POPPER'S EXPERIMENT AND THE INTERPRETATION OF QUANTUM MECHANICS

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ABSTRACT

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Popper invented a thought experiment that is alleged to test the *Copenhagen interpretation* of Quantum Mechanics, though not the theory itself. In particular it is alleged to test the application of the Heisenberg uncertainty principle to indirect measurements. The experiment is similar to the EPR thought experiment, but it suffers from some technical flaws. These flaws are examined to see if they affect the validity of the argument; so long as they can be alleviated by technical means, the argument retains its force. The experiment has been recently instantiated, with some resolutions of these technical flaws. The results, on face value seem to vindicate Popper and to indicate a violation of the uncertainty principle. Nevertheless, Kim and Shih, who carried out the experiment, give their own interpretation that suggests that the principle should be considered intact but that the correct way to understand the results is to adopt a new metaphysics. Others suggest different reasons why the results do not amount to a violation of the uncertainty principle. Despite a variety of responses, which are carefully examined, the general point that an interpretation cannot alter the empirical basis of a theory is shown to be true. The importance of Rigolid's work in this connection is emphasized.

CONTENTS

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Chapter 0: Introduction

Quantum Mechanics has been a theory of unique interest to the philosophy of physics and of science. Its most popular interpretation, the Copenhagen interpretation, is usually taken as having a strong positivistic background, and even as a fundamental theory it has been argued to be anti-realistic. It has given rise to paradoxes that do not yet seem to have found their resolutions, despite many decades of work by scientists and philosophers.

The strong positivistic background, together with the paradoxes that it has given rise to, has made a number of scientists and philosophers object to the Copenhagen interpretation. Einstein provided one of the main objections in the form of the EPR thought experiment. This thought experiment led to important theoretical progress and understanding concerning the foundations of the subject, and eventually to an experimental investigation of the 'Bell inequalities' that follow from a form of the experiment. The experiments seem to be in favour of the standard interpretation and against Einstein's 'realism'.

More recently, in the early eighties, Popper, a metaphysical realist, provided his own thought experiment that is similar to the EPR experiment. In the mind of its inventor, the experiment aims to refute the Copenhagen interpretation and to salvage realism; that is, Popper saw it as a potentially crucial experiment between the orthodox Copenhagen interpretation of quantum mechanics and a 'realist' interpretation. The proposal for the experiment, having been devised by a philosopher, rather than a physicist, perhaps unsurprisingly, had some technical flaws in its original form. Nevertheless, almost twenty years after its original proposal, it has found its way to the physicists' lab. The results of the experiment are, on the face of it, quite surprising—because they seem to suggest a violation of the uncertainty principle. A number of attempts have appeared in the literature purporting to explain these results.

The chief focus of this thesis is the investigation of the experiment and its ability to test the interpretation, rather than the quantum theory itself. Most of the previous examinations of Popper's experiment dismiss it too easily, at the first sign of weakness. The intention behind this thesis is to examine Popper's position by giving it the best possible defence while at the same time remaining critical. That is, the intention is to first try and separate out the elements of his philosophy that, while being problematic, do not affect the validity and force of his argument. Then, to carry out an examination of his proposal of the experiment will be carried out and, having identified any problematic points, investigate which ones can be ironed out by granting appropriate concessions to Popper, and which, if any, should be considered fatal for the argument. Only after such a defence has been provided, will the results of the instantiation of the experiment be examined to see if they bear on the issue that Popper wants to investigate.

The analysis of the experiment, its concretisation by Kim and Shih, together with a critical assessment of their experiment will be given in chapters 3 to 8. However, I begin by setting this central issue within a more general context. Chapter 1 gives a very brief and superficial account of Poppers philosophy of science and an outline of Popper's views on quantum mechanics; this, as we shall see, calls for a major clarification of what Popper means by realism in general and by a realist interpretation of quantum mechanics in particular—and this is undertaken in Chapter 2. I shall argue that, while Popper sees the two as continuous, there is in fact an important disconnection between his general views on scientific realism and the particular 'realism' about quantum mechanics that he endorses.

The third chapter looks at the original proposal and provides a detailed analysis of its possible outcomes. It also provides an analysis of the technical problems that are present in the original proposal, together with an account of how these might be overcome. The fourth and fifth chapters examine the reactions that appeared in the philosophical and physics literature respectively, before the instantiation of the experiment.

The instantiation of the experiment is based on a process called Spontaneous Parametric Down Conversion. These processes are governed by the physics of Non-Linear Quantum Optics, and the sixth chapter provides a summary of the relevant issues. The experiment was performed by Kim and Shih and the seventh chapter examines their report and their interpretation of their results, whereas the eighth chapter examines the various attempts appearing in the literature for the interpretation of those results. The final chapter provides a summary of the conclusions arrived at in the thesis. Particular attention is paid to a neglected work of Rigolin's dealing on the experiment.

Chapter 1: Popper's Philosophy, Realism, and Quantum Mechanics

Quantum mechanics is thought of as a theory that is not readily interpreted realistically. Popper was strongly opposed to this idea: "The *realistic view of the world*, together with the idea of approximation to the *truth*, seem to me indispensable for an understanding of the perpetually *idealising* character of science... The realism dispute is highly topical in quantum mechanics... I am totally on the side of *realism*".[1] Section 1.1 first sets out the basic tenets of quantum mechanics and of realism, and the reasons why they are not easily reconcilable. Two different senses of realism are distinguished, and the extent to which quantum mechanics is in disagreement with each one is examined. Then section 1.1 briefly discusses some of the problems that scientists met in trying to 'interpret' the theory. Section 1.2 gives a brief review of Popper's philosophy, and 1.3 looks at how realism acted as the motivation for his experiment. Popper's ideas about quantum mechanics are given in the section 1.4. In order to contrast his ideas with those he attacks, the Copenhagen interpretation, together with some of the reactions to it, are examined in the section 1.5. This chapter sets the stage for examining the criticism that was raised regarding Popper's ideas concerning the quantum theory; this will be the subject of the second chapter.

[1] Popper (1999: 21–2); his emphasis.

1.1. Realism and Quantum Mechanics

Throughout the history of science, many crucial discoveries in the mathematical and physical sciences have involved decisive breaks from what had been received common sense and the received wisdom. For example, during the Middle Ages, Aristotelian science reigned supreme. Aristotle held that two falling bodies would fall with different speeds, proportional to their weight and that the natural state of an earthy body was to be stationary at the centre of the earth. For Galileo, on the other hand, any two bodies, whatever their masses, would fall with the same speed (ignoring air resistance) and the natural state of an earthy body was to continue in uniform motion unless acted upon by a force.

In Galileo's *Two New Sciences*[2] the physical concept of *inertia* is formulated and used, and his representative, Salviati, is at great pains to explain to Sagredo and the Aristotelian Simplicio how two balls, one weighing two hundred pounds and the other only half a pound, will reach the ground almost at the same time if dropped from a height of 200 cubits.[3] In other words, what would seem natural to the earlier scientist was not 'natural' at all to the later one. It should not be surprising then that the quantum revolution has forced a shift in our intuitions and our framework. But all the previous shifts were initially resisted only later to become part of our 'common-sense' knowledge and to form the basis from which a still later shift was to take place. The quantum shift however is arguably unique in that it has sparked deep and widespread disagreements that even today, more than a century after the theory's first articulation, are still debated without definite signs of settlement.

The basic scientific realist assumption holds that the world is independent of our exploring activities and science has proved a successful way to explore

[2] Galilei (1954)

[3] (*Ibid.*: 62–7)

it. That is, our best scientific theories give us an at least approximately true account not just of the phenomena but of the 'deep structure' of the universe underlying the phenomena. For the purposes of this discussion, this basic realist thesis will be called 'realism1'. The main consideration underpinning 'realism1' is the impressive empirical success—in particular predictive success—of accepted theories in mature science. How, the realist asks, short of a miracle, could theories make stunning predictions if theories were not at least approximately true? This is the main idea behind the 'no miracle argument', and Putnam's slogan, 'Scientific realism is the only philosophy of science that that does not make the success of science a miracle'. As Psillos puts it: "Modern defenders of scientific realism have based their defence of the idea that the impressive predictive and explanatory successes of scientific theories would remain unaccounted for, unless we accept that the entities, processes and causal mechanisms they posit to operate behind the phenomena are real".[4]

The quantum theory has undoubtedly been staggeringly empirically successful. For example, the experimental investigation of the Lamb shift has yielded a high precision verification of the theoretical calculations provided by the quantum theory of electrodynamics. The theoretical calculation of the electron g-factor in the Lamb shift, gave a value that was in agreement within twelve orders of magnitude with the measured value for the electron spin g-factor.

'Realism1' about quantum theory would, then, simply hold that quantum theory is an approximately true description of reality, claiming that physical systems have quantum states associated with them and that these evolve in accordance with Schrödinger's equation, except when a 'measurement' is made. 'Realism1' can remain neutral with respect to any particular metaphysical 'interpretation' of

[4] Psillos (1999: xxii)

the theory. However, it is well known that there has been an enormous amount of dispute about how to interpret the quantum theory. And in the course of this dispute, a particular type of interpretation has come to be associated with the term 'realism'. Since this realism certainly goes beyond 'realism1' about quantum theory, I shall distinguish it as 'realism2'.

In order to count as a 'realist2' construal of quantum theory, an interpretation must assign micro-entities, like electrons, photons, etc., attributes, like position and momenta, of which they always possess definite, 'sharp' values. Moreover, these values are not subject to influences due to measurements performed on other entities at spatio-temporal locations that are distant enough not be reached via signals travelling at the speed of light. So 'realism2' is committed to retaining two metaphysical features—all systems have definite space-time trajectories and all actions are 'local'.

Opponents of 'realism1' take various precise positions. The most straightforward is classical instrumentalism which holds that one should not venture beyond the empirically verified predictions of a successful theory; we should not think of the theory as telling us anything about the universe's 'deep structure' of the physical situations described by the theory. Thus the Church tried to deny the truth of the Copernican theory which asserted that the earth rotates around a central sun. This claim was allegedly inconsistent with Holy Scriptures, which teach that the earth is immovable, and the Church tried to assert that the theory should be regarded as no more than a calculating device[5].

One can of course adopt an instrumentalist view of *any* scientific theory. This is just what instrumentalists advocate. However, many scientists and philosophers, otherwise antithetical to instrumentalism, believe that there are very spe-

[5] Popper discusses the use of instrumentalism by the Church in his (1982*b*: 102) for example. See also Holton and Brush (2001: 23-25).

cial difficulties in adopting any sort of 'realist2' interpretation of quantum mechanics.

The origins of quantum mechanics can be found in the early part of the twentieth century when a number of experiments indicated some phenomena that could not be understood within the classical framework: Black-body radiation, the photoelectric and the Compton effect among others indicated that energy is quantized; it comes in discrete units. Light and electromagnetic radiation in general display particle-like characteristics. Further, atomic spectroscopy and the stability of atoms indicated that particles such as electrons produce wave-like interference patterns.[6] In response to these findings, over the first quarter of the century, physicists developed a new mechanics that could accommodate them: Quantum mechanics.

But why should it be held that there is a particular tension between realism and the quantum theory? One major problem lies in the attempts to interpret the theory. The theory seems to state what will happen to any system of any number of particles. But the application of the theory also requires the addition of a measuring apparatus, or an observer, somehow 'outside' of the system. Bohr, for example, in his interpretation of the theory, considers the properties of the system to have meaning and existence only in relation to particular apparatuses or experimental arrangements. Moreover this apparatus is described using terms from a theory that is thought to be incompatible with quantum theory—classical physics. But this incompatibility is not the only problem. The role of measurement makes it hard to imagine how the theory could be applied to the whole universe, as, for example, Newton's theory was standardly considered to be. Not only is the world-in-itself beyond the power of our mind to picture, as Kant would

[6] See the discussion by Planck (1925), one of the founders of the theory.

argue, but, according to Bohr's complementarity view, even the empirical world cannot be described with just one picture—instead, according to Bohr, we have to use different pictures for different situations.

Quantum theory describes any dynamical system using the notion of a state. The state of a system evolves in time according to the Schrödinger equation. This may mean that it evolves from one given state into another that is the superposition of two states representing the two observable alternatives—for example the transition from an non-decayed atom to the superposition of a decayed and a non-decayed atom. In the case of a classical stochastic theory the transition to either of the two states would be reflected by a stochastic element in the equation itself and would in effect represent our ignorance of the underlying dynamics. But here it is reflected in the fact that the new state is the superposition of two states.

The peculiar features of superpositions as compared to the classical description can be shown with the help of the property of spin. A measurement of the spin of an electron, say, in a specific direction will always produce one of two results: $+1$ or -1, otherwise called spin-up or spin-down. If a collection of electrons is prepared so that each would display spin-up in a measurement of spin in some specific direction x, then the theory represents them by abstract vectors $|x+\rangle$, in an abstract mathematical space called Hilbert space. If a measurement of spin is then performed in a direction z at right-angles to x, then the rules of quantum theory can be used to write the vector $|x+\rangle$ as a superposition of the z-spin states:

$$|x+\rangle = \frac{1}{\sqrt{2}}|z+\rangle + \frac{1}{\sqrt{2}}|z-\rangle,$$

where the squares of the numerical coefficients give the probabilities of observing spin-up or -down in the z-direction in a given measurement. This is importantly different from the situation where we have mixed a number of electrons that have spin-up in the z-direction and an equal number of electrons that have spin-down in the z-direction. The difference between superpositions and mixtures is the following: A measurement in the above state of the spin in the x-direction has probability 1 to yield the result $+1$ and 0 to yield the result -1, and a measurement in the z-direction has probability $1/2$ to yield the result $+1$ and $1/2$ to yield the result -1. But if a collection of electrons is prepared so that half would display spin-up and half would display spin-down in measurement of spin in the z-direction, then a measurement in *any* direction has probability $1/2$ to yield the result $+1$ and $1/2$ to yield the result -1. That is, in the case of superpositions, the probabilities depend on the direction we choose to measure, whereas in the case of mixtures they do not. More generally, according to the theory quantum states, s, are represented by vectors of unit length, $|s\rangle$, in a Hilbert space, $\mathcal{H}$ (a linear vector space over the complex numbers). Knowledge of the state $|s\rangle$ allows the prediction of probabilities for the results of measuring a quantity A for a system in that state. This is achieved by decomposing the state $|s\rangle$ into a sum of orthogonal vectors

$$|s\rangle = c_1|a_1\rangle + c_2|a_2\rangle + \cdots \quad \text{with} \sum_i |c_i|^2 = 1$$

The measurable values a_i are the eigenvalues of A, and the corresponding vectors $|a_i\rangle$ are the eigenvectors or eigenstates of A. If the system happens to be in one of the eigenstates of A, say $|a_n\rangle$, then a measurement of A is certain to give the result a_n. But if the system is in a superposition, like $|s\rangle$, then Born's Rule tells us that the probability of observing a_n in any given measurement is given by the square of the modulus of the complex amplitude c_n, $|c_n|^2$. Conventionally we say that if a system is in a superposition of eigenstates of some observable, then that observable has no definite value, whereas if the system is in one of the eigenstates, then the observable has a definite value. This is often called the eigenvalue-eigenstate link.

The fact that a system is in a superposition of eigenstates of some observable has no definite value for that observable forces the conclusion that, unlike in the picture suggested by the classical theory, the world is indefinite in a peculiar way: things may not always have well-defined positions or momenta. This contrasts with the specific realist position given above, 'realism2', holding that quantum systems must not be unlike macroscopic objects that do have well-defined positions or momenta. This prompted opponents of the orthodox interpretation to say that a system in a superposition state must in fact be either in the one or in the other state; and hence that the description that the quantum theory provides must be incomplete.

In response to this incompleteness charge, the defenders of the orthodox interpretation claim that the system is in a superposition as long as no measurement is performed on it; a measurement makes it collapse into one of the two states in a stochastic manner. That is, the defenders of orthodoxy denied the position of 'realism2': When a measurement of momentum is performed on a system that yields a sharp value, the system immediately after the momentum measurement is in an eigenstate of momentum and possesses that sharp value. According to the theory, the system that is in a momentum eigenstate is inevitably in a superposition of position eigenstates. But according to the eigenvalue-eigenstate link, this means that immediately after the momentum measurement the system does *not* possess a position. Accordingly, the defender of 'realism2' has to deny the eigenvalue-eigenstate link and to interpret the theory and the results of the experiments in a way that does not require that systems possess values for their attributes only when these are measured.

However, although orthodoxy contradicts 'realism2', 'realism1' can live peacefully with the theory: If quantum theory is our best theory for the micro-world, then what it says about that world is approximately true, not only of the quantum phenomena but also of the 'deep nature' of the micro-world underlying these phenomena. That is, the advocate of 'realism1' holds that any micro-system that evolves independently, does so according to the Schrödinger equation, and when a measurement is performed on it, 'collapses' into an eigenstate of the measured attribute, as long as measurement is understood as an interaction with a macroscopic apparatus and does not depend on an interaction with a conscious observer.

Albert Einstein disagreed with the orthodox view and adopted a position that is realist in the sense of 'realism2'. In a paper with Podolski and Rosen,[7] he gave the following criterion for the existence of physical reality: "If without in any way disturbing a system we can predict with certainty... the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity."[8] This criterion characterizes physical reality in terms of objectivity, meaning its independence from any direct measurement. The implication here is that a direct measurement of physical reality reflects, but does not actively create, that which is observed.

Correspondingly, the opponents of the orthodox view have tried to complete the description of the physical world by adding so-called 'hidden variables'. This approach would both alleviate the clash with realist2 intuition and reveal quantum theory as not being the ultimate theory of nature. The problem with such attempts is that, as Bell has shown,[9] they require the postulation of a mysterious, non-local action-at-a-distance.

Various experimental tests (like the Aspect experiment[10]) confirm the quantum predictions to a high degree and hence violate the Bell inequalities. Although

[7] Einstein, Podolski, and Rosen, (1935)

[8] (*Ibid.*: 778)

[9] Bell (1964)

[10] Aspect (1976)

these tests could, and have been questioned (on the grounds of experimental inefficiencies one encounters in the quantum world), to the extent that they are reliable, they raise doubts about any attempt to completely reject the eigenvalue-eigenstate link. Indeed they suggest that the assumptions made by EPR that lie behind the Bell inequalities are incompatible with quantum theory and with experience, as this is expressed by the results of experiment.

The assumption which is central both to the derivation of the Bell inequalities and to the EPR argument is the principle of no-action-at-a-distance, otherwise called 'locality', telling us roughly that the outcomes in one part of the experiment cannot immediately be affected by measurements performed in another part with a spatial separation from the first. Hence, the Bell theorem is thought of as showing that locality is incompatible with quantum theory, and the experimental tests that show the violation of the equalities, as demonstrating that, either nature is non-local, or that the eigenvalue-eigenstate link should be retained.

In the context of quantum physics, then, the realism-antirealism (realism in the sense of 'realism2') dispute concerns the physical reality of quantum objects and the degrees to which we can acquire exact experimental knowledge concerning various parameters of those objects. The problems concerning our knowledge of those objects arise from the fact that quantum theory has been interpreted as imposing limits on the exact simultaneous measurement of values of parameters such as the spins along two orthogonal directions. Furthermore, various paradoxes arise from a tension between the orthodox interpretation of quantum theory and the local realism suggested by Einstein's theory of relativity. Given the results of the Aspect experiments, it seems that the impossibility of a faster-than-the-speed-of-light causal interaction between two objects has to be abandoned in favour of an interaction that seems instantaneous irrespective of their distance apart.

This realism-antirealism dispute referred to in the preceding paragraph is the issue that Popper wants to probe by proposing his experiment. But can one probe a metaphysical issue using an experiment? Redhead poses this very question and gives a very clear statement of the issues at stake in his (1995*b*):

> Is it possible to bring experiments to bear on metaphysical theses such as realism? In recent developments in quantum mechanics particularly associated with the late John Bell it has been claimed that a certain type of realism, what is known as local realism, has indeed been refuted by a series of experiments, culminating in those carried out by Alain Aspect and his collaborators in Paris in 1982.
> Crudely local realism means in this context that atomic and subatomic entities, the subject matter of quantum mechanics, possess definite sharp values for all their attributes at all times, and, in addition, these attributes cannot be affected instantaneously by operations such as measurements performed on other microentities at different spatial locations from the one whose attributes are in question. In other words, realism is supplemented with a denial of the possibility of instantaneous action-at-a-distance. Now relativity theory, the other great pillar of modern theoretical physics, is standardly understood as committed to such denial. So an experimental refutation of local realism would force us to give up either locality (the no-instantaneous-action-at-a-distance principle) or the realist thesis itself. The first horn of this dilemma is closed off by appeal to relativity theory, and so the conclusion of the argument would appear to be the refutation of realism.
> Notice, however, that we are using realism here in rather a different sense from heretofore, i.e. we are not claiming to prove that the external world is dependent on human consciousness or subjective experience, but rather that attributes of microentities don't exist in all cases independently of the measurements which manifest them, but, ... measurement procedures need have nothing to do with sentient observers.[11]

There are a couple of points that need to be noted from this quotation. First, the quotation captures the essence of the question examined by the Popper experiment: That of whether "atomic and subatomic entities, the subject matter of quantum mechanics, possess definite sharp values for all their attributes at all times, and, in addition, that these attributes cannot be affected instantaneously by operations such as measurements performed on other microentities at different spatial locations from the one whose attributes are in question".[12]

Second, the quotation gives a preliminary answer to the question that it poses at its beginning: Can an experiment bear on a metaphysical issue and in particular

[11] Redhead (1995*b*: 41–2)
[12] (*ibid.*)

can it bear on the specific issue of local realism? The answer that is suggested in the quotation is 'Yes', and this is because, as Redhead points out, this particular type of realism seems to be refuted by the series of experiments that he refers to. If it has indeed been refuted by those experiments, or if it is taken as being refuted, then the assumption underlying those experiments must be that the issue of local realism *was* the issue probed by them. If the answer to the question is not positive, then the countless discussions surrounding the EPR argument that accept the claim that those experiments have refuted local realism, are operating on the wrong assumption. This question is also examined in the next chapter.

This thesis is precisely an examination of whether, and to what degree, the experiment succeeds, in principal and in practice, in probing this metaphysical issue. Specifically, Popper denies that the only way to interpret quantum theory and the results of the experiments that are in agreement with it, is the one given by Bohr; i.e., the complementarity of the classical descriptions one can give depending on the measuring situation. Popper thinks that surely: 'if the experiment turns out in one particular way, then the outcome would show that one can ascribe two complementary descriptions at the same time'.

In the various discussions that have appeared in the literature concerning Popper's experiment other issues have been raised, such as, for example, the adequacy of Popper's account of quantum theory. While these issues are interesting and are to some degree examined here, they are peripheral to the central issue indicated above. The experiment could have been proposed by anyone who does not share Popper's views. If it can in principle be performed and probe the intended issue, then it is worth performing it and having its results examined independently of any other consideration. To take one example, Popper's preferred interpretation of quantum mechanics relies heavily on his propensity interpretation of probability, but whether or not this interpretation—in Popper's version or some other—is tenable or not,[13] has no bearing on the argument given by Popper in his proposal for the experiment.

The issue that Popper wants to probe is a very important one, and because of this it is worthy of examination without any prejudice that might come from one's views about Popper's other ideas, about science in general and quantum mechanics in particular. The intention behind this thesis is to examine the experiment critically, while at the same time granting some assumptions to Popper, that are necessary for the argument to go through, and trying to alleviate any possible problems that can be dealt with without affecting the spirit of his argument.

It is in this vein that the actual instantiation of the experiment by Kim and Shih (discussed later) was performed with photons rather than with Popper's proposed massive particles—a suggestion that clearly would not work.

Similarly, whether Popper's general philosophy of science or his ideas about realism are problematic or not, has little bearing on his specific argument involving the proposed experiment. His proposal is meant as an attempted refutation of Bohr's complementarity thesis and the validity of Popper's views that act as the motivation behind Popper's proposal has only a peripheral role to play in this discussion. Nevertheless, given that such a proposal could never have come out of a vacuum, these issues will be discussed in this and the next chapter in order to give the context in which the experiment was proposed. The idea is to give a very general view of the problem background out of which Popper's proposal came and not to give any detailed scholarly analysis of Popper's general position.

[13] For example, an influential argument against propensities has been given by Humphreys, in his(1985), and is known as 'Humphreys' Paradox'; this will be examined in the second chapter.

1.2. Popper's Philosophy

Karl Popper is thought of as one of the greatest philosophers of science of the previous century, but his philosophy had also considerable social and political dimensions. In fact one could argue that his political and social philosophy underlies his philosophy of science. He described himself as a 'critical-rationalist', and as a dedicated opponent of all forms of scepticism, conventionalism, and relativism in science. He was an advocate of Enlightenment and modernity as can be seen by his aim to provide a theory of critical reason, his advocacy of personal moral responsibility in an 'Open Society', and his severe criticism of totalitarianism in all of its forms. Throughout his work one can find the general theme of the importance of philosophy in the solving of practical problems of the world, and his epistemology has political import.

Popper started his career in philosophy as a philosopher of science, but his philosophy expanded to reach a much broader scope. He made contributions in diverse areas, from Presocratic studies to modern logic, from politics to probability, and from the mind-body problem to the interpretation of quantum theory. He published three major works between 1935 and 1945: The *Logik der Forschung* (1935, appeared in English as *The Logic of Scientific Discovery* in 1959); *The Poverty of Historicism* (1957, first appeared in 1944–5); and *The Open Society and Its Enemies* (1945). The first gives his theory of science; the second extended his theory of science to history and society, and criticized the notion of historical laws; the third is a two-volume treatise on the philosophy of history, politics and society.

Popper's later principal works include *Conjectures and Refutations* (1962), and *Objective Knowledge* (1972), that are both collections of major papers; a Library of Living Philosophers volume (1974) containing an intellectual autobiography and a set of replies to his critics, and the *Postscript to the Logic of Scientific*

Discovery (1982-3), where the proposal for the experiment can be found.

Popper's theory of science is centred around two problems: 'the problem of induction' and 'the problem of demarcation'. The problem of induction is that of explicating the relation between theoretical knowledge and experience, whereas the problem of demarcation is the problem of distinguishing science from metaphysics as well as from logic and mathematics.

Popper set out his converging solutions to these problems in the *Logic of Scientific Discovery*: We get knowledge by accepting statements describing experience that contradict and hence refute our hypotheses; this means that a deductive rather than an inductive relation holds between theoretical knowledge and experience. This is, of course, intimately connected with the thesis that we should only count as scientific hypotheses that are falsifiable by experience. Popper thought that in this way Hume's problem of the inductive leap, that Hume himself thought was illogical but unavoidable, is in fact avoided. Furthermore, our hypotheses come not inductively from experience but from our propensity to guess—experience, through our mistakes, guides us only negatively in rejecting hypotheses that are contradicted by it. A common objection is that, in the same way that any amount of empirical evidence would be unable to verify a statement, no amount of experience will able to falsify it. Popper answers that there is a logical asymmetry: whereas a universal statement cannot be derived from or verified by singular statements, it can be contradicted by one of them.

Another objection is that falsifying evidence can always be circumvented by *ad hoc* definitions. Because Popper finds that this is an unconquerable difficulty, he concludes that falsifiability must be entrenched in a methodology. A methodology is a policy decision governing our actions when doing science, and the choice should be made according to which methodology will best provide for our aims.

The demarcation between science and metaphysics is thus a matter for decision, not discovery about the nature of things. A constant theme in his *The Logic of Scientific Discovery* and elsewhere, is that Popper defines his position by debate and contrast with logical positivist positions, and with inductivism and conventionalism. Unlike the logical positivists, Popper gives a constructive role to metaphysics in science, taking support here from the earliest Greek speculations about the nature of the world.

Some reflections on biology seem to have given rise to some new metaphysical perspective in the "Epistemology Without a Knowing Subject".[14] He gives a distinction between the world of physical things and the world of mental things, and he argues that objective knowledge is located in neither, but in what he calls 'World 3', which is the world of humanly created objective contents of thought. Some of the consequences of 'World 3' seem rather counterintuitive: For example, it contains not only all truths, but also all falsehoods, which thus have an equally objective existence.

With regards to some more specific points of his philosophy of science, Popper thought that the less probable a theory is, the better it is. Science is interested in theories with a high informative content, since such theories possess a high predictive power and are consequently highly testable. But the probability and the informative content of a theory vary inversely, since the more information a statement contains, the greater will be the number of ways in which it may turn out to be false. Consequently the severity of the test to which a theory can be subjected, and by means of which it is falsified or corroborated, is important.

