Din Artikel. Jeg er nemlig ikke helt sikker paa, at jeg helt forstaar den Vægt, Du lægger paa Friheden til at forskyde Snittet imellem Objekt og Maaleinstrument. Ved ethvert veldefineret kvantemekanisk Problem maa det jo dreje sig om en given klassisk beskreven Forsøgsopstilling og hvis man ændrer paa Maaleinstrumenternes Art eller Brug og derved paa Opstillingen, vil jo Fænomenet almindeligvis fuldstændig ændres. For en given Forsøgsopstilling synes jeg derfor, at Snittet (naturligvis bortset fra en Forskydning indenfor de Omraader af Maaleprocessen, der beskrives entydigt ved den klassiske Teori) er fastlagt af selve Problemets Art.

Endvidere forstaar jeg ikke helt, hvorledes den omhandlede snævre Forskydningsmulighed for Snittet ved en given Forsøgsopstilling kan bruges i Forbindelse med det specielle Maaleproblem, som Du behandler i det følgende. Jeg synes nemlig, at Antagelsen om, at der mellem Systemerne A og B kun finder en Vekselvirkning Sted for en bestemt Værdi af en til A hørende Koordinat , maa betyde at der allerede i Forsøgsopstillingen findes Midler til at fastlægge en bestemt Værdi q . Er f.Eks. en simpel Rumkoordinat kræves der, synes jeg, at B's Plads i Forhold til de Dele af Forsøgsopstillingen, der fastlægger Koordinatsystemet, er entydigt bestemt, hvorved det jo er ligegyldigt, om Ber direkte forbunden med den faste Ramme, eller om den staar i saadanne Vekselvirkninger med andre Led af Forsøgsopstillingen , at dens Beliggenhed i Forhold til Rammen derved kan entydigt bestemmes. En Benyttelse af B til Maaling af den komplementære Størrelse kræver derfor en væsentlig anden Forsøgsopstilling, hvorved Muligheden for at definere en Værdi paa entydig Maade gaar tabt. At dette Forhold, der er en umiddelbar Konsekvens af selve Definitionsmulighederne for de benyttede klassiske Begreber, er ogsaa en Konsekvens af Kvantemekanikken kan jo ingen, der som Laue og Schroedinger kender Teorien, betvivle, og jeg frygter derfor, at de ikke vil være mere overbeviste om Teoriens Rationalitet ved Din Argumentation. Deres Kritik gaar jo saavidt jeg kender den ene ud derpaa, at de er utilfredse med en akausal Beskrivelsesart, og at jeg ikke kan indse, at der herpaa kan gives noget andet Svar, end at alle Beskrivelseselementerne efter selve vor Erfaringsart maa være klassisk defineret, og at den klassiske Teori alligevel ikke rummer Plads til de kvantemekaniske Lovmæssigheder.

Jeg ved, at Du forstaar, at det ikke er min Hensigt pludselig at optræde som ren Kværulant, og jeg er ganske forberedt paa, at jeg enten har misfor



Leipzig 29.9.35.

Lieber Bohr!

Hab vielen Dank für Deinen Brief, ich hab mich sehr gefreut, dass es Dir – wie man aus dem Stil des Briefs gut herauslesen kann – wieder viel besser geht!

Was zunächst die physikalischen Fragen Deines Briefs betrifft, so ist natürlich die Verschiebung des Schnittes, die in meinem Manuskript behandelt ist, eben die innerhalb des Gebiets, in dem die klassische Physik angewendet werden kann. (Das hab ich am Schluss von §1 wohl auch ausdrücklich gesagt). Ferner sind wir ~~[struck out]~~ darin einig, dass ein System B, das nur für einen bestimmten Wert $q_A$ anspricht, nicht geeignet ist, die zu $q_A$ komplementäre Koordinate zu messen; es ist ja auch nicht nötig, dass das System B in beiden Fällen das gleiche ist. (Wenn man wünscht, dass B noch die Messung von $q_A$ oder $p_A$ möglich macht, so kann man B z.B., wie wir ja früher oft besprochen haben, aus einer Lichtquelle u. einer Linse in festem Rahmen bestehen lassen. (A sei ein Elektron in einer

# The Bohr-Heisenberg Correspondence

I have experienced difficulties in trying to understand more clearly the argumentation in your article. For I am not quite sure that I fully understand the importance you attach to the freedom of shifting the cut between the *object* and the *measuring apparatus*. Any well-defined quantum-mechanical problem must be concerned with certain classically described experimental setting, and if one changes the kind or use of the measuring instruments, and thus the [experimental] setting, the phenomenon will always change completely. *I therefore believe that for a given experimental setting the cut is determined by the nature of the problem*.

Bohr to Heisenberg, 15 September 1935

# The Enduring Disagreement

- Bohr has emphasized that it is more realistic to state that the division into the *object* and *rest of the world* is not arbitrary [but is determined by the very nature of the experiment].

  W. Heisenberg, *Physics and Philosophy* (1958)

- [Bohr and I could not agree on] whether the cut between that part of the experiment which should be described in *classical terms* and the *other quantum-theoretical* part had a well defined position or not… I argued that a cut could be moved around to some extent while Bohr preferred to think that the position is uniquely defined in every experiment.

  Heisenberg to Heelan, 1975

# Attempts at a *Physical* Reformulation 1950s–60s

The historical context – a new wave of criticisms and misunderstandings:

- Bohm's hidden variables theory (1952)
- De Broglie's reconversion to a causal theory program (1953)
- Everett relative-state interpretation (1957)
- The Soviet critique of the Copenhagen interpretation (1950s)
- Wigner's subjectivist reading of von Neumann (1961-3)

The thermodynamic program – condition of irreversibility

- Ludwig (1955)
- The Copenhagen school's exchange with Wheeler and Everett (1957-8)
- Prosperi, Loinger and Saleri (1962)
- Rosenfeld (1965)

It is understandable that in order to exhibit more directly the link between the physical concepts and their mathematical representation, a more formal rendering of Bohr's argument should be attempted.
L. Rosenfeld, 'The Measuring Process in Quantum Mechanics' (1965)

# Weizsäcker on the Quantum-Classical Transition

Having thus accepted the falsity of classical physics, taken literally, we must ask how it can be explained as an essentially good approximation. This amounts to asking *what physical condition must be imposed on a quantum-theoretical system in order that it should show the features which we describe as 'classical'*. My hypothesis is that this is precisely the condition that it should be suitable as a measuring instrument. If we ask what that presupposes, a minimum condition seems to be that irreversible processes should take place in the system… I am unable to prove mathematically that the condition of irreversibility would suffice to define a classical approximation, but I feel confident it is a necessary condition.

C. F. v. Weizsäcker, 'The Copenhagen Interpretation' (1971)

# Heisenberg on the Quantum-Classical Transition

Even in large dimensions there are many solutions of the quantum-mechanical equations to which no analogous solutions can be found in classical physics. In these solutions the phenomenon of the 'interference of probabilities' would show up… Therefore, *even in the limit of large dimensions* the correlation between the mathematical symbols [of quantum mechanics] … and the ordinary [classical] concepts *is by no means trivial.* In order to get at such an unambiguous correlation *one must take another feature of the problem into account*. It must be observed that the system, which is treated by the methods of quantum mechanics is in fact a *part of a much bigger system (eventually the whole world*)… The interaction with the bigger system with its undefined microscopic properties then introduces a new statistical element into the description … of the system under consideration. In the limiting case of the large dimensions this statistical element destroys the effects of the 'interference of probabilities' in such a manner that the quantum-mechanical scheme really approaches the classical one in the limit.

W. Heisenberg, *Physics and Philosophy* (1958)

# Wheeler, Zurek and the Origins of Decoherence?

The role of decoherence is to establish a boundary between quantum and classical. The boundary is in principle moveable, but in practice largely immobilized by the irreversibility of the process of decoherence… The equivalence between 'macroscopic' and 'classical' is then validated by the decoherence considerations, but only as a consequence of the practical impossibility of keeping objects which are macroscopic perfectly isolated.

W. Zurek, 'Preferred States, Predictability, Classicality and the Environment-Induced decoherence' (1993)

Wheeler was absolutely essential in defining the problem, or rather, the whole set of problems [in the1970s and 80s which led to the development of decoherence].

W. Zurek, personal communication, 2008.

Zurek developed his term paper for Wheeler's seminar on the two-slit experiment:
W. Wooters & W. Zurek, 'Complementarity in the double-slit experiment: quantum nonseparability and a quantitative statement of Bohr's principle' (1979)