Concerning the concept of truth, Popper was initially uneasy with it and in his earliest writings avoided asserting that a theory which is corroborated is true (or close to the truth)—every theory is an open-ended hypothesis for him and so

[14] (1972: Chapter 3)

it has to be at least potentially false. For this reason Popper restricted himself to saying that a theory which replaces a falsified theory is a 'better theory' than its predecessor. Later he came to accept Tarski's reformulation of the correspondence theory of truth, and in his *Conjectures and Refutations* he integrated the concepts of truth and content to frame the metalogical concept of 'truthlikeness' or 'verisimilitude':

> "Popper suggested that the aim of science should be the development of theories with higher degrees of *verisimilitude* (likeness to truth). One may well accept the view that all exiting scientific theories are (likely to be) false, and yet also hold that they are closer to the truth than their predecessors. If, science as it grows, moves on to theories with higher verisimilitude, then there is a clear sense in which this process takes science closer to the truth (although at any point in time, we may not know how close to the truth science is)".[15]

Critics of Popper came to see the concept of 'verisimilitude' and its various problems as central to Popper's philosophy of science, and consequently held that the whole edifice of the latter had been subverted. Popper protested that he had not intended to imply that the concept is central to his philosophy, but instead that its chief value is heuristic and intuitive. However, this response has not satisfied his critics.

Popper spent much time and effort in his later years attempting to show that criticisms of his work were either based upon misunderstandings, or that his views could, without loss of integrity, be made compatible with new and important ideas.

One such criticism has been pointed out by Lakatos and regards Popper's demarcation criterion. Popper often cites the discovery of Uranus as a critical test that was successfully passed by, and gave strong corroboration to, the Newtonian theory. Lakatos, though, denies that this should be thought of as a critical test and asks what would have happened if Neptune was not found? Would Newton's theory have been falsified? Clearly not, since the failure could have been

[15] Psillos (1999: 261–2)

attributed to any number of reasons other than the falsity of 'core' Newtonian physics.

Lakatos argued that theories are falsified, if at all, not by Popperian critical tests, but rather within the elaborate context of the research programmes associated with them, when these programmes become increasingly difficult to sustain by an increasing amount of data contradicting them. Popper's idea does not take into account that all high-level theories evolve despite the existence of anomalies which are incompatible with the theories. Such anomalies are not usually taken as an indication that the theory is false, but on the contrary, as a motivation for revision of the auxiliary hypotheses which are associated with the theory in order to incorporate, and explain, existing anomalies.

Another criticism is associated with the fact that scientific laws are expressed by universal statements taking the logical form 'All Xs are Y', meaning 'If anything is an X, then it is Y'. That is, they take conditional form rather than existential, and since they are not existential in nature, they logically cannot imply any basic statements, since basic statements are explicitly existential. The problem for Popper is then to specify how any basic statement can falsify a scientific law. Popper answers that scientific laws are always taken in conjunction with statements giving some 'initial conditions' of the system under investigation and these are singular existential statements that, when combined with the scientific law, give implications that can falsify the law. But Hilary Putnam for example has argued that the assumption that singular existential statements will always do the work of bridging the gap between a universal theory and a prediction that is false, in that in some cases at least the statements required to bridge this gap are general rather than particular.

This section provided a very brief summary of the most important aspects of Popper's philosophy that form the background to, but are not integral to the consideration of the experiment he proposed as a possible refutation of the 'Copenhagen interpretation' of quantum mechanics and to his views on quantum mechanics more generally. The next sections deal more specifically with these issues.

1.3. Popper on Realism as the Motivation for his Experiment

Popper proposed his experiment in the introductory chapter of his book *Quantum Theory and the Schism in Physics*.[16] The chapter has the title: "On a Realistic and Common Sense Interpretation of Quantum Theory",[17] and he starts by declaring that "It is the great task of the natural sciences and of natural philosophy to paint a coherent and understandable picture of the Universe.".[18] He further thinks that "Today, physics is in a crisis... And part of the present crisis—the almost permanent revolution of its fundamental theories—is, in my opinion a normal state of any mature science. But there is also another aspect of the present crisis: it is also a crisis of understanding".[19] Popper gives two reasons for this troubling aspect of the crisis: "(a) the intrusion of subjectivism into physics; and (b) the victory of the idea that quantum theory has reached complete and final truth".[20] Popper finds 'subjectivism' unacceptable in all its manifestations. It can, for instance, be blamed for "several great mistakes. One is the positivism of Mach... Another is the subjectivist interpretation of the calculus of probability..."[21] In general Popper holds that scientists should be trying to give 'a coherent and understandable picture of the Universe'. But so far as the state

[16] Popper (1982*b*: 27–30)

[17] (*Ibid.*: 1–34)

[18] (*Ibid.*: 1)

[19] (*ibid.*)

[20] (*ibid.*)

[21] (*Ibid.*: 2)

of quantum mechanics as he faced it went, they had failed to do so. Instead they had introduced subjectivism into physics via a particularly unacceptable version of positivism. On standard interpretations for example, the Heisenberg uncertainty principle does not tell us anything about the world independently of our operations in it, but instead sets a limitation on our knowledge of that world. The second exceptional—and for Popper objectionable—aspect of the 'crisis' was what he saw as Bohr's insistence that quantum mechanics as it stood, was a complete and therefore final theory.

Popper writes:

> "The central issue here is *realism*. That is to say the reality of the physical world we live in: the fact that this world exists independently of ourselves; that it existed before life existed, according to our best hypotheses; and that it will continue to exist, for all we know, long after we have all been swept away."[22]

He refers to his arguments for realism in his (1962) and (1972), and states that those arguments were "partly rational, partly *ad hominem*, and partly even ethical."[23] For example, in his (1972) after he states that realism is the "thesis of the reality of the world"[24] and also that "*the central tenet of what may be termed 'realism'* [is that one's] own existence will come to an end without the world coming to an end too",[25] Popper states that "[r]ealism is essential to common sense"[26] and that "common sense, or enlightened common sense, distinguishes between appearance and reality" and it "also realises that appearances... have a sort of reality; or in other words that there can be a surface reality—that is an appearance—and a depth reality."[27]

22 (*ibid.*, emphasis in the original.)
23 (*ibid.*)

24 Popper (1972: 33)

25 (*Ibid.*: 35)

26 (*Ibid.*: 37)

27 (*ibid.*)

Popper admits that "realism is neither demonstrable nor refutable... [b]ut it is arguable and the weight of the argument is overwhelmingly in its favour".[28] The antithesis to realism is, of course, idealism which, according to Popper, "In its simplest form, idealism says: the world... is just a dream".[29] As Popper points out, this theory too is not refutable (any attempt to show that we do not live in a dream might be part of that dream). But although there are no conclusive arguments for either position, Popper thinks that there are inconclusive, but nonetheless telling "*arguments in favour of realism*; or, rather, *against idealism*".[30] For example (a) "realism is part of common sense and that the alleged *arguments* against it are not only philosophical in the most derogatory sense of this term, but at the same time based upon [a] mistaken part of commonsense..."[31] (b) Moreover, with the notable exception of quantum mechanics "almost all, if not all, physical chemical, or biological theories imply realism, in the sense that if they are true, realism must also be true."[32] (Here he adds: "This is one of the reasons why some people speak of 'scientific realism'. It is quite a good reason. But, because of its (apparent) lack of testability, I myself happen to prefer to call realism 'metaphysical' rather than 'scientific'."[33]) Finally, idealism implies that it is our mind "which creates this beautiful world",[34] and this is refuted by Popper's knowledge that he is not the creator; he holds that "[d]enying realism amounts to megalomania".[35] Popper therefore proposes to "accept realism as the

[28] (*Ibid.*: 38)

[29] (*ibid.*)

[30] (*ibid.*)

[31] (*ibid.*)

[32] (*Ibid.*: 40)

[33] (*ibid.*)

[34] (*Ibid.*: 41)

[35] (*ibid.*)

only sensible hypothesis—as a conjecture to which no sensible alternative has ever been offered".[36]

The other place that Popper has referred to for his arguments for realism is his (1962, Chapter 3, especially Section 6). There he defends his 'third view concerning human knowledge', and gives more specific arguments against instrumentalism. He states that "[i]nstrumentalism is embraced by Bohr and Heisenberg only as a way out of the special difficulties which have arisen in quantum theory."[37] But difficulty in interpreting a theory is hardly a sufficient reason for accepting such a view given that such difficulties have arisen before and had subsequently been overcome:

> "Maxwell at first inclined towards an essentialist interpretation of [his] theory: a theory which ultimately contributed more than any other to the decline of essentialism. And Einstein inclined at first to an instrumentalist interpretation of relativity, giving a kind of operational analysis of the concept of simultaneity which contributed more to the present vogue for instrumentalism than anything else: but he later repented."[38]

So Popper would like to get physicists to realise that "the principle of complementarity is *ad hoc* and ... that its only function is to avoid criticism and to prevent the discussion of physical interpretations; though criticism and discussion are urgently needed for reforming any theory".[39] And because "instrumentalism is, as I have tried to show, no more acceptable than essentialism", scientists should not "accept either of them, for there is a third view". and this view,

> "preserves the Galilean doctrine that the scientist aims at a true description of the world, or of some of its aspects, and at a true explanation of observable facts: and it combines this doctrine with the non-Galilean view that though this remains the aim of the scientist,

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[36] (*Ibid.*: 41)

[37] Popper (1962: 114)

[38] (*ibid.*)

[39] (*ibid.*)

> he can never know for certain whether his findings are true, although he may sometimes establish with reasonable certainty that a theory is false."[40]

Popper then gives his formulation of the third view of scientific theories: "they are *genuine conjectures*—highly informative guesses about the world which although not verifiable ... can be submitted to severe critical tests. They are serious attempts to discover the truth".[41]

He expands on this third view by stressing some aspects of it that differentiate it from essentialism and instrumentalism, starting with essentialism. He has earlier defined essentialism as the combination of the last two out of the three doctrines of Galilean philosophy that he disagrees with: "(1) The scientist aims at finding a true theory or description of the world, which shall also be an explanation of observable facts. ... (2) The scientist can succeed in finally establishing the truth of such theories beyond all reasonable doubt. ... (3) The best, the truly scientific theories, describe the 'essences' or the essential natures' of things—the realities which lie behind the appearances".[42] Given that this means that essentialism implies that we need to discover the real world behind the appearances that are apparent to us, it has to be discarded because "the world of each of our theories may be explained, in its turn, by further worlds which are described by further theories—theories of a higher level of abstraction, of universality, and of testability".[43] Thus Popper's third view—often called conjectural realism—is that scientific theories always *attempt* to describe the real objective world; but while they *may* succeed in doing so, we can never know this—it is always possible that they will systematically be refuted and replaced by a still better theory which implies that they are strictly speaking false.

[40] (*ibid.*)
[41] (*Ibid.*: 115)

[42] (*Ibid.*: 103–4)

[43] (*Ibid.*: 115)

Popper's central aim is to contrast his third view with 'instrumentalism'. He takes instrumentalism to be the claim that there is no reality behind the appearances; there are only appearances and our theories are simply calculating devices that allow the "the derivation of singular statements [about those appearances] from other singular statements".[44] Popper recognises the simplicity of this view, but thinks that the strongest argument for it is founded on Berkeley's nominalistic philosophy of language. According to this philosophy, expressions like 'forces of attraction' are meaningless for such things cannot be observed. Accordingly, the whole of Newtonian theory lacks any "informative or descriptive content".[45] Given this, the dispute, for example between Galileo and the church would be dissolved, because, although Galileo uttered a descriptive sentence when he said 'and yet, it moves', its function, when properly understood, is merely instrumental.

Popper's main criticism of instrumentalism is that because it takes scientific theories purely as calculating devises, it entails that there is no difference between the 'pure' sciences and the 'applied' sciences. But Popper argues that there is in fact a difference between them by pointing to what he calls a logical asymmetry between the relations that hold between the two. He thinks that theories are tested by attempts to refute them, whereas no corresponding attempts are made in the case of rules or instruments of computation. Instruments are *used* within their limits of application, but they are not refuted. For Popper the instrumentalist view cannot explain scientific progress since, in order to account for the replacement of Newton's theory by Einstein's, the instrumentalist would have to say that classical mechanics is not refuted but it is right where its concepts can be applied. But that would surely just mean that it can be applied where its concepts can be applied. So, Popper holds that the instrumentalist view fails.

[44] (*Ibid.*: 108)

[45] (*Ibid.*: 109)

1.4. Popper and the Copenhagen Interpretation

In the introduction of his *Quantum Theory and the Schism in Physics*[46] Popper, as well as giving a detailed account of his views on quantum mechanics, also proposes his experiment.[47] This section gives the main points of his views on the theory and the reasons that led him to propose the experiment. The description of the experiment in some detail follows in the next chapter.

Popper opposed the Copenhagen interpretation of the quantum formalism, developed by Heisenberg, Bohr and Pauli, since it was first proposed in the 1920's. He considered that quantum mechanics, in its statistical interpretation developed by Einstein and Born, had no special epistemological consequences. Moreover, for him the indeterminacy relations were just scatter relations, and the 'collapse of the wave packet' was something that occurred in any probabilistic theory and had nothing to do with Planck's constant $\hbar$ or with an action at a distance. Further, he argued that the theory can and should be interpreted realistically and locally.[48]

Popper is a realist of a metaphysical kind. He does not think that physics supports his realism but, conversely, that his metaphysical realism warrants our demand on science to "paint a coherent and understandable picture of the Universe"[49]. In the same way that he believes that the world will go on after his death, he expects science to provide an understanding of the world and our position in it. In a sense this expectation is impossible to test empirically, as it is impossible to test our expectation that the world will go on after our death.

Popper conjectures the reality of matter, of energy, of particles, of fields of forces, of wavelike disturbances of these fields and the like, and he also argues

[46] Popper (1982*b*)

[47] (*Ibid.*: 27–30)

[48] (*Ibid.*: 25–27)

[49] (*Ibid.*: 1)

that quantum mechanics says nothing about epistemology, about our knowledge and its limits—nothing, that is, more than Newtonian dynamics does. As part of his realism he considers the Heisenberg relations to be simply scatter relations, and to have no special significance for the theory of knowledge. They are neither about indeterminacy nor about uncertainty; they simply inform us about the scatter of physical particles, such as photons or electrons or neutrons, after they have, for example, passed through a narrow slit. He further conjectures that most experimental physicists and biologists, share his realistic attitude and further that most physicists believe that their own interpretation of quantum mechanics, which is fundamentally realistic, is identical to the "official" interpretation, the Copenhagen interpretation developed by Bohr and Heisenberg. Thus he considers the prevalence of the orthodox view as a historical blunder which left behind three important doctrines stemming from the 'idealistic' or 'positivistic' Copenhagen Interpretation and its basic contention that quantum physics is not so much a theory of micro particles as a theory of our knowledge of micro particles. He suggests that most of the doctrines of the Copenhagen Interpretation have died a natural death. However, the following three doctrines are still pretty much alive.

1.4.1. The Three Doctrines

The first doctrine is that the Heisenberg formulae

$$\Delta E \Delta t \geq \hbar, \quad \Delta p_x \Delta q_x \geq \hbar, \quad \Delta p_y \Delta q_y \geq \hbar, \ldots$$

and so on, are about limits to human knowledge or to the precision of possible measurement on particles. This first doctrine Popper denies. His denial is a direct result of the fact that this doctrine is incompatible with his realism. He asserts that the Heisenberg formulae are about the lower limits of the scatter of particles.[50] By considering an experiment where an ensemble of particles is prepared so as to have their position within q_x and $q_x + \Delta q_x$, he asserts that the momenta of the particles will show a statistical scatter

$$\Delta p_x = \frac{\hbar}{\Delta q_x}$$

and that the particles themselves will possess sharp positions and, at the same time, sharp momenta.

The second doctrine that stems from the Copenhagen interpretation is that particles and waves are "complementary" views of the same microphysical entities. We become aware of these entities, or we acquire knowledge of them, via our measurements, and in some measurements they appear as particles and in other measurements as waves. Popper opposes this doctrine from his realist standpoint—he even thinks of it as "a mistaken and vicious doctrine"[51]—and, together with Louis de Broglie, he asserts that particles, which are carriers of energy, are always accompanied by waves, while waves are perhaps not always accompanied by particles—there may be empty waves. Furthermore, Popper thinks that the Copenhagen interpretation has prevented many physicists from investigating the interesting consequences of this possibility.

[50] (*Ibid.*: 54)

[51] (*Ibid.*: 42)

The third doctrine is the famous 'collapse of the wave packet'. It says that if in an experiment a particle is reflected by a semi-transparent mirror instead of passing through it, then the part of the wave packet which has passed through the mirror is destroyed by our knowledge that it cannot accompany a particle (so that its amplitude is zero). Popper believes that this view derives from a trivial misinterpretation of probability theory—one that he himself cleared up more than 50 years ago.

1.4.2. Popper's Opposition to Subjectivism

As mentioned, Popper notes that the idealistic and non-realistic way of speaking has bitten deep into physics. For example, in quantum mechanics there is much talk about 'observables'. The term is used to such an extent that we no longer notice that the term "observable" introduces a subjectivist ideology into physics—an ideology of a kind that Popper considers completely inadmissible. It is the ideology of von Neumann, of the positivistic philosophy, and Popper has been fighting against it since at least 1925.[52] For Popper this ideology suggests to physicists that one can say in advance what is observable and what is a 'hidden variable', and that one can give a list of the observables, excluding everything which is not on that list, as non-observables. But accepting this would mean that physics was not capable of progressing, and of introducing new kinds of variables.

However, as Popper notes,[53] physics has indeed introduced many new variables since Von Neumann introduced this ideology into physics. When Von Neumann introduced his proof there were only electrons and protons, together with their positions, momenta, spins, and energies. In a sense all variables are hidden: Nothing could be more hidden, from von Neumann's point of view, than, say,

[52] (*Ibid.*: 11–13, 85)

[53] (*Ibid.*: 11–14)

the colour or the charm of a quark. What we do in physics is in general to invent hidden variables by way of our conjectures and theories. They cease to be 'hidden' when our theories turn out to be successful; this is, precisely, how theory and experiment cooperate. So, the talk about hidden variables is, for Popper, ideological and misleading, and, moreover, this kind of ideology has worked its way even into the thinking of genuine realists.

How did it come about that the view that particles possess position and momentum was abandoned? For Popper it was abandoned only in 1927, some two years after the birth of quantum mechanics and after Pauli's idea that electrons in an atom have no trajectories. This led to the idea that the particle itself has neither position nor momentum, but that by 'measuring' the particle (and thus interfering with it), we impose upon it a position and a loss of the precision of its momentum, or alternatively a momentum and a loss of the precision of its position. If we measure one parameter, then the other becomes smeared. At the same time, all this is attributed not to the particle but to the measurement, to the interference with the particle or, rather, the wave packet. The wave-particle was thought to be another incarnation of the particle and, in some sense, identical to it. The so-called wave-particle dualism greatly enforced the idea of the trackless particle.

Concerning the general problem of the (rational) understanding of quantum theory, Popper, in accordance with his general philosophy of science, thinks that 'understanding' should not be thought of as a matter of picturesque imagination or intuition, but as a matter of being aware of the logical functions of a theory.[54] Thus one should be especially aware of the open problems which a new theory was expected to solve and of the problems which were newly created by it; and

[54] (*Ibid.*: 103)

of comparing and evaluating the various competing theories from the point of view of their power to solve old problems and to create promising new ones. He consequently disagrees with Bohr's suggestion that we have to renounce the hope of understanding our theories since the conditions permitting our understanding of macro-physics were absent in micro-physics.

Heisenberg maintained the view that quantum mechanics should have a special epistemological status that determines the limits to our knowledge. Popper thinks that this view arises from a misunderstanding of probability theory and that the theory has no more and no less epistemological import than any classical physical theory. More precisely, he thinks that the so-called indeterminacy relations have no special epistemological import. They do not indicate anything like limits to our knowledge. They are merely scatter relations.

1.4.3. Propensities[55]

For Popper, quantum theory is a probabilistic or statistical theory. It is as objective and realistic as Newtonian mechanics or as Boltzmann's Gas Theory. Furthermore, statistics do not enter physics because of lack of knowledge but because in various problems we need statistical information, and further we need statistical premises to derive statistical conclusions for the solution of these problems. For example, Planck tried in 1897–1900 to solve an essentially statistical problem, and as a result came up with the radiation formula, where the constant $\hbar$, makes its first appearance. For Popper, the opposite view, and almost all the existing difficulties, arise from a misunderstanding of probability theory. Specifically, the main sources are the old tendency of interpreting probability subjectively and the neglect of the calculus of relative or conditional probabilities.

The problem with interpreting the probability calculus subjectively for Popper

[55] (*Ibid.*: 68-74, 125–130)

is that by starting from a subjective interpretation of probability premises and then deriving from them an objective statistical conclusion, one is committing a logical mistake. The mistake is, as von Mises has shown, that at some step of the derivation, the non-statistical meaning of some symbol is replaced by a statistical one, and thus we construct a bridge from the non-statistical, probabilistic premises to the statistical conclusion.[56]

In order to apply the probability calculus to the situations of interest in physics (and maybe biology), Popper has proposed his own propensity interpretation. He does not consider this to be the best interpretation for every situation, but the best suited for these specific cases where we study repeatable experiments. His interpretation seems to him to be a development of the classical interpretation, due to de Moivre and Laplace. For the classical interpretation $p(a, b)$ (signifying the probability of event a given the event b) is given by the proportion of equally possible cases of the event a that are compatible with event b. In this development the first step is to avoid the confinement to equally probable cases, as this creates problems, and to introduce 'weights'. In doing this the 'number of cases' is replaced by the 'sum of the weights of the cases'. The second step is the interpretation of those weights as "measures of the propensity, or tendency, of possibility to realize itself upon repetition". Now $p(a, b)$ can be interpreted as "the sum of the weights of the possible cases that satisfy the condition b which are also favourable to a, divided by the sum of the weights of the possible cases that satisfy b".[57]

According to Popper the main point is to distinguish between probability statements (statements about frequencies in virtual sequences of experiments) and statistical statements (statements about frequencies in actual sequences of exper-

[56] (*Ibid.*: 66–67)

[57] (*Ibid.*: 70)

iments), and to consider the weights in the probability statements as measures of the conjectural virtual frequencies that are to be tested by actual statistical frequencies. In this way Popper purports to circumvent von Mises' problem by replacing the possibilities with propensities that are interpreted as tendencies for the production, under similar experimental conditions, of the relative frequencies. These propensities are supposed to be real properties of the particular experimental arrangements.

1.4.4. Complementarity

Concerning complementarity, Popper thinks that the relations between particles and waves are insufficiently explored, and that those relations are not of the character attributed by complementarity. Complementarity, being an ideology rather than having the character of a theory, is an empty word that could and should be abandoned.[58] Popper also thinks that there are many reasons to support de Broglie's view that, though there are no particles without waves, there may be empty waves without particles which he calls propensity waves.[59]

1.4.5. Hidden Variables

For Popper, Von Neumann's proof of the non-existence of hidden variables is not only invalid, as is admitted now by almost everybody, but the concept of hidden variables is also highly ambiguous and can be abandoned without loss.[60] There is also a further point to be made concerning hidden variables: Popper thinks that all reality is hidden and that it is the task of science to discover the hidden reality; he considers the opposite view, as sheer ideology.

[58] (*Ibid.*: 10, 50)

[59] (*Ibid.*: 48)

[60] (*Ibid.*: 11)

1.4.6. Measurements

With regard to measurement Popper distinguishes two types of measuring experiments: Those which are like the measuring of polarisation, or measuring the spin of a particle, which may change the measured state of the object unless an identically oriented polarizer precedes the experiment. The measurements of this first kind can be called non-classical measurements. This is opposed to what Popper calls classical measurements such as the measurement of position, which do not, in general, change the position of the object measured.

Accordingly, there are two types of Einstein, Podolsky and Rosen experiments.[61] The non-classical experiments—for example Aspect's experiment—that Popper calls EPRB where B stands for Bohm, and the classical which is just the one given in the paper of Einstein, Podolsky and Rosen (EPR) in its original form. Popper considers that second kind of EPR experiment as obviously simpler and easier to interpret. Furthermore, he thinks that the two types of experiment may possibly lead to different results, although this does not seem likely to him.

To confirm his position Popper presents an experiment similar to the one given in the EPR paper and notes that in his opinion the EPR paper was designed to establish that a 'particle may possess at the same time position and momentum', as against the Copenhagen interpretation. This is an experiment of the classical type. Some people have suggested that it may not be possible to carry the experiment out. Obviously Popper thinks that not only may it be carried out, but also that it would be far simpler than Aspect's experiment. He further thinks that the outcome of the experiment would be predictable by quantum mechanical calculations. Popper has made specific predictions for the outcome that he thinks would refute the Copenhagen Interpretation while leaving the quantum formalism in-

[61] (*Ibid.*: 22–25)

tact. Also, if correct, Popper's prediction would at least show that what he calls Heisenberg's subjectivist interpretation, that Heisenberg's uncertainty relations are limitations to our (predictive) knowledge, is false.

Without much elaboration, as this will come later, the experiment, which Popper considers as a simplification of the EPR experiment, is the following: We have a source S in the centre of the experimental arrangement. There are two screens on either side of the source. On the screens there are slits, parallel to the y-axis, and these can be varied in their width Δy. Behind each of the two screens we have a battery of coincidence counters. The source S need not be very small as each pair of correlated particles emanates from an extremely small region, and the 'scatter' (for Popper), or 'uncertainty' of the particle arriving at that side. Popper suggests all the possible (for him) outcomes and selects the ones that would favour his realism (as opposed to the Copenhagen Interpretation). The outcome that favours his realism is that when the screen on one side is left open, then *nothing will happen*: The particle on that side will not scatter (or, in the Copenhagen language, its uncertainty will not increase), as opposed to the particle that goes through the screen on the other side.

Popper, though, keeps an open mind about the outcome of the experiment and accepts that since quantum physics has surprised us many times, the outcome of the experiment might not be what he anticipates. Should the results of the experiment support the subjectivist Copenhagen Interpretation, then he would interpret them as indicative of action at a distance similarly to the published results of the Aspect experiment. Considering the possible outcomes of his experiment which he considers as absurd and supporting the Copenhagen Interpretation, Popper thinks that they follow from Heisenberg's thesis that quantum theory is not about particles but only about our knowledge of particles and that "objective reality has evaporated".[63] For example, if it is our knowledge which creates the scatter, then the particle on the left side (where the screen has been removed) is supposed to scatter because of our knowledge. This for Popper is absurd as our 'knowledge' may come long after the event and, accordingly, we ought to say that the second particle does not scatter where the screen has been. Rather, it should scatter, when we have checked the evidence that the first particle has scattered, by which time it might have travelled much further than the apparatus. The sort of difficulties indicated by Popper, makes him think that the events are connected in a more or less classically causal way, and not dependent on our knowledge of them.

[63] Popper (*Ibid*: 35) quotes Heisenberg from *Daedalus* **87**, 1958, pp. 95–108.

1.4.7. Action-At-A-Distance

For Popper, the major issue that stems from the EPR type of experiments is the issue of action-at-a-distance.[64] Popper thinks that quantum mechanics is not an action-at-a-distance theory. This is not to say that he would consider action at a distance as absurd; far from it as Newtonian mechanics is a theory which he considers a very good one and it is an action-at-a-distance theory.[65] But Popper considers field theories which avoid action at a distance as much preferable to the ones that do not. So, although Popper does not have an objection to an action at a distance *per se*, his objection is to the specific kind of action at a distance that he thinks is suggested by the outcome of his experiment that favours the Copenhagen interpretation, which does not diminish with increasing distance. In order to accept such an action at a distance we must have overwhelming evidence and, as he is very sceptical about the Aspect experiment, he thinks that there is not sufficient evidence available for saying that quantum mechanics is not a local theory.

Popper considers two bases for the assertion that quantum theory is an action at a distance theory. The first one is the 'collapse of the wave packet' and the second is the Bell inequalities.

Popper dismisses the 'collapse of the wave packet' as a reason for action at a distance because it can occur in every probabilistic theory and has nothing what so ever to do with an action at a distance. He notes that one of the obvious questions which stems from Heisenberg's discussion of the 'collapse of the wave-packet' is: Did the wave-packet collapse when we gained knowledge of what had happened, or did the wave packet collapse when the experiment happened which later led to our knowledge? He thinks that these are two actually different situations and

[64] (*Ibid.*: 22–27)

[65] (*Ibid.*: 173)

the distinction is of crucial importance. Popper notes that Heisenberg discusses in considerable detail some points relating very closely to this formulation, but he does not address this decisive question. Heisenberg's point is that knowledge is what is crucial, but does not address the crucial point that our knowledge is not simultaneous with the experiment which creates the knowledge.

Popper considers the following experimental situation originally suggested by Heisenberg:[66] By reflecting a wave packet at a semi-transparent mirror we can decompose it into a reflected packet and a transmitted packet. If after a sufficient time, when the two parts will be separated by a distance, an experiment yields the result that the photon is, say, in the reflected part, then the probability of finding the photon in the other part immediately becomes zero, and thus we have a kind of action at the distant point.

But Popper does not see a problem with this. He thinks there are two possibilities. The first is that the wave packet is physically real: It can be thought of as a propensity field which can be tested by the statistical distribution upon frequent repetitions of the experiment. In this case it does not collapse, but remains unaffected, because the probability (or propensity for Popper) that, upon repetition, the photon will be found not to be reflected remains unaffected. (Remember that $p(a,b) = 1/2$ does not conflict with $p(a,c) = 0$.) The second possibility is that the wave packet is an unreal and merely mathematical representation of our probabilistic estimate. In this case, which is not, Popper thinks, the true case for various logical and physical reasons, there can be no physical action upon the wave packet. The problem for Popper arises only if we regard the wave packet as somehow complementary and identical with the particle: This is a view that ought for him to have been eliminated with the adoption of Born's statistical

66 (*Ibid.*: 76–77)

interpretation.

The second basis for action-at-a-distance is the Bell inequalities. Popper refers to tests, like the Aspect experiment, which involve these inequalities and admits that, although he does not think of them as conclusive, most of them seem to go for the Copenhagen Interpretation of the theory and against local theories. Popper admits that this is surprising to him as he was expecting those tests to refute the theory. Instead it looks as if the tests have gone the other way.[67]

Redhead, in his critique of the Popper experiment,[68] notes that, at the theoretical level the Bell inequalities are often interpreted to imply the following assertion, called by T. Angelidis[69] 'the Universality Claim' (U.C.): U.C.="*All possible local theories (of the emission and propagation of light or of particles in opposite directions) lead to statistical predictions that differ from the predictions of the quantum formalism*". This claim, is basically the claim that any theory which is local in the sense that it does not permit signals to travel from any point to a different point that is outside the light cone of the first point, would make predictions that can be shown to be in disagreement with the predictions of the quantum theory.

Since 'local theories' are theories (or models) that do not imply action at a distance, it follows from the U.C. that all theories are by definition either local theories or action at a distance theories (and never both). This means that, *if* U.C. is true, *then* quantum mechanics must be an action at a distance theory (as asserted by Heisenberg and the Copenhagen Interpretation on other grounds). On the other hand, Popper thinks that if U.C. is mistaken, then there is no reason whatsoever to believe that quantum mechanics is not a local theory, provided

[67] (*Ibid.*: 22–25)

[68] Redhead (1995)

[69] Angelidis (1983) and (1993)

that his view of the 'collapse of the wave packet'—as an action-at-a-distance—is accepted.

So, according to Popper's analysis of the situation, everything depends on a *theoretical* issue. It all depends on whether or not U.C. is true; nothing depends on the experiments. This is the case, Popper thinks, since the experiments have, as could be expected, supported quantum mechanics, which, if U.C. is false, may be just one of the possible local theories. But it seems that even those who criticize the Bell inequality are convinced that quantum mechanics is nonlocal. Perhaps for this reason, it looks like no one is attributing to U.C. the crucial importance given to it by Popper. Popper asserts here that the claim that quantum mechanics is nonlocal is just that—a claim or conjecture. He would like someone to perform his experiment also in order to review the very weak and, in his opinion, non-existing reasons that speak in favour of this conjecture.

As we have seen, it has been suggested that as long as we cannot exploit instantaneous action at a distance for the transmission of signals, Einstein's interpretation of the Lorentz transformations and his principle of relativity is not affected. Popper disagrees with this suggestion. He thinks that this is mistaken, as one would have to introduce an inertial frame relative to which two events occur simultaneously in an absolute sense. With the idea of infinite velocity, a Newtonian-Lorentzian absolute space with an absolute coordinate system becomes, for Popper, almost unavoidable. He writes in a footnote that "For within special relativity, two events on the x axis which are simultaneous relative to the inertial system S_1 are never simultaneous relative to the inertial system S_2 unless S_1 andS_2 are not moving relative to each other along the x axis, even if there should be no interaction (and therefore no signaling) between these two events"[70].

[70] Popper (1982*b*: 20*n*)

He notes here that if, against his expectation, we have to interpret Aspect's or his own experiment as indicative of instantaneous action at a distance, the most reasonable way would be to accept Lorentz's interpretation of his formalism of special relativity and to give up Einstein's interpretation. Consequently, those experiments could become, in Popper's interpretation, the first experiments that are crucially decisive between Lorentz's and Einstein's interpretations of the formalism of special relativity, as all the experiments supporting special relativity are not crucial between Lorentz's and Einstein's interpretations and therefore could be retained.

1.5. Quantum Mechanics vs. Realism

While a brief look at the tension between quantum mechanics and (some versions of) realism was given in the first section, this section presents an outline of the Copenhagen interpretation, which is the interpretation that Popper wants to test with his experiment, the subsequent reactions to it, and some more sophisticated views on quantum theory.

1.5.1. The Copenhagen Interpretation

Quantum theory is the theory of the microcosm and, in terms of predictive success, it is perhaps the most successful theory we have. At the same time it seems to challenge our understanding as it violates some of the fundamentals of classical physics that have become part of our common sense. An interpretation of the theory aims to provide a bridge between the results of the experiments and the mathematical formalism that consists the theory. The Copenhagen interpretation of quantum theory was the first such attempt and among its founders were Niels Bohr, Werner Heisenberg, and Max Born. But it is not an easy matter to give an account of the interpretation which all its founders will agree on. Bohr and Heisenberg never totally agreed on how to understand the formalism, and neither used the term 'the Copenhagen interpretation' as a joint name for their ideas. In fact, Bohr once distanced himself from what he considered to be Heisenberg's more subjective interpretation.

Bohr holds that physical concepts can only be applied in specific experimental contexts. For example, both position and momentum are essential to quantum theory, but the experimental arrangements for measuring them are physically incompatible. Furthermore, the Heisenberg uncertainty relations imply that the more information is encoded in the state about the one, the less there is about the other. Therefore, Bohr concludes that these concepts are *complementary*, and that each concept can be applied only within the appropriate experimental context.

The driving force behind the development of the theory and also of complementarity was the correspondence rule. Bohr's theory for the hydrogen atom, when dealing with large quantum numbers, gave results that coincide approximately with the results of classical electrodynamics. This led Bohr to see this as a methodological requirement for the development of the theory: The correspondence rule was meant to make sure that when Planck's constant becomes negligible the predictions of the theory should correspond with those of classical physics. But this also means that beyond the technical use of the rule, it also had methodological consequences, since it would only be possible to compare the values predicted by the two theories if the concepts in both are compatible. Bohr held the metaphysical idea that we cannot do without classical concepts; that they are indispensable for our understanding of physical reality and that we can only analyse and compare quantum phenomena using classical concepts. This naturally led to the idea of complementarity: Since classical concepts were indispensable, and their meaning had to remain unchanged, their application had to be restricted, and they became complementary.

Both Heisenberg and Bohr started to look for a coherent interpretation for the mathematical formalism, once Heisenberg had managed to formulate a consistent quantum mechanics in 1925. In doing so Heisenberg developed his uncertainty principle or indeterminacy relation, and Bohr gave an analysis in terms of concrete experimental arrangements, with special focus on the double-slit experiment. He presented his complementarity idea at Como in 1927 and regarded Heisenberg's relations as a way of expressing the complementarity of descriptions of atomic phenomena. Initially Bohr accepted, albeit in a reluctant manner, that complementarity applied to the wave and particle picture and their duality. But by

1935 he had recognised that "there is no question of mutual exclusion of them in the sense of their attribution to exclusive experimental arrangements. Those properties of the classical pictures that are conserved in quantum mechanics, appear in one and the same arrangement".[71] For example in the two-slit experiment both pictures can appear, since the interference pattern can be made to consist of single dots, and such an empirical fact could not be disregarded by Bohr. But "[c]omplementarity is designed to show that this 'dilemma as regards the choice' between the pictures is not a real dilemma; such choice is not necessary",[72] so he abandoned wave-particle complementarity and insisted on the complementarity of dynamic and kinematic properties.

Furthermore, until that time, when also the EPR paper appeared, Bohr was suggesting that this complementarity was due to the uncontrollable interaction between the apparatus and the measured object. But after the EPR paper he argued that those inherent attributes that are only idealizations of classical physics lose their meaning, and Heisenberg's 'indeterminacy relations' have to indicate the ontological limits of the accuracy of measurements, rather than the epistemological limits that would be implied if indeed they were the result of the interaction between the apparatus and the measured object. For Bohr, the ascription of classical concepts to the measured quantum phenomena depends on the experimental context. One needs the whole setup in order to specify the defining conditions for the application of kinematic and dynamic properties in the quantum domain. Quantum phenomena are complementary in the sense that the ascription of properties to quantum systems depends on mutually exclusive measurements, but the information that such an experiment provides exhausts all the possible objective knowledge that we can have of that object.

[71] Held (1994: 882)

[72] (*ibid.*: 890)

The main elements of the Copenhagen interpretation are Bohr's correspondence principle, indeterminism, Born's statistical interpretation of the wave function, and Bohr's complementarity interpretation of certain atomic phenomena together with the uncertainty relations that give a quantitative description of complementarity. Complementarity and the uncertainty relations, as we shall see, form the focus of Popper's experiment.

The point of the disagreement between Bohr and Popper (as the latter saw it) is whether or not these relations express a limitation of our abilities to know the world, i.e. a limitation on our knowledge—as the early Bohr thought—or they express a fundamental limitation on the world itself—like the post 1935 Bohr held. Is it the case that a quantum system has both the disposition to give a value for position and the disposition to give a value for momentum and, because of the disturbance that any of the two measurements will result in, we can only know one of the two; or is it that all quantum systems can only actually possess one of the two dispositions? The answer given by the Copenhagen interpretation is that the latter is the case: The wave function is complete; it describes everything there is to know about the system, and the limitations implied by the uncertainty relations are ontological in the sense that they show the limits of what there is, rather than epistemological in the sense that they show what we can know. Both answers have been defended by various authors with no sign of general agreement.

It is clear that, in Heisenberg's own view, all the above questions stand or fall together. Indeed, we have seen that he adopted an operational "measurement=meaning" principle according to which the meaningfulness of a physical quantity was equivalent to the existence of an experiment purporting to measure that quantity. Similarly, his "measurement=creation" principle allowed him to attribute physical reality to such quantities. Hence, Heisenberg's discussions moved rather freely and quickly from talk about experimental inaccuracies to epistemological or ontological issues and back again.

1.5.2. Bohr and Einstein

Einstein strongly opposed the orthodox interpretation of quantum mechanics, and expressed this opposition in his heated debates with Niels Bohr. Einstein insisted that Bohrs interpretation was necessarily incomplete as it entailed instantaneous correlations and actions-at-a-distance. Wheeler and Zurek (1983) quote Pais and Rosenfeld describing the atmosphere at two of the Solvay Conferences. Pais says of the 1927 fifth Solvay Conference:

> ... All participants were housed in the same hotel and there, in the dining room, Einstein was much livelier. Otto Stern has given this first hand account [to Res Jost]: "Einstein came down to breakfast and expressed his misgivings about the new quantum theory, every time he had invented some beautiful experiment from which one saw that it did not work... Pauli and Heisenberg who were there did not react to these matters, "ach was, das stimmt schon, das stimmt schon," ah well, it will be alright, it will be alright. Bohr on the other hand reflected on it with care and in the evening, at dinner, we were all together and he cleaned up the matter in detail."[73]

and Rosenfeld says of the 1930 sixth Solvay Conference where Einstein thought he had found a counterexample to the uncertainty principle:

> "It was quite a shock for Bohr... he did not see the solution at once. During the whole evening he was extremely unhappy, going from one to the other trying to persuade them that it couldn't be true, that it would be the end of physics if Einstein were right; but he couldn't produce a refutation. I shall never forget the vision of the two antagonists leaving the club [of the Fondation Universitaire]: Einstein a tall majestic figure, walking quietly, with a somewhat ironical smile, and Bohr trotting near him, very excited... The next morning came Bohr's triumph."[74]

Einstein contributed to the early development of the quantum theory with his work on the photoelectric effect. He also contributed to the discussions concerning the foundations of the subject with his EPR thought experiment (discussed in the next section). In parallel with these developments, Einstein changed his attitude from a positivist position—one that demanded from a scientific theory

[73] Pais (1979), p. 901; quoted in Wheeler and Zurek (1983) p. viii.
[74] Rosenfeld (1968), p. 232; quoted in Wheeler and Zurek (1983) p. ix.

only empirical and predictive accuracy—to a realist attitude which asked for a realist account based on causal explanation, and ontological commitment to the theoretical entities. For example, Fine notes: "...the shift from special to general relativity, [is] a shift that brought Einstein away from Mach's sensation based positivism toward Planck's realism..."[75] From this realist position Einstein maintained, accordingly, that orthodox quantum mechanics was incomplete in that it could not provide a realist ontology that is consistent with the predictive success of the theory, and that therefore it did not fulfil the requirements for an adequate theory. This does not mean that Einstein became more conservative. Fine[76] points out in his comparison of the attitudes of the old and the young Einstein towards the quantum theory that he was not unwilling to scrutinise or even replace the classical concepts. In fact when compared to Bohr, Einstein was the more radical of the two, as it was Bohr who come up with the method of complementarity in order to rescue the classical concepts of momentum and position.

Bohr, on the other hand, would consider that the conceptual problems arise from the application of classical concepts to situations that should be described quantum mechanically that suggest the realist convictions in the first place. He would consider the theory complete and free of problems as long as one would stick to an empirical approach that keeps knowledge at the phenomenological realm and 'reality' at the noumenal one. Consequently Bohr would not accept that a further development of the theory would make it complete by reconciling it with a realist perspective and with relativity theory. He would therefore reject any developments such as those advocated by Einstein.

[75] Fine (1986: 16)

[76] (*Ibid.*: Ch. 2)

1.5.3. The EPR Argument and Bohr's Objection

The tension between quantum mechanics and relativity sprang out of Einstein's attempts to show that one could obtain precise values for incompatible observables of a quantum system. This effort by Einstein came in a series of attempts (a glimpse of which is captured by the above quotes by Pais and Rosenfeld) to prove that Bohr had overlooked some factors whose inclusion would avoid the problematic aspects of the theory. But Bohr countered those attempts by showing that Einstein had overlooked other aspects that prohibit one from doing so. The EPR paper[77] was his final attempt to circumvent the uncertainty principle by an appeal to the orthodox theory and remote anti-correlation.

Classical conservation laws together with quantum mechanics imply that two particles having joint angular momentum equal to zero after an interaction, will continue to have this even after they have travelled away from each other. Einstein's argument that, given the locality principle (that the measurement on one particle cannot superluminally influence the state of the other given a sufficient distance), if one can measure particle A's value for a given parameter then the anti-correlated value of the same parameter for particle B becomes also known without the undertaking of a physical measurement on it. This contrasts with the limitations of Heisenberg's uncertainty principle. Einstein wanted to emphasize the ontological issue of the existence of such values in a way that is independent from the experimental set-up that is used to obtain them. For Einstein, to consider those values as produced by the act of measurement was tantamount to denying the basic scientific tenet of an observer-independent truth. Furthermore, to confuse the limits of observation with the limits of the reality of quantum objects was to commit a category mistake.

[77] Einstein, Podolski, and Rosen, (1935)

Bohr's reply[78] was that the EPR claim that the value of an observable of a system can be predicted without a disturbance of that system is ambiguous. This is because the two particles belong to the overall system, and so any measurement on either particle disturbs the system. For Bohr, if the particles always exhibit an anti-correlation irrespective of the measurement that is made on particle A, then the state of particle B should depend on the choice of which parameter was measured on particle A, and not on its intrinsic properties.

An analysis of the EPR argument that emphasises the aspects that are relevant to the discussion of Popper's experiment is given by Fine.[79] Fine starts by noting that the EPR argument can be expressed using two assertions:

(INC) *The quantum mechanical description of a system given by the state function is incomplete* (as [Einstein, Podolsky, and Rosen] say, not "every element of physical reality has a counterpart in the theory").

(NSV) *Observables represented by noncommuting operators cannot have simultaneous reality* (i.e. cannot have simultaneously sharp values).[80]

Fine explains that the authors draw the conclusion (INC), from two premises:

$$(INC) \vee (NSV) \qquad \text{and} \qquad \neg(INC) \rightarrow \neg(NSV)$$

It is easy to show that this is in fact a valid argument. According to Fine, the authors establish the first of those two premises by supposing that

> a pair of noncommuting observables of a system have simultaneous values and they note that no state of the system is simultaneously an eigenstate for both observables. Hence they conclude that the description given by the state function for such a system would be incomplete.[81]

78 Bohr (1935*a*); and (1935*b*)

79 Fine (1986: 32–9)

80 (*Ibid.*: 32)

81 (*ibid.*: 33)

I.e., they establish that $\neg(NSV) \rightarrow (INC)$ from which the disjunction follows. To establish the second premise, "the authors assume the antecedent (i.e. that the theory is complete) and try to establish the existence of simultaneous values for position and linear momentum (in the same direction) in a certain interesting system."[82] This system is of course the EPR system. Using the criterion of reality (referred to in section 1.1) the authors " conclude that for such a system at least one particle must have simultaneously definite position and momentum."[83]

At this point Fine digresses to point out that the argument, as presented, is more complex than he is making it out to be. Using other evidence from various letters of Einstein to Schrödinger, for example, and from the fact that the definite article is missing from the title (something apparently natural for a Russian native speaker), Fine latter concludes that "Einstein did not write the paper, Podolsky did, and somehow the central point was obscured."[84] So the paper would have been more clear if Einstein himself had written the paper, given that his style is normally more clear than that in the actual paper.

Returning to the argument, Fine notes that the reality criterion was aimed at Bohr's 'doctrine of disturbance', and it hit its target as Bohr agonised for a response daily. This response

> "focused on EPR's criterion of reality which, in a typical phrase, Bohr said "contains an essential ambiguity". It was then precisely the question of disturbance to which Bohr responded. For he argued that the phrase "without in any way disturbing a system" was the ambiguous culprit. There was, he admitted, no question ("of course") of a physical ("mechanical") disturbance of one system brought about by measuring its correlated twin, "but even at this stage there is essentially the question of an *influence on the very conditions which define the possible types of predictions regarding the future behaviour of the system*"."[85]

[82] (*ibid.*)

[83] (*ibid.*)

[84] (*Ibid.*: 35)

[85] (*ibid.*: 34)

What Fine does next is to make two very important observations concerning Bohr's response to EPR. The first concerns the philosophy underlying Bohr's response:

> "what Bohr himself underlines (the italics are his) is virtually textbook neopositivism. For Bohr himself simply identifies the attribution of properties with the possible types of predictions of future behaviour. (I think this point needs emphasizing, for many commentators seem inclined to suppose that Bohrs tendency to obscure language is a token of philosophical debate, whereas I find that, as here, where it really matters, Bohr invariably lapses into positivist slogans and dogmas.)"[86]

The second observation is of particular importance for Popper's argument. It points to the difference that Popper's experiment wants to probe, and the fact that the EPR argument is the one that pushes Bohr from a position that Popper agrees with, to a position that Popper does not accept and thinks that his experiment can prove wrong:

> "Bohr's stated response to EPR marks a definite break from his previously stated view. For in earlier writings and in his response to Einstein at the Solvay conferences, Bohr had always argued that disturbance created by a measurement of a particular variable caused a real change in the physical situation which altered the preconditions for applying complementary variables. But here Bohr switched from this doctrine of actual physical disturbance to what one might call a doctrine of semantic disturbance. In a way that Bohr does not account for on physical grounds, the arrangement to measure, say, the position of one particle in a pair simply precludes meaningful talk of the linear momentum of the unmeasured (and admittedly undisturbed) other particle. I think it is fair to conclude that the EPR paper did succeed in neutralizing Bohr's doctrine of disturbance. It forced Bohr to retreat to a merely semantic disturbance and thereby it removed an otherwise plausible and intuitive physical basis for Bohr's ideas."[87]

Popper, in his argument, attacks precisely the notion that the uncertainty relations express some 'semantic disturbance' of the system due to the act of measurement. He rather thinks that they express the physical disturbance that Bohr used in his response to Einstein at the Solvay conferences, that is, before the EPR paper. His argument is simply that in a situation like the one described

[86] (*ibid.*: 34–5)
[87] (*ibid.*: 35)

by EPR, if the disturbance is semantic, as Bohr was forced to admit here, then the uncertainty relations should hold for an indirect measurement, i.e., one that is performed 'without in any way disturbing a system'. If on the other hand the uncertainty relations express the physical disturbance due to the act of measurement, then the relation should not hold for such an indirect measurement. An instantiation of the experiment should tell us one way or the other.

Returning to Fine's analysis, he concludes that the central conclusion of the EPR argument is that one of the following two assertions need to be abandoned:

(1) the description by means of the ψ-function is *complete*

(2) the real states of spatially separated objects are independent of each other.[88]

Fine emphasises that the conclusion is *not* that incompleteness should be concluded. Only the fact that the two sentences cannot be held together. Moreover, the argument says Fine is *not* about the uncertainty relation and hidden variables. But the point made in the previous paragraph still stands: The argument did force Bohr to abandon physical disturbance for semantic disturbance and so Popper can legitimately use this point regardless of the fact that Einstein's argument has a different conclusion. Fine goes on to say that Einstein had managed to neutralise even semantic disturbance because "even in the semantic version of that doctrine, measuring the momentum of A does not preclude assigning a somewhat earlier *momentum* to B, which is all the argument requires."[89]

For Fine, Einstein's intention was to show that quantum theory only provides a "statistical account of a realm of objects whose properties outstrip the descriptive apparatus of the theory."[90] But this incompleteness was not a sign that some

[88] (*Ibid.*: 37)

[89] (*Ibid.*: 38)

[90] (*ibid.*)

extension of the theory via some hidden variables is needed. Instead he favoured some unification with his relativity theory. Fine adds that not only did Einstein not succeed, but Bell's argument suggests that in fact it is the second assertion above that needs to be abandoned. Nevertheless, Fine himself does not feel that the arguments are conclusive and, further, that it might be that they are both false.

Fine closes his analysis by pointing out that it might well be that the idea of incompleteness was suggested to Einstein by Heisenberg. He cites a letter from Heisenberg to Einstein: "it seems likely to me that quantum mechanics can never make direct statements about the individual system, but rather it always gives only average values."[91]

To recap the earlier discussion and the one provided by Fine, Bohr countered Einstein's attempt to show that such properties, or, as the EPR paper puts it, the 'elements of physical reality' exist. Moreover, one could not argue that the values of the parameters were determined by any reasoning that relied on what happened at the source as, according to the theory, they were produced by the settings of the measurement apparatus and these could not be traced to the source. This meant that either one had to revise the whole of quantum mechanics or one had to accept the completeness of the orthodox interpretation; and further that the only line available to a realist interpretation was to admit some non-local, faster-than-light causality. The proponents of the orthodox view would thus argue that there is no way to maintain both the experimental results that supported quantum mechanics and Einstein's local realism. Nevertheless, Bohr's retreat from the notion of physical disturbance to that of semantic disturbance is an important issue and Popper attacks this very issue with his experiment. It seems

91 (*Ibid.*: 39)

that Popper is not alone in thinking that Bohr was forced to 'positivist slogans and dogmas'.

1.5.4. The Collapse of the Wavefunction and Hidden Variables

Despite this apparent defence of the orthodox theory it still seemed that it failed to explain the wavefunction collapse. The theory, that is, did not give an account of the mechanism according to which, when a measurement is made, the wavefunction instantaneously changes from a superposition of states of possible measurement outcomes into a state consistent with just one of those possible outcomes. Attempts to explain this led some physicists to the admission of consciousness as a determining factor for this collapse. De Broglie instead[92] proposed his pilot-wave theory in which the complete description of the quantum system was not given solely by the wavefunction. The wavefunction acted as a guide for the particle and determinate values could be given to it. On this account, the limits suggested by the uncertainty principle become epistemological limits on our powers for precise observation rather than ontological limits of quantum reality.

Following de Broglie, Bohm, also disagreeing with the orthodox view, proposed his hidden-variable theory[93] that held that the quantum formalism is incomplete and assumed that it could become complete by the addition of some hidden-variables that would allow a realist interpretation, and at the same time provide a solution to the various problems associated with the theory.

In response to such arguments Bell eventually showed that any hidden-variable theory which takes locality and determinism on board would not be able to predict the results of experiments, as they are predicted by quantum theory, because any such theory would violate Bell's inequality. This inequality shows that accord-

[92] de Broglie (1960)

[93] Bohm and Hilley (1995); Cushing (1994)

ing to quantum mechanics and in agreement with the experiment, there exists a greater degree of anticorrelation between two particles in an EPR set up than could be allowed by a local hidden-variable theory.

It seems that the violation of Bell's inequality by the predicted and experimental results forces an admission of some form of non-locality irrespective of which side one wants to take in the realism-antirealism debate.

Indeed Aspect's experiments were a test of this difference between the orthodox interpretation and hidden variables theories, and it turned out to be decisively on the side of the orthodox interpretation. This, on the other hand, is no problem for the orthodox theory, as it has taken the instrumentalist line. It has taken, that is, the line that the aim of science is to 'save the phenomena' by accepting the results of the experiments and accommodating them in the simplest possible way, and resisting at the same time the search for realistic and metaphysical explanations.[94]

Bohr therefore, simply denied that there is an answer to the question, posed at the beginning of this section, as to the mechanism of how exactly the wavefunction 'collapses' in order to produce the determinate results at the end of the measurement. For him to pose such questions would mean that one adopts a descriptive language that works for observable objects or events but cannot be applied to the quantum domain since it imposes an inappropriate conceptual apparatus or explanatory scheme.

As long as no measurement is performed, quantum mechanics is analogous to classical mechanics in the following sense: In classical mechanics the instantaneous state of a mechanical system is described in terms of the values of the 'observable variables' of the system at any time t, e.g., the position x and momentum $p = m\dot{x}$.

[94] Duhem (1969); Gardner (1979); Mach (1960); Reichenbach (1938); van Fraassen, (1980), (1992).

That is, the state is defined as

$$[x(t), p(t)]$$

This classical definition assumes that: The two variables x, p, have precise, well defined values at each time instance t; and that it is always possible to measure these variables without disturbing the system significantly. The motion of a particle could then be determined by applying Newton's Second Law, so that its position and momentum at all future times could be determined.

Similarly, in quantum mechanics, the wavefunction defines the state of the system to the extend that it can be specified. It permits the assignment of probability-values rather than determinate values (as in the classical case) for position and momentum. One then uses Schrödinger's equation in order to specify the future states of the system, in an analogous way to the use of Newton's Second Law. For example, Squires points out that the deployment of Schrödinger's equation "allows the wavefunction to be uniquely determined at all times if it is known at some initial time. Thus quantum mechanics is a deterministic theory of wavefunctions, just as classical mechanics is of position"[95]

The problem is that as soon as one wants to pose the kind of question that deals with the details of what and how things happen during the measurement process, the kind of question that Bohr ruled out, this analogy stops. All the orthodox view says is that the wavefunction somehow collapses and thus produces the determinate values of position or momentum, and that one cannot or should not raise such questions, otherwise one either will contradict the quantum formalism or is in danger of bringing new problems and paradoxes. But the decree that one should not ask such questions would be frustrating to anyone aware of the enormous success that quantum mechanics has in 'explaining' the long list of

[95] Squires (1994: 24)

otherwise unaccounted for phenomena, some of which have given rise to some of the most remarkable advances in present-day physics.

On the orthodox view, we have at our disposal everything required of an adequate theory or interpretation in the case of quantum theory. All we need is to apply the standard quantum formalism, obtain a probability value as yielded by the Schrödinger equation, and then compare the results with those achieved through empirical observation. But if this is the case, 'interpretation' is itself being redefined in quantum-instrumentalist terms, that is, as involving no claim to understand what is really going on beyond the requirements of empirical adequacy.

It has been argued[96] that this would make it hard to square with the great success that quantum mechanics had as a physical theory that 'explains' a vast range of classically unexplained phenomena but also to inspire the development of technologies undreamt of before the advent of quantum mechanics. It would make it hard because the sense in which quantum mechanics has been inspiring all those advances involves a supposition that any theory (together with the interpretation that one uses to relate it to what is going on in the world) that brings about scientific or technological progress, does so by providing a better understanding of the real-world and its features—for example position and momenta of microparticles, causal structure, etc.—and it is only this better understanding that makes the progress possible. It is not surprising then, that some would feel uneasy with the *a priori* barring of questions that aim at such an understanding. Holland expresses this point nicely:

> Yet the interpretation of the wavefunction which ascribes to it a purely statistical significance is not forced upon us by the experimental results... On the contrary, one may take the view that the characteristic distribution of spots on a screen which build up an interference pattern is evidence that the wavefunction indeed has a more potent physical role than a mere repository of information on probabilities, for how are the particles guided

[96] Aronson, Harré and Way (1994); Salmon (1984); Smith (1981)

> so that statistically they fall into such a pattern? Such a question is naturally ruled out by the purely probabilistic interpretation. But the latter is appropriate only if we wish to reduce physics to a kind of algorithm which is efficient at correlating the statistical results of experiments. If we wish to do more, and attempt to understand the experimental results as the outcome of a causally connected series of individual processes, then we are free to enquire as to the further possible significance of the wavefunction (beyond its probabilistic aspect), and to introduce other concepts in addition to the wavefunction.[97]

A rival to the orthodox view is the hidden-variables theory. It is based on a realist2 foundation considering particles as possessing objective values, independent of the act of measurement. It assumes that a theory of quantum phenomena should not only have the high predictive success that quantum mechanics has, but also work on the principle that the reality of quantum phenomena might surpass our ability of detection. It denies therefore the orthodox resistance to go beyond the limits of empirical confirmation.

The hidden-variables theory has dealt adequately with delayed-choice experiments and the classic two-slit experiment which brought about wave-particle duality. Moreover, after the EPR and the related debates, it has also dealt adequately with the idea that quantum mechanics shows that we need a radical departure from all forms of objectivist or causal-realist thinking. If it is possible to interpret those experiments in accordance with Bohm's theory, then there is strong warrant for going beyond the orthodox view towards more refined or sophisticated interpretations.

[97] Holland, (1993: 66)

1.5.5. Van Fraassen

Some philosophers, Bas van Fraassen among them, would reject such a line of thought, like the one found in Bohm's approach, in favour of a 'constructive empiricist' approach. This approach has no ontological commitments beyond what is given as a matter of direct observational warrant.[98] According to van Fraassen's view it is simply unnecessary to posit the existence, for example, of micro-particles that play an explanatory role in our best scientific theories, but can only be 'observed' with the aid of advanced instrumentation. Concerning such entities we should adopt an agnostic stance, while carrying on 'referring' to them whenever we need to, but always bearing in mind that we do so in terms of the warrant that the current empirical evidence affords us. So van Fraassen holds that it is the aim of a scientific theory to save empirical appearances without any need for ontological commitments in the sense that a realist or causal-explanatory line would make: "To be an empiricist, is to withhold belief in anything that goes beyond the actual, observable phenomena, and to recognise no objective modality in nature".[99]

This approach is antithetical to the approach that holds that one can take a long-term view of the history of science that allows for convergence on truth as a matter of inference to the best explanation. Such views would be based on the fact that there are various once unobservable entities, such as molecules and atoms, that have entered science as the speculative posits that van Fraassen describes, but in the course of scientific development have acquired strong realist credentials. This was achieved not only through the development of more advanced explanatory theories concerning their structure, causal powers, etc., but also through

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98 van Fraassen, (1980), (1992)

99 van Fraassen, (1980: 202)

more refined observational techniques.[100] For example Gartner argues that "there was a gradual transition from an instrumentalist to a realistic acceptance of the atomic theory, because of gradual increases in its predictive power, the "testedness" of its hypotheses, the "determinateness" of its quantities, and because of resolutions of doubts about the acceptability of its basic explanatory concepts".[101]

Van Fraassen's attempt to meet such arguments part-way by stretching the term 'observable' to cover what could be described by a human observer has been countered, for example by Hacking,[102] among others, who objected that science has used radio telescopes and electron microscopes to extend the limits of human observation, as well develop techniques to verify their accuracy. It is an obvious objection that we should not think that there is anything special about the unaided human perception, or indeed human perspective in general. Edwin A. Abbott's and George Gamow's entertaining introductions to space-time and relativity,[103] describe the adventures of beings that live in the two-dimensional flatland. In Gamow's book one of the scientists of flatland gets in trouble by proposing the crazy idea that there could be a third dimension. He ends up in prison only to be rescued by a three-dimensional being that takes him out of the prison through the third dimension. Moreover, Churchland[104] asks how would van Fraassen's view apply to beings that were sentient but rooted to the ground like trees. A philosopher in this world taking this position would urge an anti-realist perspective for the very distant objects that are proposed theoretically, but cannot be observed by his fellow tree-like beings. These entertaining fictions show that

[100] Gardner, (1979); Nye, (1972)

[101] Gardner, (1979: 1)

[102] Hacking, (1983)

[103] Abbott, (1983); Gamow, (1940)

[104] Churchland, (1985)

there is something problematic if we limit knowledge just to what human observers can extend their powers of direct observation to: In both these examples there are consequences that follow from the non-directly observed parts of the fictional universes, and sticking to the limitation leads to the wrong conclusions.

Of course, given the problems that one faces in interpreting quantum mechanics with an interpretation that is based on realist or causal-explanatory ideas, the theory would be the most suitable ground for constructive empiricism. Indeed in his (1992), van Fraassen makes such a claim and rejects the idea of metaphysical realism and fundamental questions like those concerning the existence of subatomic particles "which the philosopher can answer speculatively by positing abstract, unobservable, or modal realities".[105] Science in general and quantum mechanics in specific should aim to save the phenomena and refrain from metaphysical debates.

Nevertheless, Van Fraassen develops a highly sophisticated line of argument. In his book he looks to the whole range of interpretative options and considers carefully the counter-proposals given by advocates of a realist approach. He proposes a modal interpretation which claims to represent a significant advance on the standard Copenhagen doctrine and also to provide a more complete physical theory in something like the sense required by Einstein and Bohm. In any case his thesis is at its best in the aftermath of quantum theory where the basic notions of knowledge, truth, reality, etc., have become highly problematic.

105 van Fraassen, (1992: 481)

1.5.6. Action at a Distance, Holism, Non-Separability

One of the peculiar aspects of quantum theory is the correlations it posits between distant objects. Discussions of those correlations in quantum mechanics usually take place in the context of EPR/Bohm type of experiments, and involve the issues of action at a distance, entanglement, non-separability and holism. The last two have also been suggested as features that clearly distinguish quantum from classical physics.

'Action-at-a-distance' occurs when an effect is produced by a cause that acts at some distance from the effect, without any intervening agents that transmit cause. Newton's theory of gravity supposes exactly that: A force is exerted between two objects that is proportional to their mass and inversely proportional to their distance, whatever their distance apart.

Newton himself, as is well known, did not like the idea of such an action. He and others therefore searched for an ether to do the mediating—a search that eventually led to the discovery of the theory of relativity. Relativity places constraints on such an action as it assumes that no signal can travel faster than the speed of light.

As briefly mentioned earlier, John Bell in his (1964) showed that given some plausible assumptions, any local model of the EPR/B experiment would have measurement outcomes that are subject to certain inequalities about the probabilities of those outcomes, the 'Bell inequalities'. These inequalities are incompatible with the predictions of quantum mechanics. When technology allowed for experiments to be performed in the 1970s which tested this difference, their results supported the predictions of quantum mechanics; and this gave rise to a general consensus that the quantum world is non-local in some way.[106] Although this is a general

[106] Berkovitz (1998a), (1998b) Butterfield (1989), (1992); Healey (1991), (1994); Howard (1989); Redhead (1987); Teller (1989).

consensus, it is *not* universal. For example Chang and Cartwright[107] argue, from a realist-towards-theories perspective that a causal account of the correlations that are present in an EPR experiment is actually possible, if one rejects the violation of factorizabilty in EPR. This argument, they claim, is based on insisting on retaining the classical descriptions for these experiments. Controversial issues are that of the exact nature of non-locality, its compatibility with relativity theory, and the analysis of the exact nature of the violations of factorizability.[108]

Two conditions, first, *parameter-independence*, stating that in an EPR/B experiment the probabilities for the outcomes of the two measurements are independent of the settings in the distant apparatus, and second, *outcome independence*, stating that they are independent of the outcome in the distant apparatus, turn out to be equivalent to factorizability when taken together.[109] It is commonly held that the types of non-locality involved in violations of the two conditions are different: Violations of parameter independence are taken to be indicators of some action at a distance that is incompatible with relativity;[110] violations of outcome independence are taken, by contrast, to be indicators of some sort of holism, non-separability or 'passion at a distance that is not incompatible with relativity.[111] This analysis in turn has been argued by others to be either irrelevant or misleading.[112]

So, one way to understand the fact that in the EPR/B experiment there seems to be an instantaneous influence between the two distant apparatuses, is to as-

[107] Chang and Cartwright (1993).

[108] Jarrett (1984), (1989).

[109] (*ibid.*)

[110] Redhead (1987: 108)

[111] (*ibid.*: 107); Howard (1989)

[112] Butterfield (1992); Maudlin (1994: 94–98)

sume that there is some kind of action at a distance that makes these influences propagate through space time in a non-continuous way. But a different understanding is that the influences are due to the non-separability of the quantum states describing composite systems like the particle pairs in those experiments or that there is some type of holism concerning those systems.

This latter understanding, however, seems to exclude the possibility of action at a distance. Generally, in quantum mechanics a complete description of the probabilistic dispositions for the possible results of measurements performed on a quantum system is given in terms of its state. A composite system might be in a pure state without its component systems being in their own pure states; we say that such systems are "entangled". The statistics of the measurements on such systems can either be understood using the notion of action at a distance, or the notions of non-separability and holism.

A general (and vague) statement that has widely been given for the thesis of Holism is that 'the whole is more than the sum of its parts'. The related thesis of non-separability can also be given a general statement saying that the whole is constituted by one state and not by states of its parts. Of course these vague statements allow for several different interpretations that could be applied to different fields. For example, holism can be interpreted either as a *metaphysical* thesis, saying that there are some composite wholes whose natures are not determined by the nature of their parts, or as a *methodological* thesis, claiming that the behaviour of a complex system can only be understood if it is understood as a whole, and not in terms of the behaviour of its component parts.

A more precise characterisation of explanatory holism is given by Healey:

> *Explanatory holism* "is the view that a satisfactory explanation of the behavior of an object of some type cannot be given by analyzing that object into its component parts and appealing to the laws that hold of these parts."[113]

[113] Healey (1991: 397)

The opposing thesis to methodological holism is that of methodological reductionism, claiming that the best way to understand a complex system is to look for the laws, regularities and principles that govern its parts.[114] The methodological debate between holism and reductionism is well suited to social and biological sciences, although it is also applied to physics, for example to Condensed Matter physics. There, reductionists would try to express the phenomena relating to solids or liquids in terms of quantum mechanics, whereas holists would hold that new concepts can be found at the macroscopic level which although they do not contradict quantum mechanics, are at the same time not deducible from it.

Metaphysical holism may be divided into three species: Ontological, nomological and property holism. Ontological Holism is the thesis that there are some objects that are not wholly composed of basic physical parts. Nomological Holism is the thesis that there are objects that obey laws that are not solely determined by the laws that apply to the structure and behaviour of their physical parts. But the more relevant issue for the debate of quantum non-locality is that of property holism, which deals with the relation that holds between the intrinsic properties of the whole and the intrinsic properties of its parts and denies that the former

[114] A nice example that can be used to illustrate the methodological holism view can be found in Roger Penrose's *Shadows of the Mind* (1994: 46), although he uses it "[t]o illustrate a lack of any real understanding by present-day computers". He gives the diagram of a chess position where both White and Black have all their pawns and Black has some rooks and bishops, whereas White only has the King beside the pawns. But the pawns themselves (black and white) form a diagonal wall so that none of the other Black pieces can break it and the two sides are completely separated by that wall. Penrose describes it thus: "In this position, black has an enormous material advantage, to the extent of two rooks and a bishop. However, it is easy for white to avoid defeat, by simply moving his king around on his side of the board. The wall of pawns is impregnable to the black pieces, so there is no danger to white from the black rooks or bishop. This much is obvious to any human player with a reasonable familiarity with the rules of chess. However, when the position, with white to move, was presented to "Deep Thought"—the most powerful chess computer of its day, with a number of victories over human chess grandmasters to its credit—it immediately blundered into taking the black rook with its pawn, opening up the barrier of pawns to achieve a hopelessly lost position!" This can easily be used to demonstrate that full knowledge of the laws that govern the components (that the pawns move forward and take other pieces diagonally) does not provide an understanding of the whole (the wall that is formed by placing pawns diagonally); one needs further an understanding of the relations that are formed by the pawns being combined together.

are determined by the latter. In doing so properly holism presents a thesis similar to non-separability. Naturally each one of the three species would require a clarification of the notion of 'basic physical part', but that is a matter best settled in the context of specific debates.

Property holism, as well being in opposition to reductionism, is also opposed to the thesis of supervenience[115] and to particularism.[116] The notion of supervenience and property holism are closely related in the following sense: Property holism holds that some properties or relations of the whole do not supervene on intrinsic properties and relations of the parts. Quantum mechanics appears to be a good example, since there are composite quantum states, like the entangled states, that do not supervene on the states of their subsystems. Particularism can be defined as the condition holding that the world is composed of individuals and that all individuals have non-relational properties and all relations supervene on the non-relational properties of the relata. Teller defines relational holism as the denial of this latter thesis.[117]

One problem is in providing exact definitions for the notions of 'intrinsic property' and of 'supervenience'. This is because there are properties of the whole that do depend on the properties of its parts—for example the relation which holds if and only if the parts compose a whole with all its properties—but holism looks for the ones that do not. One attempt to characterise a property as intrinsic is to say that it is so if an object has that property independently of the existence and states of any other objects.[118] An intrinsic property of the object is then said to supervene on the intrinsic properties of its parts and their spatiotemporal re-

[115] Healey, (1991)

[116] *ibid.*; Teller, (1986), (1989)

[117] Teller, (1989: 213)

[118] Langton and Lewis (1998)

lations if and only if there can be no change in the former without a change in the latter.[119]

Given these and similar considerations, Healey arrives at the following two opposing theses:

> *Pure physical particularism*: Every qualitative, intrinsic physical property and relation of a set of physical objects from any domain D subject only to processes of type P is supervenient upon the qualitative, intrinsic physical properties and relations of their basic physical parts (relative to D and P).
>
> *Pure physical holism*: There is some set of physical objects from a domain D subject only to processes of type P, not all of whose qualitative, intrinsic physical properties and relations are supervenient upon the qualitative, intrinsic physical properties and relations of their basic physical parts (relative to D and P).[120]

Non-separability also comes to different varieties: State non-separability and spatiotemporal non-separability. State non-separability is, obviously, the violation of the condition of state separability. Given the state of a system composed of two physical subsystems, the state of the whole will not be independent of those of its parts. Howard notes that Einstein formulated the condition of state separability, for a system composed of two subsystems, A and B. He quotes Einstein in his sketch of the EPR *Gedankenexperiment*, noting that Einstein stresses "as its only important feature the fact that by choosing to measure different observables of one system, A, we can attribute different ψ-functions, ψ_B or $\psi_{\underline{B}}$, to the other system, B. (He says that it does not even matter whether ψ_B and $\psi_{\underline{B}}$ are eigenfunctions of observables, as long as they are different.):[121]

> Now what is essential is exclusively thatψ_B and $\psi_{\underline{B}}$ are in general different from one another. I assert that this difference is incompatible with the hypothesis that the ψ description is correlated one-to-one with the physical reality (the real state). After the collision, the real state of (AB) consists precisely of the real state of A and the real state of B, which two states have nothing to do with one another. *The real stare of B thus*

119 Kim (1978)

120 Healey, (1991: 402)

121 Howard (1985: 180)

cannot depend upon the kind of measurement I carry out on A. ('Separation hypothesis' from above.) But then for the same state of B there are two (in general arbitrarily many) equally justified ψ_B, which contradicts the hypothesis of a one-to-one or complete description of the real states.[122]

Howard cites Einstein's 1948 essay, 'Quanten-Mechanik und Wirklichkeit' to show that his "commitment to realism is unambiguous" and that "Equally unambiguous, however, is his commitment to separability as a necessary physical condition for this realism:"[123]

> "Further, it appears to be essential for this arrangement of the things introduced in physics that, at a specific time, these things claim an existence independent of one another, insofar as these things 'lie in different parts of space'."[124]

Howard continues "What is less clear is why Einstein believed separability to be a necessary condition for realism. There is one important clue in the quoted passage, where Einstein asserts that separability is a necessary condition for testability:"[125]

> "Without such an assumption of the mutually independent existence... of spatially distant things, an assumption which originates in everyday thought, physical thought in the sense familiar to us would not be possible. Nor does one see how physical laws could be formulated and tested without such a clean separation."[126]

"There is another clue in the next paragraph, where Einstein says that locality is an additional necessary condition for testability:"[127]

> "The complete suspension of this basic principle would make impossible the idea of the existence of (quasi-)closed systems and, thereby, the establishment of empirically testable laws in the sense familiar to us."[128]

Finally Howard notes: "So the argument is that both separability and locality are

[122] Howard, (*ibid.*) quotes the letter from Einstein to Schrödinger, 19 June 1935
[123] (*ibid.*)

[124] Howard (*ibid.*), quotes Einstein.
[125] (*ibid.*)

[126] Howard (*ibid.*), quotes Einstein.
[127] (*ibid.*)

[128] Howard (*ibid.*), quotes Einstein.

necessary conditions for testability, the latter in particular because it grounds the existence of closed systems. But still one asks, 'Why?' "[129]

This last question of Howard's is backed up by the fact that the assignment of states to systems in quantum mechanics seems not to conform to these expectations. The quantum state of a system gives a specification of the probabilities for the various results of measurements on that system. *State separability* can then be formulated as the condition holding that the state assigned to a physical system composed of subsystems is supervenient on the states then assigned to its component subsystems.

This means that the condition obtains if and only if a system possesses a separate state that determines its qualitative intrinsic properties and relations, and the state of any composite system is supervenient upon the separate states of its subsystems. There are, consequently, two ways that state separability could fail: The subsystems may simply not be assigned any states of their own; alternatively the states that the subsystems may be assigned may fail to determine the state of the system they compose.

Indeed in quantum mechanics, state assignments have been deemed to violate the condition of state separability in both ways. First, entangled systems do so because the state representing their composition does not factorize into a product of Hilbert space vectors, each one representing a pure state for each subsystem and belonging to the Hilbert space of each subsystem. Second, each of the subsystems may be assigned what is called a 'mixed state' represented a density operator, in which case the subsystem mixed states do not determine the compound state in a unique way.

When Healey distinguishes holism from non-separability, he uses the latter

[129] (*ibid.*)

in the spatiotemporal sense: "Holism has to do with the irreducibility of certain part-whole relations, while non-separability is to be understood in spatiotemporal terms."[130] Starting from the idea that when assembling an object, like a versatile toy model that allows the construction of different objects from the same pieces, the properties of the final object will depend on how the pieces are put together, one can capture this with the notion of *Spatial separability*. Healey defines this Spatial non-separability as:

> "[The breakdown of] Spatial separability: The qualitative, intrinsic physical properties of a compound system are supervenient on the qualitative, intrinsic physical properties of its spatially separated component systems together with the spatial relations among these component systems."[131]

This is related to Howard's 'separability principle', which "asserts that any two spatially separated systems possess their own separate real states."[132] Both Howard and Healey discuss this in connection with the notion used by Einstein in his discussion of the EPR paper with Schrödinger. Healey points out the similarity between spatiotemporal non-separability and holism:

> *Spatial nonsepambility*: There exists a compound physical system, not all of whose qualitative, intrinsic physical properties supervene on the qualitative, intrinsic physical properties of its spatially separated component systems together with the spatial relations among these component systems.
> *Pure spatial holism*: There is some set of physical objects from a domain D subject only to processes of type P, not all of whose qualitative, intrinsic physical properties and relations are supervenient upon the qualitative, intrinsic physical properties and the spatial relations of their basic physical parts (relative to D and P).[133]

By generalising from space to space-time a naturally suggested notion is that of spatiotemporal non-separability, which Howard defines as the violation of the separability condition: "*Spatiotemporal separability*: The contents of any two

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[130] Healey (1991: 408)

[131] Healey (*ibid.*: 410)
[132] Howard (1985: 173)

[133] Healey (1991.: 412)

regions of space-time separated by a non-vanishing spatiotemporal interval constitute two separate physical systems. Each separated space-time region possesses its own, distinct state and the joint state of any two separated space-time regions is wholly determined by the separated states of these regions."[134] Moreover, Healey argues that spatial separability follows from spatiotemporal separability.[135] This means that spatial nonseparability entails spatiotemporal nonseparability, but spatiotemporal nonseparability neither entails physical property holism nor spatial nonseparability.

It is not easy to find examples of property holism or spatiotemporal nonseparability in classical physics. It is common ground that any non-local quantum theory would involve some type of non-separability or holism. For example if one considers a particle pair such that the particles have opposite spins in the z-direction, these can be written as $|\psi\rangle = |z+\rangle_1 \otimes |z-\rangle_2$ or $|\psi'\rangle = |z-\rangle_1 \otimes |z+\rangle_2$ (where the indices 1 and 2 refer to the two particles in the pair). In this case we say that the state can be decomposed into a product of separate states of the two particles. But such a particle pair may also be in a superposition of these states. Then it can be written as a linear sum of the states $|\psi\rangle$ and $|\psi'\rangle$:

$$|\psi''\rangle = \frac{1}{\sqrt{2}}|z+\rangle_1 \otimes |z-\rangle_2 - \frac{1}{\sqrt{2}}|z-\rangle_1 \otimes |z+\rangle_2.$$

Now, however, the state cannot be decomposed into a product of separate states of the two particles, and the particles do not possess any definite spin in the z- or any other direction. The state of the pair is not completely determined by the states of the two particles as there is a property of the pair that is not determined by the properties of the individual particles: The pair is correlated in such a way that measurements along any direction on the two will give anti-correlated results.

134 Howard (1989: 225–6)

135 Healey (1991.: 411)

This correlation is said not to be supervenient on the properties of the individual particles. So one can say of this state for the pair that property and relational holism holds, and given that the measurement on one particle affects the result of the measurement on the other, process separability fails.

The fact that the quantum state for entangled quantum system violates state separability, is not so problematic for a state that only specifies its probabilistic dispositions; but if the state specifies categorical properties, then Einstein's real state separability principle is under threat. On the other hand, according to the Copenhagen interpretation, which holds that the state only specifies the properties for which it assigns probability 1, state non-separability entails property holism. Various experimental results in fact confirm the predictions of quantum theory, which predicts probability distributions for combinations of joint and single measurements that *do not* factorize into products of two independent single distributions. If it is accepted that the state provides probability distributions for the results of measurements of the dynamical variables rather than a precise real value assignment, then this is already a violation of Einstein's principle. One of the alternatives is to conceive of the state as incomplete and supplement it with additional 'hidden variables'. Moreover, modal interpretations[136] have also suggested some kind of holism or nonseparability.

Part of the views of Bohr can be seen as supporting some kind of ontological holism. Bohr held that one can endorse a quantum system with properties such as position or momentum only in the setting of some experimental arrangement that can measure that property. In this sense he was taking the quantum system plus the measuring apparatus as a whole and the events that characterise that whole cannot be spit into events that characterise the system and events that

[136] Healey (1989, 1994), van Fraassen (1991)

characterise the apparatus. It might be said that the quantum system exists, but little else can be said about its properties outside the system-apparatus whole, according to Bohr.

Beyond this system-apparatus holism that is implied by Bohr's views, there is the holism that is implied by the experimental verification of the Bell inequalities. The violation of these inequalities has been taken as implying that the principle of separability that, according to Howard, Einstein has formulated, actually fails. As noted earlier Howard thinks that there are two ways to violate separability:

> "The more modest concerns the individuation of states; it is the claim that spatio-temporally separated *systems* do not always poses separable *states*, that under certain circumstances either there are no separate states or the joint state is not completely determined by the separate states. I call this way of denying the separability principle *nonseparability of states.* The more radical denial may be called *nonseparability of systems*; it is the claim that spatio-temporal separation is not a sufficient condition for individuating systems themselves., that under certain circumstances the contents of two spatio-temporally separated regions constitute just a single system."[137]

This is what it takes to violate the spatio-temporal separability referred to earlier. The idea of nonseparability and holism is involved in the discussion of the Kim and Shih experiment, as the authors suggest that the results of the experiment can be understood better if one accepts the idea that what we have in their set-up is a single "two-photon", rather than two individual photons. This will be discussed in Chapter 8.

[137] Howard (1989: 226)

1.5.7. Popper

Popper also opposes the antirealist view from his own realist perspective. In his *Quantum Theory and the Schism in Physics*,[138] he raised the same charge as Einstein. He argued that similar tactics were used much earlier by the apologists of the heliocentric system. They claimed that the heliocentric hypothesis, as it appears in the works of Copernicus and Galileo, should not be interpreted as providing any ontological commitments. It was not meant to describe the solar system as it is, but it should only be taken as an attempt to 'save the phenomena'. This would allow that no tension existed between the heliocentric hypothesis and the structure provided by the Christian teachings.[139]

Popper's initial motivation for attacking the Copenhagen Interpretation was what he saw as inherent anti-rational tendencies in it. In the thirties he interpreted those tendencies as having their roots in the subjective interpretation of probability and in the even more dangerous mixture of the subjective with the objective interpretation. This mixture, he believes, is to blame for the quantum theoretic attempt to muddle the subject with the object of knowledge. But, on top of that, Popper later diagnosed that this muddle is connected with the deterministic interpretation of classical physics, as he discusses in his *The Open Universe: An Argument for Indeterminism*.[140]

As noted earlier, Popper sees the Copenhagen interpretation as the cause of what he called 'the Schism in Physics':[141] On the one hand the quantum orthodoxy led by Niels Bohr which included Heisenberg, Pauli, Born, Jordan, and Dirac, and, on the other Einstein, Schrödinger, de Broglie, Bohm and even Landé, who

[138] Popper (1982*b*)

[139] (*Ibid.*: 102)

[140] Popper (1982*a*)

[141] Popper (1982*b*: 1–6)

opposed the orthodox interpretation.[142] This latter group had at various stages been in the orthodox camp, but had for different reasons later distanced themselves from it. They do not seem to have a common view on the interpretation of the theory. Beyond these two groups, Popper sees the majority of working physicists as the third group that do not take part in the discussion concerning the interpretation of the theory because, for them, such discussions are philosophical and as such not relevant to what they do.[143] Popper views this last opinion of physicists as a mistake, and he furthermore sees it as part of the over-specialisation that has grown in the field and which may easily lead to the end of science and its replacement by technology.

Popper cites Pauli saying that there is an agreement among physicists for the need of a theory that can explain the atomistic nature of electricity and masses of the elementary particles.[144] He observes that some physicists admit with great resistance that there are some refutations of the theory, albeit ambiguous ones. But how then is the theory to be revised? Popper sees two methods for a possible revision of the theory: Either to alter and/or generalise the formalism, or to interpret it and gain some physical understanding of its subject matter (the relation between a theory and its interpretation is quite central to the issues relating to the Popper experiment and will be discussed at the end of Chapter 2).

Although these two methods do not have to be in conflict, this is exactly what the orthodox school has held: It only holds the first method as admissible. This, for Popper, implies the philosophical position which holds scientific theories as instruments that have to be mastered and used, but that they do not have any

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142 (*Ibid.*: 36)

143 (*Ibid.*: 37)

144 (*Ibid.*: 100)

understanding to offer.[145] This instrumentalistic view of the orthodox school has become implicit in the thinking of most physicists of the third group, something which is due to Bohr and his interpretation of quantum theory. So what they look for is only a formalism and the application of this formalism in making testable predictions.

Popper finds this unsatisfactory. He notes:

> "It is especially unsatisfactory to those who see in physics not mainly an instrument for predictions, or other practical applications, but rather an instrument (if instrument it must be) for understanding the world we live in—for *explaining* this world... Realists such as Einstein and Schrödinger feel that theories are not only instruments but also attempts to describe a physical reality... Of course Einstein believed that his... theories should be tested, and for this reason, they *had* to be instruments for prediction. But what he was after (though he knew he would hardly catch it) was the real world, of which he hoped to give a true description."[146]

That is, for Popper the realist attitude of Einstein and Schrödinger would require of theories not only successful predictions, but also to provide a description of the physical world.

But at the same time, by putting emphasis on the word 'explaining', Popper seems to think that the one (true description of the world) is the same, or that it implies, the other (explanation, understanding). This understanding concerning physical reality is to be gained by some speculation, but any speculative theory has to be severely tested, in accordance with Popper's view of science, by the use of the very predictions that those theories had to offer. The quote above shows that Popper is confusing the two notions. This is not the only place where Popper does this. Explanations are very often made out to be the target of science in the 'Three Views' as well: For example he describes essentialism as holding that:

> "*The best, the truly scientific theories, describe the 'essences' or the essential natures' of things—the realities which lie behind the appearances.* Such theories are neither in need

[145] (*Ibid.*: 101)

[146] (*Ibid.*: 102; Popper's emphasis.)

> nor susceptible of further explanation: they are *ultimate explanations*, and to find tehm is the ultimate aim of the scientist.[147]

Popper actually disagrees with essentialism. But what he has issues with is not the notion that science aims to provide explanations, but the notion that these would be the ultimate ones. This point will discussed again further in Chapter 2.

Returning to the third group of scientists, the twist that Popper notes regarding their attitude is that, although they easily dismiss anything that goes beyond the formalism and its predictions as meaningless metaphysics and philosophical talk, this view is, in itself, a very old philosophical theory that has been used in the past 'as a weapon against a rising science'.[148] He points out that Cardinal Bellarmino opposed Galileo and that Bishop Berkeley opposed Newton by rejecting the notion that reason alone will be enough to discover the hidden secrets of nature irrespective of what kind of instruments one uses. Not only are these physicists unaware of the fact that they are indeed taking a philosophical position but Popper thinks also that this philosophy is "uncritical, irrational, and objectionable".[149]

Further, Popper thinks that instrumentalism, by not claiming any right to truth, can be used to avoid refutations: "For an instrument raises no claim to the truth, and so cannot be falsified in the sense in which a theory which does raise such claims can be falsified".[150] This is something that according to Popper's philosophy of science would place the theory outside the realm of science. Of course Popper here misses the point that an instrumentalist, on the face of a refutation of the predictions of a theory by an experiment, would have to admit, not that the theory is false, but that the theory is not a good instrument of prediction.

147 Popper (1962: 104)
148 Popper (1982*b*.: 102)

149 (*Ibid.*: 103)

150 (*Ibid.*)

The instrumentalist therefore would *not* use the fact that an instrument 'raises no claim to the truth' in order to escape the refutation as Popper suggests.

The antirealist would hold that since all the scientific theories of the past have turned out to be false, the same should be the case for our present theories. Here the realist might respond that this is not incompatible with the realist position; on the contrary it is presupposed that the only way of obtaining knowledge, progress and truth is by assuming that our present theories are always subject to revision by future developments. But here comes the uniqueness of quantum mechanics as this is a theory with a huge predictive success which resists any attempt of a 'realist2' interpretation, and it might seem rational to opt for an instrumentalist attitude towards it—one that does not try to capture the reality behind the quantum appearances. In light of this, Bohr's philosophy could be understood as an attempt to block the confusion between epistemology and ontology. But it seems that his philosophy also contains an ontology that holds the paradoxes as inherent to the nature of the micro-objects and as such they preclude any possibility of a realist interpretation. Popper argues that even if some statements pertaining to the quantum domain are 'undecidable', this still does not warrant dropping the 'classical ideal' and 'physical reality' as Bohr wants.

Popper offers the following argument against the orthodox view:[151] By Heisenberg's admission the collapse of the wave packet is a consequence of the transition from the possible to the actual. This means that in a way the theory applies to the actual world via this reduction. But, again by Heisenberg's admission, this reduction cannot be derived from Schrödinger's equation. Consequently, Popper wonders how one can reconcile the claim for the completeness of the theory and the admission that we can only apply the theory to the 'actual' based on a step

[151] (*Ibid.*: 123)

that is not derivable from the theory.

This, together with a host of contradictions, anomalies and circular arguments, constitute for Popper the 'great quantum muddle' that, he thinks, result from the confusion between epistemology and ontology. A major part of responsibility for this goes to Bohr who might be re-construed as having shifted from a moderate position, according to which there are limits to our present knowledge, to a strong instrumentalism that does not allow for any realist ontology. Popper notes that in the passage from one position to the other, Bohr reasons erroneously from the limits of our present knowledge that stems from the irreducible uncertainties for this knowledge to a position where those uncertainties become part of the ultimate nature of the quantum objects.

Popper specifically thinks that the muddle arises from confusing the properties of the sample space of a population with those of the individual elements of the population. He explains what he means by that by giving a short exposition of some elements and assumptions of statistics.[152] First, we have a 'population' of possible events. Second, the physical characteristics of the situation under study determine the width of variation in those possible events: If we flick a coin there are only two possible events, if we throw a die there are six. Third, the possible event are called the points in the sample space. Forth, each point in the sample space is associated with a number that is determined by the distribution function. This distribution function is a characteristic of the sample space, not of the elements of that space. Popper gives the following example: The population of, say, England is distributed in various geographical areas of England. The distribution function is a property of the population, not of Mr Smith. Mr Smith has a probability to live in this or the other county, but those probabilities are not

[152] (*Ibid.*: 50–1)

Mr Smith's properties; they are properties of the population. Mr Smith has the property of residing in Oxford. But the population as a whole has the property of the probabilities given by the distribution function.

Thus, Popper thinks that the 'quantum muddle' is due to thinking of the distribution function as a property of the individual quantum systems, rather than the population they consist. This results in confusing the limits of the knowledge we can have concerning the sample with a fundamental and inescapable limit on knowledge *per se* as part of the quantum reality.

1.5.8. Rationale

The end product of the endeavours of the classical physicists was a highly developed mathematical structure that could deal with all the known macro phenomena, that could easily be integrated into a logical structure and that could admit a realistic interpretation. The advent of quantum physics brought a radically new physics with a different logical structure and with its own orthodox interpretation that seems to defy any attempt for a realistic picture of the world. The new quantum orthodoxy denied even the search for physical reality.

Einstein and Popper questioned whether this denial was a legitimate one, or whether its acceptance was the result of a successful propaganda campaign. They never accepted this orthodoxy, but they did this not on 'religious' grounds but by questioning the rationality of the new orthodoxy, and suggesting thought experiments that purported to show the incompleteness of the theory, in Einstein's case, or that the lesson of the uncertainty principle is epistemological, rather than ontological, in Popper's case.

Out of their effort came their correlation experiments. The EPR experiment had a very profound effect and its reverberations are still felt today, both in the physics and in the philosophy community. Countless articles and books have been written with the EPR experiment as their main, or one of the main, subject.

Popper has also devised a correlation experiment similar to the EPR experiment. But the *Popper experiment* for a number of years received far less attention than EPR, even amongst philosophers of science, let alone by physicists. Only recently, in 1999, some two decades after its proposal, was a realisation of Poppers experiment conducted. Its results are still debated and, I claim, not fully understood. From Chapter 3 onwards, the rest of this thesis, will try to look into the conceptual and the concretised experiment, and the reactions to them. But, before that, Chapter 2 gives a critical look at Popper's ideas.

Chapter 2: Criticism of Popper's Views

This chapter critically assesses some aspects of Popper's philosophy. As noted earlier, most of the criticism raised against Popper has little relevance to the validity of the argument presented by his proposal of the experiment. Nevertheless, some discussion of his views might be beneficial in providing a perspective for the experiment.

2.1. Scientific Realism and Popper's Confusion

Since Popper's motivation for the experiment is realism, a statement of what realism is, was given at the beginning of Chapter 1. In fact, in order to facilitate the earlier discussion, two distinct types of realism were described in Section 1.1. The current section looks more closely into the notion. A discussion of Popper's aforementioned confusion regarding truth and explanation is then given in Section 2.2.

Realism is one of the most debated issues in contemporary philosophy, both with regard to its nature and its plausibility. The question of realism arises with respect to a large number of subject matters, varying from ethics and aesthetics to causation, science, mathematics, and the world of macroscopic material objects and their properties. Although one could subscribe to, or reject, a universal type of realism, it is more common to be selectively realist or non-realist about specific topics, and even mix such realist and non-realist positions. Moreover, one can be more-or-less realist about a particular subject matter.

Anti-realists can be of various kinds as well, depending on whether or not it is the existence or independence aspect that is questioned or rejected. Various kinds of anti-realism can take the form of error-theories, non-cognitivism, instrumentalism, nominalism, certain styles of reductionism, and eliminativism, that typically reject existence, while idealism, subjectivism, and anti-realism would concede the existence of objects and their properties, but reject their independence from the knowing subject.

Scientific realism is the common sense conception that the world is independent of our knowledge-gathering activities, science is the best way to explore it, that one is justified in accepting the most secure findings of scientists 'at face value', provided that one recognises the fallibility of scientific research and the approximate character of such knowledge. Specifically, science is about the 'nature of things in the world', it does not simply produce predictions about the outcomes of experiments. And moreover, there is good reason to hold that our successful theories have—at least approximately—captured the 'nature of things in the world'.

An argument in support of scientific realism is that it exerts a beneficial influence on the development of science: It provides strategies for research and suggestions for the solution of special problems. To use one of Popper's examples, Copernicus' claim that his new astronomy reflected the true arrangement of the spheres, raised dynamical, methodological and exegetical problems, since his ideas were in conflict with the current physics, epistemology, and theological doctrine. Copernicus not only created these problems, but also hinted at their solution. Also, the atomic theory raised philosophical, physical, chemical and metaphysical problems and some scientists wanted to abandon it, or simply to use it as a scheme for the ordering of facts. Realists developed it and showed the limitations of such a purely phenomenological view. Einstein's criticism of the quantum theory initiated interesting theoretical developments and delicate experiments, and clarified the basic concepts of the theory. These are cases where scientific realism gave rise discoveries and contributed to the development of science.

Specifically for the Copernican case the issue is about the truth of theories. Aristotelians had physics to provide them with answers about the structure of the world, but Copernicus theory required a new physics. So there was a clash between two realist positions making different claims to truth. So a realistic interpretation had to be provided only for the theories that have been chosen as a basis for research: Kepler thinks the chosen theory has been shown to be true as Copernicus' theory does not simply fit the facts; in any case a false theory can do that. But it has also led to novel predictions and these remain true when applied to similar topics to the ones where success was achieved.

There have been some important philosophical arguments against the position of scientific realism. First, there is the empiricist argument regarding knowledge of unobservable theoretical entities and the underdetermination of theory choice by observational data. Second, Kuhn's argument that the realist idea of the growth of approximate scientific knowledge cannot be sustained, given the semantic and methodological incommensurability that results from revolutionary changes in science. Third, there are Putnam's internal realist and Fine's natural ontological attitude positions, both of which present critiques of the metaphysical versions of scientific realism. And fourth, the post-modern challenge (to both realism and empiricism) that is grounded in the idea that such phenomena as science, knowledge, evidence, and truth are all social constructions.

2.2. Scientific realism after Popper

There has of course been an enormous amount of discussion of the exact nature of, and arguments for, 'scientific realism' after Popper. Various realist positions such as Worrall's 'Epistemological Structural Realism' (itself firmly and explicitly based on Poincaré's much earlier version), and Ladyman and French's 'Ontic structural realism'—have emerged, while versions of anti-realism much more sophisticated than 'instrumentalism' as Popper understood it have been developed—notably van Fraasens 'constructive empiricism'.

How does Popper's 'third view' relate to these newer portions? And how, if at all, do these relations have an impact on the significance of Popper's proposal for an experimental test of the orthodox interpretation of quantum mechanics?

Given the complexity of the current 'scientific realism' debate the first question is, of course, itself complex. But fortunately I need do no more than briefly sketch out part of the answer to that first question since the answer to the second question—concerning the impact on the experiment to test quantum mechanics—is 'little if anything'.

Let me first explain this remark about the second question and then turn briefly, and sketchily, to the first.

As we saw earlier (see in particular the quotation from Redhead), the 'realism' that Popper wanted to see regained, but saw as lost in quantum mechanics (as orthodoxly interpreted), was a very special kind of realism. That is what has been identified as 'realism2' in the beginning of this thesis. The reason why Popper confuses the two types of realism, and thinks that realism goes beyond realism1 to include also realism2 is related to his confusion that was referred to in section 1.5.7, concerning truth and explanation.

Given that scientific realism is tantamount (in the realism1 sense) to the idea that the world is independent of the knowledge gathering activities of the knowing subjects, and the currently held best scientific theories can be taken as approximately true descriptions of the world, all that the realist has to say is that our current theories are reasonably held to be approximately *true*. There is no need, as Popper does, to venture towards any position; instead the realist can remain neutral on all issues concerning explanation. As noted earlier, Popper in his 'Three views' consistently presents the realist as constantly demanding explanations, and to bring (at least implicitly) some sort of 'does it make sense' considerations into whether something counts as an explanation or not.

Furthermore this is inconsistent with some of the things Popper says elsewhere. For example, the demand for 'making sense' means that a scientific theory should provide an understanding that is compatible with some already established metaphysical frameworks. This is the reason why Popper insists in identifying realism with ralism2: He is looking for some explanation based on some established metaphysical framework that holds precise trajectories as fundamental. But this contradicts Popper's recognition that scientific revolutions often rewrite what counts as 'making sense' and 'common sense'. Hence, we have in the quantum context Popper's failure to recognise the possibility of realism1 with regards to quantum theory.

All that the realist needs to do is to read realism off the quantum theory; this would be realist1 but not realist in the sense that Popper uses, i.e. realism2. This is because realism1 would not 'explain' why whenever a measurement is performed one gets a precise value of position or momentum; but one cannot get a precise value for both of them; it would just assert that this is the case 'without explanation'.

So then, very briefly, how does Popper's 'third view' relate to current conceptions of scientific realism? Certainly as far as epistemic structural realism goes, it has no prior commitment to any particular metaphysical framework and therefore would not insist on a 'realist' interpretation of quantum mechanics reinvoking 'classical' space-time positions.

Popper's 'third view' taken independently of any further 'realism' reported into his view of quantum mechanics is, as Worrall most directly argues in his (1982), (examined in the next section), compatible with (at least sophisticated versions of) anti-realism. Only if some positive 'epistemological ingredient', to use Newton-Smith's term, is added—saying something positive about the way the theory relates to the universe (as opposed to just saying that it has so far not been repeated) does the position count as 'scientific realism'.

2.3. Worrall on Popper

Given the challenges to realism noted above, Worrall notes that

> "no one has insisted more emphatically than Popper that theories transcend the empirical evidence, that revolutions have played a vital and irreducible role in scientific development and, at the same time, that theories should be interpreted realistically. So if indeed transcendence and revolutions pose problems for realism, we should expect to find some sort of attempted solution of those problems within the Popperian approach."[153]

Worrall notes that there were two contributions made by Popper in the realism-instrumentalism debate: "he developed certain arguments against the instrumentalist view of scientific theories... [and] he has developed his own positive view of the status of scientific theories, a view which might be called "conjectural realism"."[154]

In examining Popper's various attacks on instrumentalism, Worrall finds that "none goes through against better versions of the doctrine."[155] Those better versions of the instrumentalist doctrine are for Worrall those of Duhem and Poincaré. Worrall, after carefully examining the positions of those two scientists, finds that Popper's arguments are rather superficial and not strong enough to affect a "collapse" of the instrumentalist position that they hold, as Popper claims. Worrall concludes that "the choice is not the straightforward one between a heuristically powerful realism and a heuristically sterile instrumentalism; rather these two philosophical positions carry conflicting accounts of the main driving force of science. If realism really is superior on heuristic grounds, this remains to be proved".[156]

To start with, the list of people that Popper includes under the banner of instrumentalism (Osiander, Cardinal Bellarmino, Bishop Berkeley, Mach, Kirchoff,

[153] Worrall (1982: 202)
[154] (*ibid.*)

[155] (*ibid.*)

[156] (*Ibid.*: 212)

Hertz, Duhem, Poincaré, Schlick, Wittgenstein, Bridgman, Eddington, Bohr and Heisenberg) is quite extensive and covers a variety of positions that could not easily be unified in a single one. Moreover, since not everyone in the list would agree with all of Popper's characterisations of instrumentalism, Worrall thinks that it is best to use a broad characterisation of the concept that most of the people in the list would subscribe to and says that: "the highest level scientific theories, in so far as they transcend all empirical data, have no straightforward descriptive import".[157]

Next, Worrall notes that Popper's motive behind his criticism is his belief that instrumentalism "threatens the dignity and importance of science",[158] but by citing and analysing passages from Duhem and Poincaré he argues that they also have the dignity and importance of science at heart. And to sketch their position as implying that we do not need to learn more about the world, as Popper does, is for Worrall "a caricature of their position", as "[t]hey both gave reasons why science will, and must ever push ahead".[159]

Popper's main argument against instrumentalism—that the doctrine cannot account for the difference between "pure" theories and technological computational rules because there is nothing corresponding to attempts to refute theories for computational rules—is dismissed by Worrall because it is "entirely circular":[160] It is true that according to the instrumentalist position there isn't anything that is equivalent to falsification, but that is because the doctrine does not recognise the categories of truth and falsity as applying to theories. This only means, though, that instrumentalism differs from Popper's falsificationist

[157] (*Ibid.*: 203)

[158] (*Ibid.*: 204)

[159] (*Ibid.*: 205)

[160] (*ibid.*)

account; it does not mean that instrumentalism does not account for high-level theory testing, and indeed "[t]here is... the attempt to find out the power of the theory, or to find out how comprehensive it is".[161] A theory that turns out to be in disagreement with the facts will have to be rejected, "not as false, but as empirically inadequate".[162]

Worrall recognises though that, although Popper's argument is flawed, there is, in what Popper says, the important underlying thesis that "instrumentalism is *heuristically infertile*—that those scientists who adopt it will, in general, do less good science than those who adopt a realist view".[163] Popper's idea that instead of recognising falsifications as such, instrumentalism may use them as indications for the theory's limitations, is interpreted by Worrall as saying that instrumentalism will react to refutations "by simply making an *exception* of the specific circumstances in which the refutation arises".[164] But this charge does not harm the type of instrumentalism that Duhem or Poincaré would support, as they would not allow such *ad hoc* exceptions under their maximum unity and simplicity seeking methodologies. Despite the fact that Worrall accepts that neither Duhem nor Poincaré succeed in providing general accounts of these notions, he counters that the realist also needs them.

Worrall recognises that Popper's methodology condemns such *ad hoc* responses to falsifications. For example, Popper's methodology would not allow the difficulties with Mercury orbit to be dealt with by allowing all planets but Mercury to follow Newton's laws. But then Worrall asks: "what if God decided—for the sake of variety and in order to discomfit presumptuous mankind—to create

161 (*ibid.*)

162 (*Ibid.*: 206)

163 (*ibid.*, emphasis in the original.)

164 (*Ibid.*: 206–7)

a world that is generally Newtonian, but with one or two exceptions"?[165] This would mean that Popper's methodology would became difficult to reconcile with his aim for a realistic description of the world. Worrall then says that a Popperian might respond by allowing the methodology to lead us away from the truth, but adds that "but practically speaking, there is no doubt that [Popper] discounts this possibility"[166] and that "he implicitly assumes that Nature itself is "simple and "unified"."[167] The realist needs this assumption as much as the instrumentalist does.

The first reason has to do with the reaction of the realist to high-level problems in his theories. Because these are high-level problems the instrumentalist would be blind to them, as they do not arise out of some disagreement with empirical facts, or out of some logical inconsistency within the theory. So they must be "incoherencies", and as such they must stem from some clash with some previously held metaphysics. The claimed extra heuristics of realism must force a modification of the theory. He cites as an example the clash between Newton's theory and Descartes' mechanistic metaphysics, that led to a great deal of effort and did not bring any fruit, except maybe a revision to the notion of mechanistic explanation.

But, says Worrall, this need to revise a theory when it clashes with a metaphysical belief, should not be considered as a necessary part of scientific realism, since the realist should only insist that "his present best bet is that the world *is* the way his present best scientific theories tell him it is, and if this clashes with previously held general metaphysical views, then this may indicate the need to

165 (*Ibid.*: 208–9)

166 (*Ibid.*: 209)

167 (*ibid.*)

revise those views".[168] Just because the critics of positivism have shown that in some cases 'meaningless' metaphysics had aided scientific advancement, does not force on the realist camp the thesis that clashes with metaphysics lead to theory revisions. Worrall also cites quantum mechanics as an example where he says that "attempts to reconcile a scientific theory with previously held general views about the world have proved scientifically unfruitful".[169] So, Worrall concludes that those 'incoherencies' provide the first reason why the idea that realism is heuristically more fertile needs to be treated with care.

The second reason why Popper's claim for the heuristic superiority of realism has not been proven has to do with Duhem's and Poincaré's claim that theoretical advances most often come about from mathematical and symmetry considerations. For example, physicists might be guided by the analogies in the equations in two distinct fields and develop them further. Or, as Worrall points out, "according to Poincaré, we owe the great breakthrough in electrodynamics to the fact that Maxwell was "steeped in the sense of mathematical symmetry"; hence he looked at the current electrodynamical laws "under a new bias" and "saw that the equations became more symmetrical when a [new] term was added" (The Value of Science, 78)."[170] The new term is only given a realistic interpretation after the theoretical work was done, and this according to Duhem and Poincaré, happens in most cases. This provides an alternative account for the heuristic driving force of science, so Popper's charge of heuristic infertility cannot stick, or at least it remains to be proved.

The conclusion so far is that Popper attacks instrumentalist positions that are weaker than the ones presented by Duhem and Poincaré. What of Popper's

168 (*Ibid.*: 210)

169 (*Ibid.*: 211)

170 (*ibid.*)

positive proposal; the "third view"? Can it survive the instrumentalist's criticism? This position, after contrasting it with those criticisms, is reduced by Worrall to "a genuinely conjectural realism—a position which forms the core of Popper's view of scientific theories",[171] i.e., to the view outlined earlier, that scientific theories always *attempt* to describe the real objective world; but while they *may* succeed in doing so, we can never know this—it is always possible that they will systematically be refuted and replaced by a still better theory which implies that they are strictly speaking false.

This position, Worrall finds, is so 'unassuming' that some critics might find it that "it is scarcely worth distinguishing from instrumentalism".[172] But there are differences: "conjectural realism does allow high-level theories to be true-or-false in the usual correspondence sense".[173] Furthermore, it makes the epistemic assumption that "our presently best theories (according to our decidable methodological criteria) are our present best guesses about the truth (which is of course not decidable)".[174] But, Worrall continues, "the main argument for conjectural realism is, as Popper discerned, negative. Its virtues only become visible when it is compared with its rivals".[175] Finally, he concludes that the best feature of conjectural realism is that its differences from the Duhem and Poincaré instrumentalism are only such that they are "consistent with the facts of scientific development, whilst at the same time adding enough to allow us to follow our realist inclinations".[176]

[171] (*Ibid.*: 229)

[172] (*Ibid.*: 230)

[173] (*ibid.*)

[174] (*ibid.*)

[175] (*ibid.*)

[176] (*Ibid.*: 231)

2.4. Discussion

To recap the previous section: Worrall's examination of Popper's arguments for realism finds them wanting. Popper's criticism of instrumentalism does not do so well against the better versions of the thesis given by Duhem and Poincaré. As for his positive thesis, this only holds water in the form of the weakened thesis of conjectural realism.

I would like to turn the discussion now to those of the issues discussed so far that are more relevant to Popper's proposal for an experiment that would test the Copenhagen interpretation of quantum theory and the argument behind it. As noted earlier, Popper admits that his arguments are partly rational, partly *ad hominem*, and partly ethical. In general the above discussion shows that his arguments have more to do with exposing the alleged weaknesses of the opposing theses, and in particular of instrumentalism, and less with building up a detailed account of why in most cases realism is superior to instrumentalism.

But regarding the discussion of the experiment most of these weaknesses in Popper's arguments become almost irrelevant. His motivation—metaphysical realism—might or might not be a flawed thesis, but the validity of his specific arguments might still be intact. Popper directs the specific argument not on proving his side, as he is a falsificationist, but on falsifying the opposing view. The second part of Worrall's paper shows that Popper's positive thesis must at least be weakened, but that is not at issue here. What is at issue is what he discusses in the first part: Popper's attack on instrumentalism. Are any of the points that Worrall makes relevant for the discussion of the experiment? Most are not. But there is one that is directly relevant. This is the issue of the 'incoherencies' in high level theories. For Worrall they provide the first reason why the idea that realism is heuristically more fertile, needs to be treated with care. This is because they give rise to a tension between the theory and some preconceived metaphysical belief, such that it forces the realist to seek a revision of the theory.

It is not clear to me whether Popper will be forced to admit that his methodology forces him to demand that clashes between theory and metaphysics should be resolved by a revision of the theory. But even if this is so, as we shall see later on when examining his reasons and argumentation for the proposed experiment, Popper, at least in the case of quantum theory, does not ask for a revision of the theory. He asks for a realistic interpretation of the theory. He thinks that the theory does not force the instrumentalist interpretation, and proposes an experiment that he believes will show that there are no reasons to bar such an interpretation. So although Worrall's argument against the general Popperian position might be valid, Popper's argument in his proposal of his experiment is at least neutral to this. One might (as Redhead did) object that it is not possible to propose an experiment against an interpretation that is not at the same time against the theory that this interpretation interprets. I shall come to this very important issue in the last section of this chapter, but at least with respect to the issue of forcing a new theory because of the clash with his metaphysics, Popper does not ask for one, at least explicitly; on the contrary he asks for ways to make his metaphysics fit the theory.

There is a sense in which Popper is in agreement with Worrall here. This is with respect to the idea that the most a realist can ask for, is for a realistic interpretation of the best available theory of today; given that the best theory is the quantum theory he asks for a realistic interpretation (in his sense of realism, i.e. realism2) of that theory, should such an interpretation be supported by his experiment. He does not suggest that the outcome of the experiment is a revision of the theory, but he seeks to undermine the reasons that force an instrumentalist interpretation of that theory.

But in the sense already discussed earlier, concerning the realism1/2 distinction, Popper is in disagreement with Worrall in that he wants to make the theory compatible with his metaphysical framework, and provide explanations, instead of taking the theory at face value and reading realism1 off it.

2.5. Propensities

A number of commentators have raised objections to Popper's interpretation of probabilities as propensities. For example, Antony Eagle in his "Twenty-One Arguments against Propensity Analyses of Probability"[177] argues that a dispositional analysis of probability will either fail to adequately explicate probability, or that any such an explication cannot be employed in empirical science. He also argues that the diversity of arguments he presents is such that it is doubtful if anyone can give a successful analysis of the short that is required. Suárez[178] comments that although Popper's interpretation is famous and influential, his attempt to apply his interpretation to the quantum probabilities is fundamentally flawed in that it "attempts to solve the quantum paradoxes by merely *interpreting* the quantum probabilities as propensities",[179] (as opposed to his approach of selective-propensities; selective-propensities for him do not just interpret quantum probabilities, but explain them[180]) and moreover that this 'fundamentally flawed' interpretation "gives all propensity interpretations of quantum mechanics an unfairly bad reputation".[181]

The most influential argument against the propensity interpretation of probability has been given by Humphreys,[182] and is known as 'Humphreys' Paradox'.

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177 Eagle (2004)

178 Suárez (2007)

179 (*Ibid.*: 424)

180 (*Ibid.*: 431)

181 (*Ibid.*: 424*n*)

182 Humphreys (1985)

Humphreys notes that whereas conditional probabilities are symmetric, propensities cannot possibly be. As a consequence they do *not* obey Kolmogorov's probability calculus, and in particular Bayes' theorem.

For a well-defined conditional probability $P(A|B)$ the event B that is used as the condition is not necessary to precede the event A in time. For example one can ask: "What is the probability that $A =$ 'suspect X committed the murder', given that $B =$ 'the post-mortem examination produced such and such evidence'?" Now the event B clearly happens after the event A in this case, and assuming that the relevant probabilities are well defined, then one can use Bayes' theorem:

$$P(A|B) = \frac{P(B|A)P(A)}{P(B)}$$

But if one wants to interpret $P(B|A)$ as the propensity of the event 'suspect X committed the murder' to cause the event 'the post-mortem examination produced such and such evidence', then, given the asymmetry of the causal relationship—the post-mortem evidence do not cause X to commit the murder— it seems impossible to interpret $P(A|B)$ as the propensity of the event 'the post-mortem examination produced such and such evidence' to cause the event 'suspect X committed the murder'. So Humphreys' paradox is assumed to show that there are objective probabilities that are cannot be thought of as propensities. Or that propensities (at least the accounts of propensities that hold that if $P(A|B)$ exists, so does $P(B|A)$) do not or must not obey the probability calculus.

There have also been attempts to resolve the paradox,[183] and Humphreys has commented on those.[184] A review of those responses and the counter response by Humphreys is beyond the scope of this thesis, but it is worth noting a couple of points: First, Fetzer has offered a 'probabilistic causal calculus' that is different

[183] See for example Fetzer (1981), Gillies (2000*b*), McCurdy (2002), Miller (1991, 1994, 2002).

[184] Humphreys (2004)

from the standard Kolmogorov calculus, and second, that Popper's axiomatization of primitive conditional probabilities does not insist on that if $P(A|B)$ exists, so does $P(B|A)$, so his account might not suffer from the paradox. For example, Popper might have wished to distinguish between propensities that have a connection to relative frequencies obeying the usual probability calculus and causal propensities that behave rather differently.

In any case the point made earlier still stands: If Popper's account of propensities falls, it can do that without taking his proposal for the experiment with it.

2.6. Feyerabend on Popper and Bohr

In his (1968, 1969) two-part paper,[185] Paul Feyerabend examines Popper's critique of complementarity and the general ideas of Bohr. In doing so he distinguishes Bohr's position from that of Heisenberg and from other "vulgarized versions which have become part of the so-called "Copenhagen Interpretation" and whose inarticulateness has been a boon for all those critics who prefer easy victories to a rational debate."[186] He starts by summarising Popper's position:

> In his essay "Quantum Mechanics Without the Observer" ([17], pp. 744) Popper criticizes the Copenhagen Interpretation and suggests construing the quantum theory as a "generalization of classical statistical mechanics" ([17], p. 16). The uncertainty-relations, he says, set "limits to the statistical dispersion... of the results of sequences of experiments" ([17], p. 20) and not to what can be said about individual systems. Some strange features of orthodox microphysics are due either to a misinterpretation of probability, involving some "very simple mistakes" ([17], p. 42); or else they are a straightforward consequence of the fact that the quantum theory is a statistical theory. For example, "the reduction of the wave packet... has nothing to do with quantum theory: it is a trivial feature of probability theory" ([17], p. 37). The idea of probability that is presupposed in all these arguments, however, is Popper's propensity interpretation. Adopting this interpretation suffices for making clear what the quantum theory really is, viz. a "generalization of classical statistical mechanics" ([17], p. 16). So far Popper's position as I understand it."[187]

He comments that this is an attractive position, but it fails when it is contrasted with the facts that Bohr takes into account. He also notes the difficulty in establishing exactly what is Bohr's position from the various positions that have been associated with it.

His next point is that there is "an interesting parallelism between the propensity interpretation and complementarity. This parallelism is rather striking for it makes Popper stand much closer to Bohr whom he attacks than to Einstein whom he defends."[188] This parallelism is based on the fact that Popper under-

[185] Feyerabend (1968); (1969)

[186] Feyerabend (1968: 309)

[187] (*ibid.*: 309–10)
[188] (*ibid.*: 311)

stands propensities as being determined by the totality of the experimental arrangement. But, for Feyerabend, this is what Bohr by his notion of *phenomenon.* Propensities or probabilities, for Popper and Bohr respectively, are not properties of individual systems but of the totality of experimental arrangements or phenomena.

With regards to the correspondence principle, Feyerabend notes that this "asserts an agreement of *numbers*, not of *concepts*."[189] Moreover it is not the case that all problems of classical physics can be dealt with using quantum theory. To this effect he point out that it is there are plenty of assertions that the quantum theory makes that are in disagreement with those of classical physics, given that "quantum theory is a *linear* theory whereas classical mechanics (with such idealized exceptions as the theory of small vibrations) is not,"[190] and the approximations of quantum theory for the macroscopic level preserve linearity. This gives rise to some drastic deviations for example in measurements where "a measurement of a magnitude in a system that is not in one of its eigenstates separates these eigenstates... but it does not destroy the interference terms (this is true even if the original state of both the system and the measuring apparatus should happen to be a mixture rather than a pure state)."[191]

This means that the quantum theory, even before we start talking about its interpretation by Bohr and Heisenberg, does not allow us to say that the system is in a particular state. Furthermore, the classical level is not achieved by simply pointing to the fact that those interference terms are not detected at the macro level, because "Such removal changes an assembly of interfering wave

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189 (*ibid.*: 315)

190 (*ibid.*: 317)

191 (*ibid.*: 318)

packets which are jointly occupied by the [system] into an assembly of *isolated* wave packets which however are still jointly occupied by the [system]. And as the classical level is reached only when we are allowed to assign the electron to a *single* wave packet we need a *further* transition which cannot in any way be regarded as an approximation."[192] The discovery that the interpretation of the assembly at the macro level in terms of classical statistics leads to correct predictions allowed the combination of wave mechanics and Born's interpretation. The assumptions involved in this step constitute the 'reduction of the wave packet', and *forced* Bohr to adopt the interpretation that he did.

But, Feyerabend emphasises, although all this can be presented in a very technical language, it should not obscure the fact that it physical facts that force Bohr to propose his interpretation. So when Popper says that the reduction of the wave packet is a feature of probability theory and not of quantum theory, he is misunderstanding the fact that the Born interpretation *is* a prt of the theory. So, Feyerabend continues, Popper takes advantage of the all the efforts of the Copenhagen school and does not appreciate the difficulties involved in the combination of the two:

> "Now the "reduction of the wave packet" which leads from the unmodified quantum theory to the classical level with its propensities contains all these modifications including the approximations which are needed to eliminate the unwanted effects of a realistic interpretation of the principle of superposition... It is therefore much more complex than the simple "reduction" of propensities *for which it makes room.* Of course, there is no observable difference between the two reductions and an instrumentalist will feel no compunction to identify them. But such an identification does little to advance our understanding of the quantum theory."[193]

With regards to complementarity and the uncertainty principle, Feyerabend note that while propensities are meant to be properties of the ensemble and not of individual systems, complementarity also holds that the other dynamical variables

[192] (*ibid.*: 318–9)

[193] (*ibid.*: 321)

should also be removed from the systems and be attributed to experimental arrangement. The uncertainty principle restricts the application of complementary notions, such as position and momentum, to the same experimental arrangements, and since this is the feature that Popper wants to reject, Feyerabend discusses this next.

In order to discuss this point, Feyerabend uses a non-quantum system: A pin board. He uses this example to show that in a case that does not involve a 'quantum muddle', "the qualitative form which we have adopted here the statistical quantum law denies the existence of certain *individual events*... whereas the dynamical considerations entail that such events must occur."[194]

Popper tries to get around this difficulty by pointing to the fact that changes in the conditions bring about changes in the probabilities, but Feyerabend finds this irrelevant. The reason that led to the Copenhagen interpretation is not that there is some change but the kind of that change: There are trajectories that are allowed from a classical point of view that are not entered by the particles. From the fact that Popper does not give an explanation for the interference patern, Feyerabend concludes that he must take the redistribution of paths as a primitive phenomenon. Since each experiment has its own arrangement, it should be associated with its own propensities, and no further explanation is needed. But Feyerabend points out to the fact that

> "This intriguing hypothesis has much in common with an earlier conjecture of Bohr, Cramers, and Slater—and just like this conjecture it is refuted by the experiments of Bothe-Geiger... and Compton-Simon... These experiments show that energy, momentum and other dynamical variables are conserved not only on the average, so that one could postulate a redistribution without asking for some dynamical cause, *but in each single interaction.* "Purely statistical" redistributions are therefore out."[195]

Feyerabend therefore concludes that

[194] (*ibid.*: 326)

[195] (*ibid.*: 327)

"The idea of complementarity is therefore not just the result of having pursued a mistaken program to the bitter end as Popper would want us to believe... Bohr, after all, did consider a purely statistical theory... despite the fact that it was not in line with his one point of view... but then he found that such a theory could not be upheld. True, Bohr was originally interested in a generalization, *not* of classical *statistical* mechanics, but of classical *particle* mechanics. This part of Popper's account is unobjectionable... It was only *after* the *refutation* of this theory that he returned to his earlier philosophy—and this time with very good reasons. This important episode is not mentioned anywhere in Popper's paper—a very unfortunate omission that makes the idea of complementarity appear much more dogmatic than it actually is."[196]

Feyerabend starts the second of this two part paper by giving an exposition of Bohr's point of view. In the process he defends his philosophy from various charges that Popper is raising, such as 'uncritical', subjectivist', etc. He then turns to the uncertainty relations again. He considers again a kind of a pin board that looks very much like the arrangement for the single slit experiment. Classically the balls of this pin board would show a Gaussian distribution, but an interference pattern would not allow certain trajectories.

Having already shown that Popper's solution does not work, Feyerabend considers Bohr's solution: *"particles do not always hare trajectories."*[197] This together with the hypothesis "that the wave theory which uses extended entities connected by a phase instead of particles and their trajectories becomes valid to the extent to which the particle "picture" ceases to be valid, and vice versa"[198] and de-Broglie's relation $\lambda = h/p$, can e used to get a 'semi-quantitative' estimate for the limits of application of notions such as position, momentum, etc. This lead to the uncertainty principle. The difficulties of the individual case are thus circumvented and the uncertainties Δx, Δp, become limits of application of these concepts. Under specific conditions an accuracy grater than the one implied by the principle is simply not a feature of the world. Feyerabend gives an interesting parallelism

[196] (*ibid.*: 328–9)
[197] Feyerabend (1969: 94–5)

[198] (*ibid.*: 95)

here: A melted block of ice simply does not have hardness on the Mohs scale.

The application of the principle for Bohr is not a matter of taste but stems directly from the two hypothesis above. Feyerabend notices that Bohr disagreed with Heisenberg's attribution of the principle to the Compton effect, as such an interaction would permit us to picture trajectories. The uncertainties appear in specific arrangements and are features of those arrangements. Here, Feyerabend points out:

> It is to be admitted that Bohr himself quite often uses subjectivistic terms, speaking of a restriction of "knowledge," and so on. However, once we go beyond this cloak of words we find that the discussion is not about lack of knowledge, but about objective conditions of the individual case."[199]

Feyerabend then turns to two objections raised by Popper. The first has to do with the interpretation of the uncertainties. Popper wants to interpret them as standard deviations of well defined quantities, as that is the only thing that is needed to derive the relation from Born's rule. But Feyerabend points out that this interpretation can only be given if "the elements of the collectives with which we are dealing in the quantum theory are all in a state that is well defined from a classical point of view, i.e. provided we already know what kinds of entities are to be counted as the elements of the collectives."[200] But the statistical character alone is not sufficient for deriving the that these elements are systems which are in classically well-defined states. Since the statistical character only tells us that we are dealing with a collection, but nothing abut the nature of the individuals in the collective. This is over and above the statistical character. So the attribution of well defined trajectories comes in addition to Born's interpretation; it does not stem from it. And this addition is inconsistent with the conservation laws and with the interferences observed. The second of Popper's objections is that precise

[199] (*ibid.*: 96–7)
[200] (*ibid.*: 97)

position and momenta are needed in order to test the predictions of quantum theory, especially the uncertainty relations. But this again is shown not to be the case.

Feyerabend closes his paper by finding Popper's criticism of the Copenhagen interpretation irrelevant and Popper's interpretation inadequate. The reason why Popp's criticism fails is because he is not taking into account all the facts that Bohr had dealt with. Bohr (especially) and Heisenberg not only do not commit the mistakes that Popper accuses them of, but have actually warned against them. Popper's view although interesting, does not hold against close scrutiny. Moreover Bohr is found by Feyerabend to be open-minded concerning all sorts of suggestions, some of which similar to Popper's, and try them out. Only when these did not work, would he reject them. And whereas its true that mistakes have been committed by the advocates of the Copenhagen interpretation, the way forward is not to commit similar ones, as Popper does for Feyerabend, but to present all the arguments and try them out.

It should then be quite a welcomed suggestion for Feyerabend the one made by Popper when presenting his experiment. Because here Popper proposes the very thing that Feyerabend suggests: a test of the ideas that Popper does not like via the use of an experiment.

2.7. Theory and its Interpretation

It was mentioned earlier that Popper sees two methods for a possible revision of the theory: Either to alter and/or generalise the formalism, or to interpret it and gain some physical understanding of its subject matter. The issue of the relation between a theory and its interpretation is quite central here: In Section 1.5.7 it was mentioned that Popper has made specific predictions for the outcome that he thinks would refute the Copenhagen Interpretation while leaving the quantum formalism intact. But is this an achievable goal for the experiment? Michael Redhead asks if that is even logically possible.[201]

The issue here is whether one can use the outcome of an experiment to refute not the theory itself but just the interpretation of that theory. With respect to the two methods for revision of a theory that are possible for Popper, the question is which one is he suggesting when he proposes his experiment: Is Popper suggesting revising the formalism, or is he suggesting an interpretation of the theory that is not refuted by the experiment? From his predictions that are meant to refute the Copenhagen interpretation, and promote his own, it seems that it is the latter that he has in mind. But Redhead's point is the following: If an interpretation is meant to provide an understanding of the subject matter of the theory, then how is it possible for an experiment to produce results that agree with the theory itself, but not with its interpretation that is after all meant to help in the understanding of these very results?

Redhead discusses the issue of the interpretation of quantum mechanics,[202] and he distinguishes one sense of what he calls the *minimal instrumentalist interpretation* that only gives the relation between measurement results and the formalism on the one hand, and a different sense that is "some account of the

[201] Private communication, September 2004

[202] Redhead (1987: 44–45)

nature of the external world and/or our epistemological relation to it that serves to *explain* how it is that the statistical regularities predicted by the formalism with the minimal instrumentalist interpretation come out the way they do".[203] Redhead also considers that the formalism together with the minimal instrumentalist interpretation in the first sense can be thought of a minimal instrumentalist interpretation in the second sense.

Redhead considers the question of why we need an interpretation of the second sense, but without going into the specifics of the arguments for and against an instrumentalist interpretation. Redhead assumes that without an interpretation of the second sense, i.e. one that explains, one is simply left without an understanding of the natural world. He recognises that one can immediately run into trouble when looking for an explanation: "If X explains Y, then we can ask: What explains X? If we just have to accept X, then why not leave X out altogether and just accept Y?",[204] but he avoids the discussion on the nature of explanation as this is beyond his scope at that point. What he does offer in the sense of a motivation for explanation is a specific feature that X must have: "Suppose we accept, as a necessary condition that X explains Y, that Y is logically deducible from X. The question then is, what features of X would lead us to feel that cutting off the demand for understanding Y at the level of just accepting X is an advance on just accepting Y as it stands? The key here seems to be the "unifying effect of X. A few general principles about the nature of reality expressed in X comprehend a wide variety of seemingly unconnected observational regularities, including Y."[205] That is, Redhead considers unification to be the motivation that would make us look for an interpretation that would provide more than the minimal instrumentalist

[203] (*Ibid*)

[204] (*Ibid: 45*)

[205] (*Ibid*)

interpretation. Since the quantum phenomena are so distinct from the classical ones, one can easily see why such a feature would be an attractive one. Redhead also mentions another feature, that of 'picturability', that demands that X must have an analogue model, but he does not insist on having this feature, although he recognises that it has played some role in various attempts for an interpretation.

He then illustrates the differences between three interpretations of quantum mechanics by considering the answer that they would give to the following question: "'What can one say about the value of an observable, call it Q, in QM when the state of the system is not an eigenstate of Q?'"[206] Redhead lists the three answers as follows: Hidden Variables interpretations answer that "Q has a sharp but unknown value", Propensities and Potentialities interpretations answer that "Q has an unsharp or 'fuzzy' value", and Complementarity answer that "The value of Q is undefined or meaningless".[207] The first two views are, for Redhead compatible with realism, and at least the first, an anti-idealist one.

Here lies an answer to Redhead's question posed earlier: Is it even logically possible for an experiment to refute the Copenhagen Interpretation while leaving the quantum formalism intact? Popper *could* have answered 'yes because an experiment could be devised in such a way as to show that "Q has a sharp but unknown value", thereby refuting the answer given by the Complementarity view'. This is exactly what Popper predicts will happen upon performing his experiment: A measurement of position will take place together with a measurement of momentum, and the values measured would be sharp, or in any case 'sharper' than what the Complementarity view would allow for. We will assess this later.

Another way of giving this answer is to think, à la R.I.G. Hughes, of an interpretation as the answer to the question "What must the world be like if

[206] (*Ibid*)

[207] (*Ibid*)

this theory accurately describes it?"[208] If there are incompatible ways for the world to be like X_1, X_2, etc., all of which, as Redhead supposes, have Y logically deducible from them, and the experiment finds that the world is like what some of these incompatible ways hold and unlike what some of the others hold, then this experiment can be thought of as a crucial test between those interpretations, and not as a test for the theory.

It is crucial to recognise here that, as Redhead says, for X_i to explain Y, Y must be logically deducible from X_i, but not the other way round. The answer to the question of whether it is possible to refute only an interpretation and not the theory that it explains would necessarily be negative if X_i was logically deducible from Y, but this is not so. But Popper argues that an interpretation that tells us more about the world than the theory does, must have some content over and above the content of the theory and the experiment might exploit this excess content of the interpretation, without refuting the theory itself.

Furthermore, the Aspect experiment, while agreeing with the predictions of the formalism, seems to have imposed limitations on what an *interpretation* of quantum mechanics must look like if it is to be compatible with its findings, and thereby refuting classes of interpretations that are meant to explain the theory itself. It would then seem odd to hold that there experiments that suggest that a certain type of interpretations have to be excluded, but there cannot be experiments that could, in principle, exclude certain other types of interpretations.

In this specific case the question is the following: Does the formalism of the theory together with the minimalist interpretation imply the application of the uncertainty principle to indirect measurements as well as to direct ones? It seems clear, given the discussion of Fine on EPR, that Bohr was forced to the former

[208] Hughes (1992: 296)

only after the EPR paper and in doing so retreated to a positivist position. It is only when he admits to the semantic disturbance caused by the indirect measurement that he applies to indirect measurements. Up to that point he talks about a physical disturbance that can only be caused by a direct measurement. Unless the application to indirect measurements is considered part of the minimal interpretation, then there is no reason for Popper to think that his experiment test the theory, rather than the Copenhagen interpretation, and it seems clear that the concept of semantic disturbance is part of the Copenhagen interpretation rather than the minimalist one.

It this precisely this jump between physical to semantic disturbance that Popper wants to test. It was indicated by Feyerabend that Bohr adopted the interpretation that he did only when he had tried everything else and he was forced by the experiments to do so. In this case it seems that he was forced to the positivist position regarding semantic disturbance by a thought experiment. An experiment that would test if the uncertainty principle should be applied to indirect measurements—provided that it does that—should then be a welcomed one.

Chapter 3: The Popper Experiment

In order to test the proposition that the knowledge of the experimenter is enough to create uncertainty, as this is suggested by the Copenhagen interpretation, Popper suggested his experiment[209] which is very similar to the EPR experiment. In this chapter the experiment together with its suggested outcomes will be described in detail, it will be analysed, and a discussion of some of its technical problems will be given.

As noted earlier, Popper distinguishes two types of measuring experiments and, accordingly, two types of EPR experiments. The first type involves what he calls non-classical measurements, and the second, classical ones. Before we see what the basis for this distinction is, it is worth looking into two other distinctions—as the three distinctions are interwoven—which Popper makes between predictive and retrodictive measurements and between two kinds of state preparation. He mentions in a footnote in the *Schism* that retrodictive measurements are ones "which may strongly interfere with the particle and even destroy it, and of which Heisenberg had suggested that they may be, because of their non-predictive character, meaningless"[210], and mentions as an example of a retrodictive measurement the recording of a particle on a photographic plate. On the other hand we have predictive measurements that leave the system in a specific state so that we can predict the results of future measurements on the variables which characterize the observables of that state.

Regarding the distinction between two kinds of state preparation, Popper says that the first kind consists of merely selective state preparations. The second kind may impose new properties on the system. Popper gives the example of selecting

209 Popper (1982*b*: 27–30)

210 (*Ibid.*: 23*n*)

the state by passing the system through a polarizer. In this case we have the production of scatter to the observables that are characterized by the variables that do not commute with those of the prepared state.

Classical measurements are either of the retrodictive (in the sense that they do not allow the prediction of the future state of the object) or of the merely selective kind of measurements and do not impose new properties on the on the system, whereas the non-classical ones impose new properties on the system. Non-classical measurements, like the measurement of the spin of a particle, change the state of the measured object, whereas in classical measurements, like in position measurements, in general they do not.

Popper notes that the original EPR experiment involves classical measurements, whereas the Bohm version involves non-classical measurements. He further notes that Aspect's realisation is a realisation of the Bohm version of the EPR experiment. Although Popper does not see it as likely that the two types of experiment may have different results, he does not exclude the possibility altogether.

It is worth noting a couple of points concerning the possibility that the two types of experiment may have different results. For Popper "the EPR 'thought experiment' is only an argument, not a real experiment".[211] The experiment had the aim of establishing that knowledge *per se* is not sufficient to create 'uncertainty' and scatter, as the Copenhagen Interpretation would hold. The alternative for him is that it is the physical situation which is responsible for the scatter and further that a particle may always possess position and momentum at the same time. On the other hand the Bohm version, and the Aspect realisation of it, tests locality. So, provided that the UC holds, if the two types of experiment

[211] (*Ibid.*: 27)

do indeed have different results, and favour both non-locality and Popper's own prediction for his experiment, then this could indicate that a particle may possess at the same time position and momentum *and* that non-local theories should be favoured over local ones. (As discussed earlier, Popper thinks that if the Aspect results are corroborated even further, then we should favour the Lorentz over the Einstein interpretation of relativistic formalism.)

Popper's first attempt to present such an experiment was in 1934,[212] in his scientific debut, before the publication of the EPR experiment. This old version had mistakes which he admitted, and so by 1978 or 1977 he had the newer version that is presented in the *Quantum Theory and the Schism in Physics.*[213] The later form has obviously taken into account the long discussion of EPR, and can be regarded as a simplification of this experiment. In fact the editor of the *Schism* has added a note where he mentions that Max Jammer[214] examines the possibility that Einstein, who picked out the mistakes in Popper's first experiment, could have even been influenced by it. There is no definite conclusion but Popper rejects the idea: "the possibility that a *gross mistake* made by a nobody (like myself) may have had an influence on a man like Einstein never entered my head"[215] he says in a letter to Jammer.

To recap, the aim of Popper's experiment is to test whether knowledge alone is sufficient to create 'uncertainty', and to establish that a particle may possess position and momentum at the same time.

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212 Popper (1934)

213 Popper (1982*b*)

214 Jammer (1974: 178)

215 (*Op. cit.*: 15*n*)

3.1. The Experimental Setup

The setup of Popper's experiment is rather simple. To recap, at the centre of the experimental arrangement there is a source S of, e.g., positronium, or any other element that will disintegrate and produce pairs of particles that are emitted in opposite directions from the source. There are two screens, A and B, one on either side of the source. The x-axis runs from the source to the right (positive) and from the centre to the left (negative); and the y-axis runs up (positive) and down (negative). On the screens there are slits, parallel to the y-axis, and these can be opened and closed by varying their width Δy. Additionally, the screen on the left can be removed. Behind each of the two screens there is a battery of Geiger counters, arranged in a semicircular fashion, the center of each semicircle being at the center of the slit in each screen.

Essentially this is like the Aspect experiment but with slitted screens instead of polarisers. The Geiger counters are coincidence counters (counting particles that have passed at the same time through A and B and have arrived simultaneously at the counters) and, assuming a very low intensity of the beam of the emitted particles, there is a high probability that two particles recorded at the same time are the product of a single interaction at the source. On this Popper mentions: "[t]his should make it almost certain that only pairs of particles which have interacted are recorded".[216]

Further, Popper thinks that given that each pair of correlated particles can be considered to have interacted in an extremely small region, even if the source is somewhat extended there is no need to keep the source S in the centre very small. Since the interaction region is rather small—in this case the cross section of the interaction that is of the order of the particles' diameter—he considers this

[216] (*Ibid.*: 27)

sufficient for the argument, and thinks that there is no need to identify this small region within the perhaps somewhat larger region occupied by the source.

Given this setup, Popper looks first at what happens if one considers only one slit. If we make the slit small, by detecting a pair of particles, in effect we measure (or "select" in Popper's language) the q_y (position) coordinate, since we know that the particles must have gone through their respective slits. The width of the slit provides a measure of the "accuracy" of Δq_y for each particle. So the narrower we make this slit, the smaller Δq_y becomes; and, according to Heisenberg, the smaller Δq_y becomes, the greater will be the "uncertainty" (or "the statistical scatter" for Popper—Popper insists on this as, according to him, it makes the assertion a realistic and testable prediction) of the momentum Δp_y. Consequently there will be a Δp_y, related to the Δq_y according to the Heisenberg relations (or scatter relations for Popper). So, says Popper, if the one slit we are looking at is wide open, then only the counters near the centre will be "firing"; but if we almost close the slit, then some counters at far larger angles will also begin to "fire", by detecting the particles that have the larger uncertainty in momentum.

For Heisenberg this "uncertainty" in momentum is due to his indeterminacy relations. For Popper one does not need to worry about these indeterminacy relations: From the realist point of view one can just agree to the fact that there is a scatter due to the presence of the slit, and that the width of the slit determines the magnitude of the scatter: The projection of the scatter upon the y-axis gives Δp_y, which is the range and scatter of the momentum in the y-direction. So for the realist there will be a Δp_y, that is related to the Δq_y, and this relation will be in accordance to the known Heisenberg relations.

Having considered one slit, the discussion can be extended to the case of two. Popper recalls that the EPR paper contains an argument that has been accepted by Bohr and was elaborated by Schrödinger. This argument, which has never been contradicted in Popper's opinion, says that, if we measure the position of the particle A on the right along the y-coordinate, we obtain information, about the y-coordinate of the correlated particle B on the left. That is to say, if we measure the position of the interacting particle going to the right, we obtain information about the position of the correlated particle going to the left. This claim can be checked by performing appropriate measurements under different arrangements of the apparatus. So, with the screens in place on both sides, the Geiger counters to the right and left of the slits, since they are wired as coincidence counters, can be used to measure the width of the scatter.

Now Popper thinks that by removing the screen on the left one can obtain an EPR situation. The removal of the screen and the detection of a particle on the right (the side with the screen), means that there is a "measurement" of y of the particle on the right side. This y measurement yields information for the y-position of the correlated particle that has gone to the left.

According to Popper, the EPR argument tried to establish that a particle possesses sharp position and momentum, and thus a trajectory; and our *knowledge* of a particle's position cannot, by itself, disturb its momentum: The particle's momentum remains undisturbed. This means essentially that our *knowledge* has no physical consequences and that it cannot disturb the momentum of a distant particle in the same way that a screen would disturb the momentum.

On the other hand, according to Popper the Copenhagen Interpretation, and specifically Bohr's replies to the EPR argument, suggest that our *knowledge* of the position of B (obtained by measuring A) must, *merely as knowledge*, make the momentum of B "indeterminate", since no particle can have both. But if this is so, then *our knowledge would make the momenta on the left scatter* upon repetition (even with the screen on the left removed).

3.2. The Outcomes of the Experiment According to Popper

Given the set-up of his experiment, Popper considers the possible outcomes of the experiment.[217] He does not give a detailed derivation of those outcomes based on a given wavefunction; rather he *argues* from the well known facts about the one-slit experiment and his conjectures about the results that will follow from the particles passing through a virtual slit. He thinks that there are two possibilities of which the first would favour and the second would not favour his realistic view of quantum theory.

1. First, the possibility Popper believes to be the outcome of the experiment, and also the prediction of quantum formalism: The possibility that the particle on the left side (where there is no screen) will hit one of the central counters, while its twin on the right, after passing the slit, will show some considerably bigger scatter. This would indicate that nothing has happened to the momentum of the B-particle. This first possibility is compatible with Popper's idea, that our knowledge has no physical effect. *Nothing happens*: The B-particle goes on undisturbed, and will be detected by one of the central counters, because it is a particle that would have gone through the central slit if the screen had not been removed.
2. The second possibility is that, upon repetition, the particles going to the left will scatter (like those on the right) as a result of our knowledge of their position. This, Popper believes, is what Bohr and Heisenberg were committed to: The particles on the left will scatter, as if they had passed through a slit on the (removed) screen, equal in width to the slit of the screen on the right. That is to say, if the A-particle scatters, the B-particle will also scatter.

There are two possibilities concerning the scatters of the two particles.

[217] (*Ibid.*: 29–30)

2.a The first one is in accordance with Heisenberg's link of his indeterminacy relations to a wave packet and to Born's statistical interpretation. The second particle will have to scatter in directions that do *not* depend on the direction of the scatter of the first particle. That means that the directions of the two scattered coincident particles are uncorrelated; the direction of the scatter of the B-particle on the left will not be correlated with that of the A-particle on the right.

2.b The second is that the scatters of the particles are correlated. When the A-particle on the left scatters upwards, the B-particle on the right scatters downwards. This would correspond to the situation in the Bohm version—measurement of polarisation rather than position—of the EPR experiment, since in that case a measurement of spin-up on the left means that the measurement on the right gives spin-down.

For Popper and his realist interpretation, the quantum formalism yields possibility 1; and his prediction is that the experiment will confirm this. On the other hand, Popper thinks that the Copenhagen Interpretation is certainly committed to possibility 2. But he keeps an open mind concerning these outcomes, being aware that quantum theory has surprised common sense physics in the past.

Popper thinks that if we make the slit A very narrow so that we get a wide scatter of the momentum, the law of conservation of momentum is preserved by the absorption of the screen at A of the component of momentum in the y-direction. If the screen at B is removed, nothing is there to absorb the momentum vector; so we cannot get any increased scatter of the momentum at B merely by narrowing the slit at A, even though our knowledge of the position of the particle at B is quite considerable. Furthermore, this consideration can be generalized. Bohr stressed that it is the total experimental arrangement that must be considered (for example in his reply to EPR he says that by measuring one of the two particles, the overall system is disturbed[218]). But this is altogether different with the screen B in place or with the screen B removed. It seems clear that the removal of the screen will affect the result of the measurement of the momentum which is achieved by the battery of counters beyond B. For these reasons Popper is committed to possibility 1.

If the results of the experiment support the subjectivist Copenhagen Interpretation against Popper's expectations, then he admits that this could be interpreted as indicative of action at a distance (similar to the results of the Aspect experiment). Popper is very sceptical about this because it would have to be an action at a distance which does not diminish with increasing distance. He holds that there is not sufficient evidence available for admitting such a theory and saying that quantum mechanics is not a local theory.

3.3. Analysis of the Experiment

The gist of Popper's argument is the following: According to the uncertainty principle, the scatter of the A-particles increases when the width of the slit decreases. Given the EPR correlation of each pair, and with the B-slit removed, one asks what happens to the scatter of the B-particles when the width of the A-slit decreases. The experiment is supposed to measure the scatter of those B-particles whose counterparts have gone through the A-slit before and after the narrowing of the A-slit.

As noted above, given the measurement of the relative magnitude of the scatter, Popper makes the following claim: Given that *the formalism alone does not specify if the uncertainty relations should be applied or not in an indirect measurement*, such as the one considered here, it is *the Copenhagen Interpretation together with the formalism of the Quantum Theory* that entails that for the

[218] Bohr (1935*b*)

B-particles before and after narrowing of the A-slit we have:

$$\Delta p_{y,\text{after}} > \Delta p_{y,\text{before}}.$$

On the other hand, *his interpretation* (that denies that the uncertainty relations should be applied in an indirect measurement) *and the formalism of the Quantum Theory* entail that, for the B-particles:

$$\Delta p_{y,\text{after}} = \Delta p_{y,\text{before}}.$$

The emphasis here indicates that Popper is quite explicit in that he sees this as *a test between the two interpretations and not as a test for the formalism*, as the formalism alone does not say anything either on whether the relations apply to indirect measurements, it only implies that *if* they do apply then the uncertainty after is greater than before; but if the uncertainty relations should not be applied to an indirect measurement then the uncertainties before and after should be equal. On considering the claim that the uncertainty principle should be applied to indirect measurements as well, he writes:

> If, as I am inclined to predict, the test decides against the Copenhagen interpretation, then this does not mean that quantum mechanics (say, Schrödinger's formalism) is undermined; although it would mean that Heisenberg's claim is undermined that his formulae are applicable to all kinds of indirect measurements (a claim which the adherents of the Copenhagen interpretation—such as von Neumann—would undoubtedly regard as part of quantum mechanics).[219]

This means that Popper thinks that the formalism together with the assumption about the applicability of the uncertainty principle to indirect measurements entails an increase in Δp_y, whereas the formalism without this assumption entails that Δp_y is the same.

In effect Popper argues contrapositively by saying the following: In a single slit experiment, we explain the increased diffraction when we narrow the slit by

[219] (*Ibid.*:29)

an appeal to the uncertainty principle. Now, assume that we measure the scatter of the A-particles after they pass the slit, and that we find, in accordance with the uncertainty principle, that $\Delta p_{y,\text{after}}(A) > \Delta p_{y,\text{before}}(A)$. Assume also that we then measure, using some apparatus that will allow the identification of the particles that belong to the same pair, the two scatters for the B-particles before and after we narrow the A-slit, when there is no screen at B, and that we find that there is an increase in scatter,

$$\Delta p_{y,\text{after}}(B) > \Delta p_{y,\text{before}}(B).$$

How would we explain the increase in scatter of the B-particles, given that there is no physical interference with the particles at the B-side of the experiment? Why would the particles scatter more *in the absence* of a slit? In order to answer this we would have to lay out the usual story about scattering by one slit: The fact that there is an EPR correlation between the two particles in a pair and that this entails an indirect measurement of the y-component of position of the B-particle that results to a decrease in the uncertainty of our knowledge of this position, and also that due to the above-mentioned assumption about indirect measurements this means that this decrease should be accompanied by an increase in our uncertainty of our knowledge of the p_y-component of momentum. But if we do not observe the increase in scatter this means that this assumption is false. Furthermore, in that case, we still run the same story without the above-mentioned assumption and explain all the other things we observe in this experiment, namely the increase in the scatter of the momentum of the A-particle, and therefore the quantum formalism, and the application of the uncertainty principle to direct measurements are left intact.

Of course this tells us that the assumption is a sufficient condition for the increase in scatter. But why is this a test for the Copenhagen interpretation?

Because, says Popper, measuring the y-position of the A-particle gives us knowledge about the y-position of the B-particle. And if indeed, as the Copenhagen interpretation maintains, the uncertainty principle is about our knowledge, then the indirect precision of knowledge for the position should result in an associated decrease in the precision of our knowledge about the momentum.

Popper, from his realist perspective, maintains on the other hand, that the uncertainty principle only refers to the scatters: Passage through any slit would mean a transfer of momenta between the particle and the slit; and the narrower the slit, the larger the momentum transfer. This is what the uncertainty relation reflects. But the transfer of momentum can only take place in a direct measurement and not in an indirect one. So from his perspective, lack of an increase in the scatter of the B-particle indicates that the assumption of the applicability of the uncertainty principle to indirect measurements is false. And since the Copenhagen interpretation entails this applicability, from the lack of an increase in the scatter we should conclude the falsity of the Copenhagen interpretation.

But what about the opposite result? What happens if we observe the increase in scatter? In this case Popper thinks all is not lost for his realist interpretation. He thinks that we should accept the uncertainty principle, although it only refers to the scatters and not to our knowledge, also for indirect measurements, and take this as an indication of some causal connection between the location where the direct measurement takes place and the location where the indirect measurement takes place. For spatially separated locations this would require some kind of action-at-a-distance. As discussed above, this would count as a test between Einstein's and Lorentz's interpretation of the formalism of special relativity.

In order to evaluate the experiment one should look first at how the experiment would produce the results, and second whether the predictions of quantum theory are what Popper claims them to be. In the remainder of this chapter the issue of the technicalities of the experiment will be examined, together with the question of whether some of the difficulties that emerge can be overcome. As with his previous experiment, Popper does not get all the technical details right. But, as it will be seen in later chapters, it has been claimed by physicists that the essence of the experiment can actually be concretised. Whether this is actually the case will be examined in detail.

3.4. Overcoming Technical Problems For The Experiment

On the general issue of technical problems, it should be noted that Popper's experiment is not a flawless one. We shall point out a number of difficulties and a few have also been noted already in the papers that will be examined further down. Popper not being a physicist himself, should not be expected to provide a technically perfect experimental set-up. Redhead, in his critique of the experiment and the overall contribution of Popper to the philosophy of quantum mechanics notes that "Because some detailed arguments are flawed, this does not mean that his overall influence has not been abundantly beneficial".[220] And the experiment is indeed influential enough. It is worth therefore considering how many of the difficulties that the proposed experiment has can be alleviated without affecting the argument. It is only by granting this leeway to Popper that the experiment can be evaluated and its influence appreciated. It would not suffice to point to some technical difficulty and argue that because of this his argument does not stand. It is also required that any potential test between the Copenhagen interpretation and a realist take on quantum theory should be given every chance to succeed in deciding between the one or the other.

3.4.1. Coincidence Counters

The first problem arises from the use of coincidence counters. It is essential that only the particles that have counterparts passing through the A-slit are counted at the side of the B-slit in order to construct the statistics for the scattering on the B-side. This entails the need for identification of the twin particle to those which pass through the A-slit. This is the reason why Popper has chosen coincidence counters. The use of coincidence counters means that particles which are recorded within a very small interval of time from each other at two counters

[220] Redhead (1995: 176)

on the opposite sides are assumed to belong to the same pair. This is because, given the fact that the micro-particles used in such experiments are indistinguishable, there is no other characteristic that would identify them as belonging to the same pair.

The identification of the twin particle is done by assuming that two particles belonging to the same pair will have an equal time of flight from the source to the two counters, and that two counters on the opposite sides will absorb them at the same time. But "at the same time" here means at the same, very small, interval of time, which is the response time of the counters. Therefore this way of identifying pairs still faces the problem that there might be other particles on the B-side that do not have counterparts passing through the A-slit but that still happen to be absorbed by a counter within an interval of time during which a counter on the A-side has recorded a particle. If this is allowed to happen then the statistics which are provided by the coincidence counters are not going to be the ones which are relevant for the experiment. So how does Popper deal with this problem? Popper assumes that by using a very low intensity source one can make sure that if two particles are recorded simultaneously then they must belong to the same pair. That is, if there is a very small probability of emission within a time interval that is equal to the response time of the counters, then there is a very small probability of misidentification of the twin from a pair.

There are two problems associated with this assumption. First, the technical problem of finding a source which provides particles that form a pair with an EPR correlation, and with such a low intensity that gives a low enough probability of misidentification.

Assuming that a solution to this technical problem exists and the right substance material for the source can be found, Popper faces a second problem. This second problem stems from the fact that Popper considers sources of massive particles that, according to his realist understanding, actually scatter from the slit by the transfer of momentum with the slit. This would mean that the magnitude of the momentum might change during this momentum transfer and therefore the time of flight between source and counter might change. This change in momentum could in principle be large enough compared to the response time of the counters, but this would only mean that some particles that should have been identified, are actually not identified, and this does not spoil the statistics. But this change in momentum can also be so large so as to alter the time of flight enough to actually make it probable enough that it is absorbed at the same time with a particle from a different pair. If the argument is to go through an investigation would be needed to make sure that the statistics are right to face both these problems.

With respect to this second problem, it can immediately be observed that it does not arise for a source of photons, such as the ones used in the Kim and Shih experiment[221] which will be examined latter. One can note here that the difference between Popper's version of his experiment and the Kim and Shih realisation with photons would be equivalent to the difference between the original and the Bohm version of the EPR experiment.

3.4.2. The Finite Size of the Source

The second problem arises from the fact the source would have to have a finite size. Popper's argument is based on claiming that what is important here is the size of the region where the pair emerges from (that has the size of the cross-section of the interaction) and not the source itself. This is because if we know that a particle has gone through the A-slit, then, given the EPR correlation, we also know that the twin particle has gone through the region that corresponds to

[221] Kim and Shih (1999)

the reflection of the slit through the region where the pair emerges, i.e., through the region that is between two imaginary straight lines that start from the edges of the slit, and then go through the opposite ends of the source and extend to the virtual slit on the other side.

The problem here is that simple geometry shows that if the region where the pair emerges has a finite size, then the reflection of the slit will not have the same size as the original slit.

There is also a related problem. By decreasing the size of the source, we also decrease the uncertainty in the y-position. But this means that the uncertainty in the y-momentum increases, according to the uncertainty principle. This in turn, means that the EPR correlation is not perfect and the particles do not leave the source back-to-back, and this spoils the argument that we can infer the position of photon 2.

This problem will be examined latter, as this is a very serious problem for the whole argument. But it is worth mentioning that Kim and Shih in their experiment try to avoid this problem by the use of an optical lens.

3.4.3. The Non-Firing of the Counters at Large Angles

A third problem comes from an assumption that Popper makes which is not really necessary. This has given rise to a very acute difficulty pointed out by Redhead.[222] The arguments by Redhead will be examined later, but for now we can examine the assumption and how the problems it gives rise to can be avoided. Popper writes the following, concerning the preparation of the experiment:

> We now first test the Heisenberg scatter for both the beams of particles going to the right and to the left, by making the two slits A and B wider or narrower. If the slits are narrower, then counters should come into play which are higher up and lower down,

[222] (*Op. cit.*: 169)

seen from the slits. The coming into play of these counters is indicative of the wider scattering angles which go with a narrower slit, according to the Heisenberg relations.[223]

This means that Popper assumes that the signature indicating a larger scatter upon narrowing of the slits, is the coming into action of counters that were inactive when the slits were wide. He makes this assumption in order to show that there is some effect that should happen if the Copenhagen interpretation is correct that actually does not, or so he predicts. That is, he is arguing that when we narrow the slits then some counter at wide angles will fire that did not fire before because of the increased scatter—or knowledge of position for Heisenberg. But because of the EPR correlation we have also acquired knowledge of the position of the particle on the other side where there is no slit and we should expect the firing of the corresponding counters on that side also—an event that Popper predicts will not happen.

But the inactivity of some counters at some angles is an assumption that Popper neither would want (for the purposes of his argument) nor would need to make; and furthermore it is false! First Popper does not want to make this assumption because it is false and because, as will become clear when examining Redhead's paper it can—but maybe should not as it will be argued there—prove fatal for Popper's argument. Second Popper does not need to make this assumption because an increase in scatter can be demonstrated without this assumption.

The assumption is false because there would not be any counters that do not fire given his set-up. This is because given any scattering situation we do expect *all* the counters to fire provided we wait long enough. This means that the amplitude for the firing of counters at 90° is not zero but only very small, and although not very probable when the slit is wide, the event will happen eventually. So how can the increase in scatter be measured if all the counters fire before and after

[223] (*Op. cit.*: 28)

we narrow the slit? An analysis that answers this question in detail is provided in the next section (3.5). But for now it would be enough to say that one could demonstrate an increase in scatter by showing that there is a larger probability density for larger angles upon narrowing of the slit.

3.4.4. Are the Uncertainty Relations Equalities?

A further problem is the one pointed out by Krips.[224] He notes that, for his argument to work, Popper needs to make the assumption

$$(a) \qquad S_1(p_y) \times S_1(y) = \hbar$$

rather than

$$S_1(p_y) \times S_1(y) \geq \hbar$$

where $S_1(y)$ and $S_1(y)$ being the uncertainties in position and momentum of the particles that go through the slit that is wide open (the numbering of this equation is kept from Krips' paper). This he grants to Popper initially, but two pages later Krips bases his attack on the falsity of (a), and claims that this is so because for a sub-ensemble we have $S_1(p_y) \times S_1(y) = \infty$. We will come back to this point during the examination of Krips' argument. But here it should be mentioned that, for the initial granting of eq. (a), it can be shown that there is a way to solve the problem by granting to Popper a more innocent assumption that would see his argument through without having to rely on eq. (a). We shall see how this can be done in the next chapter.

[224] Krips (1984: 257)

3.5. Statistical Analysis of the Relation Between $\Delta\theta$ and Δp_y

As discussed in the previous section, there is a technical point that can be made concerning the set-up of Popper's experiment and the measurements that it is supposed to yield. Popper envisages the use of Geiger counters wired up as coincidence counters to test the scatter. He makes the assumption that when a slit is wide, then there would be some counters at large angles, near 90°, that will not fire. But as noted earlier this assumption is unnecessary, because this is not the only way in which an increase in scatter can be demonstrated. It can also be detected in the experiment by a larger probability density at larger angles.

The experiment aims to compare Δp_y at the B-side before and after we have narrowed the A-slit, while at the same time we keep the B-slit wide. In order to get a comparison between the scattering in Δp_y before and after narrowing the A-slit, one needs to assume that the Geiger counters indeed measure Δp_y, at least indirectly. Now, since the Geiger counters are arranged in a semi-circle, they provide the number of particles for the angle $\theta + d\theta$ that each counter covers. From this, using statistical analysis, one can deduce $\Delta\theta$. Therefore, in order to claim that this gives an indirect measure of Δp_y one should assume that there is a relation between $\Delta\theta$ and Δp_y, such that it is either a linear relation like:

$$\Delta\theta = c \cdot \Delta p_y \tag{3.1}$$

with c being positive, or some other monotonic increasing function.

Elementary statistics tells us that if there exist a linear dependence of θ on p_y then the above equation follows trivially. But is this so? And if not, what about the relation between the above scatters?

Now, for Popper's argument to work, p_y must be the y-component of momentum for the particles immediately after passing the B-slit. Assuming that this is indeed the case, can it also be assumed that the particles which have a momentum

$p'_y + dp_y$ will be the ones that will be counted by the counter covering the angle $\theta' + d\theta$? In order to facilitate the analysis, one can assume that the particles follow a classical trajectory along the line between the centre of the slit and the counter at some angle θ. It will then have a momentum vector with magnitude p and a y-component p_y. In that case elementary kinematics tells us that p_y and θ are related by

$$p_y = p \cdot \sin\theta \quad \Rightarrow \quad \theta = \arcsin\left(\frac{p_y}{p}\right) \tag{3.2}$$

This is not a linear relation. The function $\sin\theta$ is approximately linear only for small angles, and therefore its inverse, arcsin, is also approximately linear only for the same small angles. Furthermore θ depends not only on p_y but also on p. This means that not all the particles with p_y in $p'_y + dp_y$ will move along the line with θ in $\theta' + d\theta$. But what about the two scatters?

Assuming that p does not vary significantly around its mean value $p = p_0$, and that p_y has an average value of zero due to symmetry, we ask if $\Delta\theta$ is proportional to Δp_y when the average value of θ is 0 (again due to symmetry). We have

$$\begin{aligned}(\Delta\theta)^2 &= \langle\theta^2\rangle - \langle\theta\rangle^2 \\ &= \langle\theta^2\rangle - 0\end{aligned}$$

where $\langle\theta\rangle$ and $\langle\theta^2\rangle$ are the average values of θ and θ^2 respectively. Now, since the counters are arranged in semicircles we need to consider angles between $-\pi/2$ and $\pi/2$. For those angles, p_y ranges over $-p < p_y < p$. So, we have:

$$\begin{aligned}(\Delta\theta)^2 = \langle\theta^2\rangle &= \int_{-p}^{p} \theta^2 f(p_y)\, dp_y \\ &= \int_{-p}^{p} \left[\arcsin\left(\frac{p_y}{p}\right)\right]^2 f(p_y)\, dp_y \\ &= \int_{-p}^{p} \left[\arcsin\left(\frac{p_y}{p}\right)\right]^2 \frac{e^{-\frac{p_y^2}{2\sigma^2}}}{\sqrt{2\pi}\,\sigma}\, dp_y \\ &= \frac{1}{\sqrt{2\pi}\,\sigma} \int_{-p}^{p} \left[\arcsin\left(\frac{p_y}{p}\right)\right]^2 e^{-\frac{p_y^2}{2\sigma^2}}\, dp_y\end{aligned}$$

where $f(p_y)$ is the distribution function of p_y, and $\sigma = \Delta p_y$. Now using the transformation $x = p_y/p$, with x now ranging between -1 and 1 for the above limits, and $dp_y = p\,dx$, we have:

$$
\begin{aligned}
(\Delta\theta)^2 &= \frac{1}{\sqrt{2\pi}\,\sigma}\int_{-1}^{1}\big(\arcsin(x)\big)^2 e^{-\frac{x^2p^2}{2\sigma^2}}\,p\,dp_y \\
&= \frac{p}{\sqrt{2\pi}\,\sigma}\int_{-1}^{1}\big(\arcsin(x)\big)^2 e^{-\frac{x^2p^2}{2\sigma^2}}\,dp_y
\end{aligned}
$$

The last integral cannot be solved analytically, so an approximation can be used to yield a form that can be integrated. In this case, as the exponential is particularly simple, a Taylor series expansion around 0 can be used for the $\arcsin(x)^2$ function. Any expansion of order 8 or above yields extremely good accuracy for the interval of integration. The expansion of order 16 is (using $P(x)$ for $\big(\arcsin(x)\big)^2$):

$$
P(x) = x^2 + \frac{x^4}{3} + \frac{8x^6}{45} + \frac{4x^8}{35} + \frac{128x^{10}}{1575} + \frac{128x^{12}}{2079} + \frac{1024x^{14}}{21021} + \frac{256x^{16}}{6435}
$$

The integral then becomes

$$
\begin{aligned}
\int_{-1}^{1} P(x)e^{-\frac{x^2p^2}{2\sigma^2}}\,dp_y = \frac{\sigma^2}{ap^{17}}\Bigg[&\left(\sum_{n=1}^{8} b_n\,\sigma^{(2n-1)}p^{[15-(2n-1)]}\right)\sqrt{2\pi}\,\mathrm{erf}\left(\frac{p}{\sqrt{2}\sigma}\right) \\
&-\left(\sum_{n=1}^{8} c_n\,\sigma^{[15-(2n-1)]}p^{(2n-1)}\right)e^{-p^2/2\sigma^2}\Bigg]
\end{aligned}
$$

where, a, b_i and c_i are positive constants, and the error function $\mathrm{erf}(x)$ is defined as

$$
\mathrm{erf}(x) = \frac{2}{\pi}\int_0^x e^{-t^2}\,dt.
$$

So finally we get

$$
\begin{aligned}
(\Delta\theta)^2 = \frac{\sigma}{a\,p^{16}}\Bigg[&\left(\sum_{n=1}^{8} b_n\,\sigma^{(2n-1)}p^{[15-(2n-1)]}\right)\mathrm{erf}\left(\frac{p}{\sqrt{2}\sigma}\right) \\
&-\left(\sum_{n=1}^{8} c_n\,\sigma^{[15-(2n-1)]}p^{(2n-1)}\right)\frac{e^{-p^2/2\sigma^2}}{\sqrt{2\pi}}\Bigg]
\end{aligned}
$$

This function of p (the incoming momentum) and $\sigma(= \Delta p_y$, i.e. the scatter of $p_y)$, represents the square of the scatter of θ. The use of graphic methods can show that it is monotonic as long as $p \geq |p_y| >> \Delta p_y$, a requirement which is satisfied for a positive quantity like the magnitude of momenta.

It has thus been shown that an increase in scatter in side-B can indeed be measured by Popper's set-up if all the counters fire before and after narrowing the A-slit. This is because an increase in the scatter of p_y, Δp_y does indeed lead to an increase of $\Delta\theta$, and therefore by measuring the distribution of particles in the counters, one can derive conclusions concerning Δp_y. Popper's argument has thus been shown to be immune to the criticism by Redhead referred to in the previous section (subsection 3.4.3).

Chapter 4: Philosophers' Reactions to the Popper Experiment

There are three main responses to the proposed experiment by Popper in the philosophy literature that precede its concretisation by Kim and Shih: Krips's "Popper, Propensities, and Quantum Theory",[225] Sudbery's "Popper's Variant of the EPR Experiment Does Not Test the Copenhagen Interpretation",[226] and Redhead's "Popper and the Quantum Theory".[227] In this chapter these articles will be analysed and criticised. The reactions of physicists before the Kim and Shih experiment will be examined in the next chapter.

The distinction that is made here, between the papers that are examined in this chapter and the next, is not so much the one between philosophers and physicists. In any case, Sudbery is really a philosophically minded physicist rather than a pure philosopher, and the same can be said for Redhead. The distinction has more to do with the intended audience: Its clear that Krips, Sudbery and Redhead talk to the philosophers of physics, whereas Collett and Loudon[228] and Peres,[229] have the physicist in mind. On the one hand, Collett and Loudon publish their article in *Nature*, and make a point concerning the technical difficulties of the experiment. On the other, as will became clear later on, although Peres' article appears in the *Studies In History and Philosophy of Science*, it has the physicist in mind, as it almost takes for granted that there is nothing to an interpretation, over and above it being a rule that will correlate the objects of the mathematical formalism of a theory with physical quantities.

225 Krips (1984)

226 Sudbery (1985)

227 Redhead (1995)

228 Collett and Loudon (1987)

229 Peres (2002)

4.1. Krips on the Popper Experiment

One of the early reactions to the Popper experiment in the literature, and a very important one at that, can be found in Henry Krips' *Popper, Propensities, and Quantum Theory.*[230] This article is a collective review of three books by Popper: *Realism and the Aim of Science,*[231] *The Open Universe,*[232] and *Quantum Theory and the Schism in Physics,*[233] from the *Postscript to the Logic of Scientific Discovery.* The article has two sections; the first section is on Popper's new thought experiment and the second on Popper's propensity interpretation.

Before these two sections, in the introduction to his article, Krips notes a change from the Popper of *Logic of Scientific Discovery,* in what he calls "the embracing of metaphysics".[234] Indeed, in the *Logic of Scientific Discovery* Popper gave emphasis to falsification. His intention was for this to be a process that would always employ empirical criteria, and he was consequently rejecting 'metaphysics'. But in his *Schism* Popper advocated metaphysical theories that would be testable and could be rationally criticised. Krips comments that, given these provisions, this is not much of a change, but in any case this is how Popper's metaphysics should be evaluated.

As part of this evaluation Krips examines Popper's new metaphysics of quantum theory that is given in his *Quantum Theory and the Schism in Physics.* He notes that Popper's motivation is his opposition to the 'subjectivism' (or 'anti-realism') expressed by some versions of the Copenhagen interpretation and by the

230 Krips (1984)

231 Popper (1983)

232 Popper (1982*a*)

233 Popper (1982*b*)

234 (*Op. cit.*: 253)

early works of Heisenberg.[235] Part of this opposition contains the new experiment that will test the subjectivist Copenhagen interpretation, but not quantum theory itself. Krips also notes that in order to follow the argument during the examination of the experiment, he will assign the state-descriptions to ensembles, rather than to individual systems; an assumption which he questions while examining the propensity interpretation in section 2 of the article.

Krips considers the Popper experiment "as a hybrid of an EPR set-up and a single slit diffraction experiment" in which an ensemble of pairs of particles is prepared in an EPR state. This preparation means that "there is probability 1 of agreement between the measured values for the y-coordinates of the members of any given pair".[236] Krips also notes here that this also means that there is an agreement for the p_y values for each pair, but that Popper does not use this. It should be noted here that the reason why Popper does not use this is that this agreement refers to the p_y-values *before* the particles have reached the slits. With respect to the p_y-values *after* the passage through the slits, Popper would consider three different alternatives—namely that the particle passing through the slit will not scatter, that it would scatter and it p_y-value would not be correlated with the p_y-value of the other particle, and that it would scatter and it p_y-value will be correlated with the p_y-value of the other particle—only one of which says that there is an agreement such as the one Krips considers. In fact this alternative would be consistent with an action-at-a-distance.

As already described, each particle from a pair travels towards opposite situated screens with slits behind which there are counters, and the particles scatter through the slits towards the counters. Krips quotes Popper: "Now we make the

[235] Krips quotes Popper quoting Heisenberg's view, from *Daedalus* **87**, 1958: "objective reality has evaporated and that quantum mechanics does not represent particles, but rather our knowledge, our observations, or our consciousness of particles".

[236] (*Ibid.*: 254)

slit A very small and the slit B very large...";[237] and explains that the result is an accurate measurement of the y-coordinate of the A-particle and an indirect measurement for the B-particle.

Next Krips quotes Popper's main point which is that, since according to the Copenhagen interpretation the subject of quantum theory and of the Heisenberg relations is our knowledge, then it follows that the spread of the B-particle's momentum will become as big as that of A's irrespective of the fact that slit A is narrower than slit B.

Krips here also notes an ambiguity:

> between the initial scatter of measured values for p_y over the B-particles and the resultant spatial scatter of the B-particles along the y-axis after passage through B.[238]

It is not very clear why Krips thinks that there is an ambiguity here and it is not very clear what the distinction which he makes is. He says on the one hand

(a) "the initial scatter of measured values for p_y"

and on the other

(b) "the resultant spatial scatter of the B-particles along the y-axis after passage through B".

By the use, with reference to the scatter, of the word "initial" in (a) and of "resultant" in (b) it is implied that (a) is a scatter which 'exists' before (b). But the only measured momenta are the ones which are measured by the counters at the end and there are no other momenta that we can talk about after them. Otherwise if the word "initial" in (a) does indeed refer to something which 'exists' before (b), then it can only refer to the y component of the momentum of the particles before the passage through the B-slit which is never measured anyway. On the other hand it is clear that (b) refers to the y-component of the momentum

237 (*Ibid*)

238 (*Ibid.*: 255)

of the particles at the moment (or immediately after) of the passage through the slit. So if this is the case, and (a) indeed refers to the measured values by the counters, then it is hard to see the distinction: He explains in a footnote that,

> Ehrenfest's theorem (p. 26, Schiff [1955]) shows that on average the momentum of a particle of given mass determines its rate of change of position (in a given direction). Thus a scatter of momentum is reflected eventually in a scatter of spatial location.[239]

But invoking Ehrenfest's theorem is unnecessary here, as is the distinction. Furthermore it is insufficient: The particles which have pairs passing through the A-slit would indeed have a scatter in momentum, irrespective of whether 'scatter' here refers to ensembles of particles (as Popper would prefer) or to individual ones (as Copenhagen interpretation would). Given Popper's set-up, to know only the rate of change of position (in a given direction) for one particle is insufficient to determine the counter that will eventually register this particle since, as it was already shown, not all the particles with p_y in $p'_y + dp_y$ will move along the line with θ in $\theta' + d\theta$. On the other hand, what Popper does indeed need is a monotonic relation between the scatter in p_y and in θ. That was already shown to exist in the relevant region of values for the scatters in the previous chapter.

That this technical point is misunderstood by Krips is also endorsed by the following comment made by him next:

> In order to avoid the complexities of justifying this proportionality however, I shall here take 'scatter' to refer to the former scatter (of measured p_y values); and hence replace the counters behind the slit B in Popper's experiment by a single p_y measuring apparatus.[240]

Indeed scatter should refer to the scatter of measured p_y values, but the problem is that it is not very clear what a "single p_y measuring apparatus" would be. Any momentum measuring apparatus would not be able to distinguish between the p_y- and the p_x- (or p_z) component of momentum: It would simply measure the

[239] (*Ibid.*: 255)
[240] (*Ibid.*: 255)

magnitude of the momentum after it has passed the slit. But that alone does not determine the scatter of p_y—the angle of flight θ is also needed. It is exactly because of this difficulty that a semicircular array of counters is necessary. And as was already shown, the scatter in θ which can be measured by the density of particles at each angle has a monotonic relation to the scatter of p_y values.

Nonetheless this manoeuvre is done by Krips "[i]n order to avoid the complexities of justifying this proportionality", and since this proportionality has been shown to be justified there is no harm to the rest of his analysis. He also removes the slit B from the set-up.

For Krips, then, the experiment simply consists in measuring the p_y-scatter when w_1 is the width of the A-slit and also when it is w_2, with $w_2 < w_1$. And he gives Popper's claim as: "the subjectivist version of the *CI* entails that $S_2(p_y) > S_1(p_y)$", whereas from the realist point of view "his argument would seem to commit him to asserting that $S_2(p_y) = S_1(p_y)$". Indeed Popper thinks that an increase in scatter is against his expectations, and that his favourite outcome would be that the scatter does not change. But Krips accuses Popper of having prepared the "fall-back position" of admitting 'action-at-a-distance': "A positive result for subjectivism can be reinterpreted by a realist as merely(!) action-at-a-distance".[241] This is somewhat unfair as Popper does not seem to think of this as a light subject. On the contrary, he thinks that if Aspect's or his own experiment are indicative of instantaneous action at a distance, then one should conceive them as crucially decisive between Lorentz's and Einstein's interpretations of the formalism of special relativity, and thus as indicative of Lorentz's own interpretation of his formalism. This is a very serious consequence arising from such an experiment: All the experiments that established relativity are indifferent between the

[241] (*Ibid.*: 257)

two versions.

4.1.1. Krips' Analysis of the Popper Argument

So far Krips has given the outline of the Popper experiment and has pointed out that the two possible outcomes are either that the scatters before and after narrowing the A-slit are equal, $S_2(p_y) = S_1(p_y)$, as favoured and predicted by Popper; or that the scatter will increase, $S_2(p_y) > S_1(p_y)$, as Popper claims that the subjectivist *CI* entails. He then moves on to attack Popper's argument.

The first step in his analysis of Popper's argument is to grant him a technical point which Krips thinks is necessary for the argument to work. He notes that for Popper's argument to work, the quantum-theoretical uncertainties must be minimal:

(*a*) $$S_1(p_y) \times S_1(y) = \hbar$$

rather than $S_1(p_y) \times S_1(y) \geq \hbar$. Because only then we can use this together with

(*b*) $$S_2(p_y) \times S_2(y) \geq \hbar$$

and

(*c*) $$S_1(y) = w_1 \quad \text{and} \quad S_2(y) = w_2$$

to show that

$$S_2(p_y) \geq \frac{\hbar}{S_2(y)} = \frac{\hbar}{w_2} > \frac{\hbar}{w_1} = \frac{\hbar}{S_1(y)} = S_1(p_y), \tag{4.1}$$

i.e. $S_2(p_y) > S_1(p_y)$ (Krips gives names to his equations on the left and he does not name this last equation—my numbering, like (4.1), stands on the right). Otherwise if one does not grant Popper Krips' (*a*) equation, but instead uses $S_1(p_y) \times S_1(y) \geq \hbar$, then the last equation would look like:

$$S_2(p_y) \geq \frac{\hbar}{S_2(y)} = \frac{\hbar}{w_2} > \frac{\hbar}{w_1} = \frac{\hbar}{S_1(y)} \leq S_1(p_y), \tag{4.2}$$

This then would be a problem since it does not tell us anything about the relation between $S_1(p_y)$ and $S_2(p_y)$.

Krips is right to point this out. We do not expect that in every experiment where two incompatible (according to the uncertainty principle) observables are measured, their uncertainties, when multiplied, would be equal to $\hbar$. The uncertainty principle gives the lower bound for this product, and in any real experiment the two uncertainties in the product will also be influenced by experimental errors etc., and thus the product will normally be expected to be higher than $\hbar$. But is this fatal to Popper's argument? To what extend does it undermine it? Krips is willing to grant this for the sake of the argument but he comes back to it two pages later where he not only claims to have shown that Popper should not have assumed it, but where he is also using this as his main attack on Popper's argument.

We shall examine this last point later, when we reach the appropriate stage, but in the meantime, is it not possible to find a way of not granting to Popper Krips' eq. (a), but, instead, showing that $S_2(p_y) > S_1(p_y)$ without it? The answer is 'yes', provided that we make the following assumption:

$$S_1(p_y) \times S_1(y) = k\hbar, \quad S_2(p_y) \times S_2(y) = k\hbar \quad \text{and} \quad k \geq 1 \tag{4.3}$$

That is, provided we assume that for the *same experiment* the contributions from the other factors, errors etc., are the same throughout the experiment. It is the job of the experimenter to tell us whether this is a good assumption to make, but it looks an innocent and reasonable one. In any case it seems that in any experiment where the uncertainty principle is tested, such an assumption is indeed being made. Furthermore it seems to be less controversial than just granting Popper eq. (a) above. Provided that this is accepted we can then rewrite (4.1) as

$$S_2(p_y) = \frac{k\hbar}{S_2(y)} = \frac{k\hbar}{w_2} > \frac{k\hbar}{w_1} = \frac{k\hbar}{S_1(y)} = S_1(p_y), \tag{4.4}$$

so that Popper can have his

$$S_2(p_y) > S_1(p_y) \tag{4.5}$$

that is vital for his argument.

4.1.2. Krips' First Point Against Popper's Argument

Once Krips has granted (4.5) to Popper he states his first attack on Popper's argument:

> As indicated above it is the premise (c) of this derivation which Popper questions; and it is the falsity of (c) which Popper uses as an explanation for the failure of '$S_2(p_y) > S_1(p_y)$'—a failure which he predicts in opposition to a subjectivist reading of the *CI*. But Popper's argument here errs *in laying the premise (c) at the door of subjectivism* [my emphasis].[242]

I emphasised the last bit of the above quotation because this is *not* what Popper does. To see this it is instructive to look at what Krips says next:

> True, a subjectivist reading of *CI* does imply (c); but (c) can also be justified on far less controversial grounds—from within the central formalism of *QT*, independent of any particular interpretation of *QT*... This justification of (c) is quite independent of the *CI*—subjectivist or otherwise. Indeed all we need in the justification is the central part of *QT* (the 'Born statistical interpretation') which takes us from the state-vector of a system to the 'observable' probabilities of measuring physical quantities to have particular values. Thus in denying that $S_2(p_y) > S_1(p_y)$, Popper is putting forward a substantial variant of *QT*, and not just a realist interpretation of it.[243]

True, if Popper had disagreed with eq. (c) above, then he would be in disagreement not only with a subjectivist reading of QT, but with the theory itself. But eq. (c) is not what Popper has qualms with. What he puts "at the door of subjectivism" from the above derivation of equations is not (c) itself—after all he talks about scatters himself—but the first and the last equation in (4.4) above, namely:

$$S_2(p_y) = \frac{k\hbar}{S_2(y)} \quad \text{and} \quad \frac{k\hbar}{S_1(y)} = S_1(p_y) \tag{4.6}$$

[242] (*Ibid.*: 257)
[243] (*Ibid.*: 257)

That is, Popper denies that the uncertainty relation above applies to a virtual slit at which no act of measurement, and subsequently no exchange in momentum, has taken place. This means that he accepts the rest of the equation:

$$\frac{k\hbar}{S_2(y)} = \frac{k\hbar}{w_2} > \frac{k\hbar}{w_1} = \frac{k\hbar}{S_1(y)} \tag{4.7}$$

but he claims that (4.5) is wrong and that we have instead

$$S_2(p_y) = S_1(p_y) \tag{4.8}$$

This does not mean that Popper disagrees with the uncertainty principle itself. It only means that he disagrees with its applicability to a situation where we have *indirect knowledge* (through an indirect measurement), rather than knowledge through an actual measurement, through which some exchange of momentum has taken place. Therefore Krips is wrong when he claims that

> Thus in denying that $S_2(p_y) > S_1(p_y)$, Popper is putting forward a substantial variant of QT, and not just a realist interpretation of it. So what is being tested by Popper's thought experiment is not after all the CI; rather it is a crucial test between QT itself and Popper's variant of it.[244]

Popper's experiment is immune to this criticism of Krips, because the criticism is misplaced. If something is to be criticised, this should be the spirit of Popper's experiment, and not that which is merely convenient to criticise.

Moreover, in the sentence after the above quotation, Krips gives his own prediction for the outcome of the experiment:

> I predict therefore (in opposition to Popper) that were it possible to perform Popper's experiment, then we would find that $S_2(p_y) > S_1(p_y)$; QT would be vindicated and Popper refuted.[245]

It will be seen later on that at least in this Popper was right: The experiments *do seem* to deny that $S_2(p_y) > S_1(p_y)$. The problem for Popper is that it is not very

[244] (*Ibid.*: 257)
[245] (*Ibid.*: 257)

clear whether this fact argues against the Copenhagen interpretation. In making this prediction however, Krips shows that his criticisms of Popper are misplaced, and that his understanding of the theory is wrong.

4.1.3. Krips' Second Point Against Popper's Argument

The second point which Krips makes is related to Popper's claim for 'action-at-a-distance' if the experiment indicates that $S_2(p_y) > S_1(p_y)$, rather than an equality between them. It also relates to the assumption of the non-firing counters at large angles vis. a vis. scatter. As noted in the previous chapter, Popper does not need to make this assumption because larger scatter can be indicated without it. So what follows from Krips is based on this assumption which Popper should not have made in the first place, as the argument can go through without it. Nevertheless, Krips has an interesting point concerning conditional probabilities and scatter. It will be argued here that this point does not affect Popper's argument.

Krips questions whether a larger scatter indicates 'action-at-a-distance'. He agrees that according to Popper's argument, the Copenhagen interpretation implies action-at-a-distance if $S_2(p_y) > S_1(p_y)$. But he thinks that this is a mistake stemming "from adopting too loose an interpretation of the term 'scatter'".[246] He rightly points out that (given the Heisenberg relations) 'scatter' should read 'standard deviation' (and this accords with the analysis in the previous chapter). He accepts that 'action-at-a-distance' is indicated if values of $p_y(B)$ are not obtained when the slit is wide, and that they are obtained when the slit is narrow. But he does not accept that this indicates that $S_2(p_y) > S_1(p_y)$. He rightly points out that it is possible that $S_2(p_y) > S_1(p_y)$ even without any spurious 'action-at-a-distance', but simply by making an appropriate selection. Indeed he gives an

[246] (*Ibid.*: 257)

example where an appropriate selection of a sub-ensemble changes the standard deviation. This is indeed correct, and as Krips rightly points out:

> we can accommodate the result that $S_2(p_y) > S_1(p_y)$ within a quite realist version of QT (or Popper's subjectivist version of CI for that matter) without appealing to action-at-a-distance (contrary to Popper's claim).[247]

He also adds, "[i]n fairness to Popper", a remark concerning the "version of the *CI* espoused by the later Heisenberg": He points out that the scatter in measured values from an ensemble in a pure state reflects the objective indeterminancy for each system. In this case action-at-a-distance is indicated by a change in scatter as this indicates a change in each system, rather than a selection of a sub-ensemble with different statistics. Nevertheless Krips thinks that his claim above holds.

The points made by Krips are correct: In principle it could be argued that *some* selection *could* lead to a different standard deviation—this is almost a tautology. He says that "it can be argued that by narrowing the slit we simply select out fewer of the same elements that would have been selected had the slit been broader".[248] But this is not enough. Krips needs to show how it can be done in this case, and specifically he needs to show that out of all the particles that pass through the wide virtual B-slit, the ones which scatter more are the ones near the centre of the virtual slit. Earlier on however, he has argued that "the measured values for the y-coordinates of the particles within the initial sub-ensemble of A-particles are evenly spread over the space-interval occupied by the slit."[249] Presumably this holds for the particles passing through the virtual B-slit. But in that case what is the physical reason for the particles in the middle to scatter more than the ones at the edges of a virtual slit? It is not enough to say that in

[247] (*Ibid.*: 258)
[248] (*Ibid.*: 258)

[249] (*Ibid.*: 257)

principle this might be the case: There should be a physical explanation of why this should happen.

On the other hand the realist Popper can have a very good story to tell as to why the particles which pass through the A-slit have a larger standard deviation when the slit is narrowed: When the slit is narrowed there is a larger distribution of particles near the edges of the slit than when the slit is wider. And since scattering is due to the exchange of momentum with the edges of the slit, it follows that more particles will scatter at wider angles when the slit is narrow than when it is not. Therefore Popper has a coherent story to tell about the uncertainty relations that, as noted earlier, for the realist are scattering relations. But there is no reason for the particles in the centre of the virtual slit to scatter more. If that were the case then Krips' alternative to action-at-a-distance would be 'magic-at-a-distance'.

One can of course object that the realist story runs into trouble when one considers more general issues, such as the explanation of the secondary picks in the single-slit experiment and so forth. On this, of course Popper needs a more elaborate story, such as the one involving propensities that Popper uses to discuss his resolution of the paradoxes of Quantum Theory.[250] The further objection that Popper's explanation involving propensities also runs into trouble is a serious one, but the story given in the proceeding paragraph is only meant to show that even that explanation is more plausible than the one given by Krips. It is not meant as a more general defense of Popper's take on quantum mechanics in general.

So this second point which Krips makes is thus muted: Even if there are no non-firing counters and the larger scattering is translated to 'larger probability distribution for larger angles' there is no reason why the particles in the middle

[250] Popper (1982*b*: 144–58)

of the slit should exhibit this larger probability distribution upon the execution of the experiment. For a realist interpretation, such as Popper's, 'action-at-a-distance' is indeed an open avenue should the result of the experiment indeed be $S_2(p_y) > S_1(p_y)$ rather than $S_2(p_y) = S_1(p_y)$.

4.1.4. Krips' Third (and 'most definitive') Point Against Popper's Argument

Finally, Krips gives his third criticism of Popper's experiment which relates to him granting the technical point when he was giving his first criticism. This he thinks is both the 'most definitive' and the 'easiest to put aside' because "by doing a 'Bohm-Aharanov' to Popper's experiment"[251] it can be avoided. Krips argues that the aforementioned technical point cannot really be given to Popper and that only changing the experiment and using a formulation similar to the Bohm-Aharanov version of the EPR experiment, would let his argument stand. It has already been argued above that Popper does not really need this technical point. What he needs instead is a more innocent one; that of assuming that the experimental errors influence the results of measuring the scatter equally before and after the *virtual* slit has been reduced—that is, the experimental errors can be hidden under the unique constant k in my (4.3). Here I shall argue that Krips is mistaken in thinking that the technical point (which Popper does not need) cannot be granted, and that this mistake stems from his misunderstanding of the uncertainty relations.

Krips argues that Popper's argument relies not only on the Heisenberg inequality for the collection of particles that pass through the wide B-slit:

$$S_1(p_y) \times S_1(y) \geq \hbar$$

[251] (*Op. cit.*: 258)

but also on the corresponding equality for their uncertainties:

(a) $$S_1(p_y) \times S_1(y) = \hbar$$

But he thinks that this is not the case due to the correlation of the B- with the A-particles: Since the initial ensemble of A-particles are in, as he says, "a mixture of y-coordinate eigenstates",[252] (this point is justified in a footnote where he notes that it "follows from the von Neumann rule for deriving the density operator of a sub-system from the density operator of the total system, as applied to the EPR state for the initial ensemble of all pairs of A-particles and B-particles.".[253]) the B-particles are also in a mixture, because of this correlation. This means that when the A-particles pass through the slit they would still be in a mixture with a narrower dispersion, and the same holds for the corresponding B-particles. He adds: "for a collection of particles in a y-coordinate eigenstate—or a mixture of such—the scatter of measured momentum values is easily seen to be infinite".[254] Krips

Therefore for Krips not only does (a) fail, but we also have

$$S_1(p_y) \times S_1(y) = \infty \tag{4.9}$$

and furthermore this makes "Popper's thought experiment impotent", because "if $S_1(p_y) = \infty$ then it is impossible to decide between $S_2(p_y) > S_1(p_y)$ and $S_2(p_y) = S_1(p_y)$".[255] Krips then goes on to show how this problem can be avoided 'by doing a 'Bohm-Aharanov' to the experiment'. Before the examination of this reformulation, it is appropriate to see why it is an unnecessary one for Popper's argument.

[252] (*Ibid.*: 259)

[253] (*Ibid*)

[254] (*Ibid*)

[255] (*Ibid.*)

Krips is wrong here for a couple of reasons. First he misunderstands the nature of the uncertainty relations when he claims that eq. (4.9) follows from $S_1(p_y) = \infty$. When we say that one of the two uncertainties in the product equals infinity we do not really mean that it therefore follows that the product will also equal to infinity. It is rather the other way round: When we have reasons to think that one of the two terms in the product is equal to zero then we have to assume the other to be infinite in order to keep their product finite and non-zero. For example in the measurement of the lifetimes and of the spreads of the peaks of energy absorbed at particular energy levels in various subatomic collisions that indicate the existence of a particle, we find that their product is of the order of $\hbar$; but when it comes to stable particles then we can measure with great precision their rest mass and we interpret their stability as indicating an infinity in their time uncertainty. But that does mean that we ever measure anything to be infinity as this is an impossibility.

At the same time, if Krips was just wrong about this, his argument would still be valid: If indeed it was the case that $S_1(p_y) = \infty$, then Popper's argument would be muted. Because then we would not be able to compare the uncertainties before and after we narrow the slit, $S_1(p_y)$ and $S_2(p_y)$. But this brings us to the second mistake made by Krips. That is, Krips is wrong when he thinks that $S_1(p_y) = \infty$. And he is wrong both because this is not what the theory says and because this is not what the experiments seem to suggest. To start with the experimental situation, what does it mean to say that the uncertainty in p_y, $S_1(p_y)$, is infinite? It means that the particles could have any value for the magnitude of the y-component of their momentum. If we ask how this situation would manifest itself experimentally, the answer is that this amounts to the situation where all the counters are firing equally at all angles. Effectively, Krips makes a prediction about the outcome of the experiment which is not compatible with any of the experiments. Because if what he says was correct, then it does hold not only for the correlated B-particles, but also, by the same token, for the A-particles; i.e., in any EPR situation we should be finding that result, on both sides, irrespective of Popper's argument.

From the theoretical point of view, Krips justifies his assertion that $S_1(p_y) = \infty$, by saying that the ensemble of A-particles is in a mixture of y-coordinate eigenstates. This is because, as he explains in a footnote,

> This follows from the von Neumann rule for deriving the density operator of a sub-system from the density operator of the total system, as applied to the *EPR* state for the initial ensemble of all pairs of A-particles and B-particles.[256]

But the correlation does not mean that the A-particles are in a mixture of y-coordinate eigenstates. On the contrary, since the two particles in a pair have a momentum correlation—that is, they are in a momentum eigenstate—the pair is in a superposition of position states. This result will be shown explicitly in a later chapter. It will be shown that, following the criticism by Michael Redhead[257] not only are the uncertainties not infinite but also that this yields a different, more problematic issue for the thought experiment. But it should be noted here that the reformulation suggested by Krips is unnecessary, since the thought experiment does not suffer from the problem that Krips *thinks* exist.

Nevertheless, Krips reformulates the experiment with discrete observables, so as to avoid this alleged difficulty. In his reformulation we have particle pairs (A_1, B_1), (A_2, B_2), etc., that are spatially separated and prepared in a pure state given by

$$\sum_i c_i f_i(A) \times f_i(B).$$

Here $f_i(A)$ is the eigenvector of the non-degenerate physical quantity $Q(A)$ (for

[256] (*Ibid.*: 259)
[257] Redhead (1995)

the A-particles) for eigenvalue q_i. Krips here goes on to say 'and similarly for $f_i(B)$'. One can only assume that this means that $f_i(B)$ is the eigenvector of the same physical quantity when measured on the B-particle, $Q(B)$, and that its eigenvalue is $-q_i$, since particles are meant to be anticorrelated. In any case this does not affect the rest of Krips' discussion.

Krips then considers two different experimental situations. An interesting point to note is that they do not correspond exactly to the situations that Popper describes: In the first Krips considers the probability for a physical quantity measured on a B-particle to have a specific value and in the second he considers the same probability after another observable has been measured on the A-particle. Once that has been done he concludes that "the scatter of measured values for $P(B)$ in the second situation could not exceed the scatter in the first situation".[258] His discussion before he makes this assertion does not have anything to do with scatter, but it has to do with probabilities. In a footnote he then makes a general claim about scatter. The discussion has a few mistakes, so it will be best if a long passage is quoted first and then an attempt is made to understand what Krips means. It might then became possible to see whether his assertion could follow from a discussion that he could have made, and how it does not follow even then. So Krips says the following:

> In the first situation we estimate the probability $\text{Prob}_1(P(B), j)$ for a physical quantity $P(B)$ to be measured to have the value p_j over the whole ensemble. QT tells us that
>
> (d) $$\text{Prob}_1(P(B), j) = \sum_i |c_i|^2 \, |f_i(B),\, g_j(B))|^2,$$
>
> where $g_j(B)$ is the eigenvector of $P(B)$ for eigenvalue p_j. In the second situation we firstly measure $Q(A)$ for the A-particles, and then consider the ensemble of B-particles for which the corresponding A-particles exhibit the value q_i for $Q(A)$. For this second ensemble we measure the probability $\text{Prob}_2(P(B), j)$ of $P(B)$ being measured to have the value p_j. Clearly $\text{Prob}_2(P(B), j) = \text{Prob}(Q(A), i/P(B), j)$, where the latter probability

258 (*Op. cit.*: 260)

is the conditional probability of $Q(A)$ being measured value q_i given that the measured value of $P(B)$ is p_j. QT tells us that this probability is

(e) $$\text{Prob}_2(P(B), j) = \frac{|c_i|^2 \, |f_i(B), g_j(B))|^2}{\sum_i |c_i|^2 \, |f_i(B), g_j(B))|^2},$$

But if we followed Popper's line of reasoning above then we would have to conclude that the scatter of measured values for $P(B)$ in the second situation could not exceed the scatter in the first situation (while holding in reserve that if we were wrong, then this would indicate action-at-a-distance). And it is easy to demonstrate that this conclusion is inconsistent with QT, *i.e.* that the scatter, *qua* standard deviation, of the distribution defined by (e) above must, in certain cases, exceed that defined by (d) (without involving any action-at-a-distance effects). So Popper is in conflict with a central part of QT, and not just the *CI*.[259]

And in that last paragraph he gives the proof of the claim which concerns the scatter in the following footnote:

Let the standard deviation of $P(B)$ in the case where the B-particles are in the state $f_i(B)$ be $d_i(P(B))$. Suppose this differs from $d_{i'}(P(B))$ for some i'. Now $d(P(B))$, the standard deviation of $P(B)$ for the case at hand, where the B-particles are in a mixture of states $f_i(B)$ (with $|c_i|^2$ the probability associated with the state $f_i(B)$), is easily seen to be $\sum_i |c_i|^2 d_i(P(B))$. Hence $d(P(B))$ must be less than at least one of the $d_i(P(B))$—viz. the minimum one. *Q.E.D.*[260]

So once Krips finds, in his eq. (d), the probability for the B-particles to have any of the eigenvalues for the $P(B)$ observable, he then proceeds to find, in his eq. (e), the same probability after another observable $Q(A)$ has been measured on the A-particle. Then he makes the claim mentioned above concerning the two scatters.

The only rational way of understanding this together with his equation (e) is by considering

$$\text{Prob}_2(P(B), j) = \text{Prob}(P(B), j | Q(A), i),$$

in conjunction with Bayes rule for probabilities to assert:

$$\text{Prob}_2(P(B), j) = \frac{\text{Prob}(P(B), j \wedge Q(A), i)}{\text{Prob}(P(B), j)}.$$

[259] (*Ibid.*: 259–60)
[260] (*Ibid.*: 260)

He must then identify Prob$(P(B), j)$ in the denominator with the probability in his equation (d), and $(P(B), j \wedge Q(A), i)$ with $|c_i|^2 \; |f_i(b),\, g_j(B))|^2$ in order to get his (e).

But even if this identification can be made, this would still be irrelevant to Popper's argument, as Krips makes a comparison between the two as if they correspond to the two situations in his experiment. But they clearly do not. In Popper's experiment the position of the A-particles, which corresponds to Krips' discreet $Q(A)$, is always measured. So when he says that 'if we followed Popper's line of reasoning' then the scatter in the second situation could not exceed the one in the first, he is wrong: Popper's reasoning does not refer to the situation which he is describing. Moreover what he has done is to give two rules, (d) and (e), to find some probabilities for some specific event to happen, and then say that they would have values that are in disagreement with the ones that we should arrive at, had we followed Popper's argument. But Popper's argument was never about probabilities but about scatters, and the values of the scatters do not follow from those probabilities. Or at least they do not follow in the way that Krips has outlined in the above passage.

He then says that the conclusion concerning the scatter of the distribution defined by (e) must, in certain cases, exceed that which is defined by (d). That might be, although the footnote quoted above is wrong. Variances, *not* standard deviations are related thus. Still the situation defined by (d) is not one that is relevant for Popper's experiment.

4.1.5. Conclusion of the Discussion of Krips' Paper

The above analysis of Krips' paper has shown a number of problematic points for his discussion.

First, there is the misguided point concerning the ambiguity "between the initial scatter of measured values for p_y over the B-particles and the resultant spatial scatter of the B-particles along the y-axis after passage through B", that was discussed in section 4.1. Although this has brought into light a confusion concerning momentum measuring apparatuses, it is rather harmless for Krips as it is not vital for the rest of the his argument. Moreover the proportionality between p_y and θ have been shown to exist in the previous chapter, and therefore his manoeuvre is unnecessary.

Second, Krips' point that the product of uncertainties is not "equal to" but "greater or equal to" $\hbar$, which would create a problem for Popper's argument by denying the comparison between the scatters before and after narrowing the slits. This point of Krips' has been muted by the observation that Popper's argument goes through if one grants to him the assumption that for the same experiment the contribution of the experimental uncertainties are constant, as this is indicated in eq. (4.3).

Third, his first point against Popper's argument saying that Popper disagrees with Krips' eq. (c). As it was argued Popper disagrees not with the uncertainty principle, but with its applicability to an indirect measurement, and thus he does not disagree with quantum theory itself. As an outcome of this criticism, Krips makes the prediction that the experiment would yield what Popper considers to be the Copenhagen prediction—a prediction, as we shall see, that is to be denied by the Kim and Shih experiment.

Fourth, Krips' disagreement with Popper that should the result (4.5) be produced by the experiment, this would indicate action-at-a-distance. Krips thinks that (4.5) *can* be explained without it, but, as discussed, this possibility requires an explanation of why, in a virtual slit, the particles in the middle scatter more than the ones at the edges of the virtual slit—an explanation that he does not provide.

Fifth, in Krips' third point there is the mathematical mistake of deducing the product of uncertainties to be infinite, $S_1(p_y) \times S_1(y) = \infty$ in eq. (4.9) above, from one of the two terms in the product being infinite; a mistake that is not really crucial for his argument. But the problem for Krips is that not even one of the uncertainties is infinite according to the theory. Furthermore Krips' point about conditional probabilities, though they might be valid, are rather irrelevant for Popper's argument.

Thus, Krips' criticism was shown to be misguided and wrong; and it does not affect Popper's argument. Moreover Krips makes the prediction that the experiment will produce what Popper considers to be the Copenhagen prediction and this prediction is not what is found in the Kim and Shih experiment. Having said that, it should be noted that Krips' predictions are based on a slightly different version of the experiment—where the slit B is removed and the slit A has two positions (wide and narrow), rather than the one actually used in the experiment with slit A constant and slit B in two positions, narrow and wide open—but this does not affect his argument, except for the obvious change in the results so that they correspond to each other.

4.2. Sudbery on the Popper Experiment

The second response to the Popper experiment in the philosophical literature can be found in the article by A. Sudbery.[261] The title of his article, 'Popper's Variant of the EPR Experiment Does Not Test the Copenhagen Interpretation', gives the main point of the criticism that Sudbery puts forward.

Sudbery notes that Popper uses the EPR set-up to demonstrate that quantum mechanics predicts some effects that can only be understood as a "demonstration of instantaneous action at a distance",[262] and states that his intention is to show two things: First, that these action-at-a-distance effects do not follow from the theory. Second, that Popper has conflicting attitudes towards quantum mechanics as they are demonstrated in the *Schism.*

4.2.1. Sudbery's Analysis of the Popper Experiment

Sudbery starts his description of the experiment by noting that, for Popper, we have pairs of particles that have "interacted in the past".[263] Sudbery notes that it is better to think of this phrase as meaning that they were produced together. This coincidental production has the effect that their y-coordinate are correlated so as to allow the extraction of information for the B-particle from a measurement of the y-coordinate of A. This is allowed by the theory if the wave function of the particles is similar to the EPR wave function which represents particles with equal values for the y-coordinate and opposite values for p_y-coordinate, such as:

$$\psi(\mathbf{r}_{\mathrm{A}}, \mathbf{r}_{\mathrm{B}}) = \phi_1(x_{\mathrm{A}})\, \phi_2(x_{\mathrm{B}})\, \delta(y_{\mathrm{A}} - y_{\mathrm{B}}) \qquad (S.1)$$

(equation numbers like (S.1), (S.2), etc., refer to equations in Sudbery's paper) where ϕ_1, and ϕ_2 are separated localized wave packets. He then says that, for

[261] Sudbery (1985)

[262] (*Ibid.*: 470)

[263] Sudbery quotes Popper with no given reference.

Popper, we need to pass the particles through slits with widths Δy in the y-direction and then measure p_y by means of recording it by an array of detectors arranged around the slit. Repetition of the experiment provides the spread in p_y which is related to the width of the slit by the uncertainty relation

$$\Delta y \cdot \Delta p_y \geq \frac{\hbar}{2} \qquad (S.2)$$

As Sudbery notes, "this holds separately for A and for B".[264] Hence, if the A slit is narrower than B, then, because of the increased knowledge of y_{A}, we must be able to acquire "increased knowledge of y_{B}, so Δy_{B} is reduced even though the slit has not been touched",[265] with the consequence of an increased $\Delta p_{y\mathrm{B}}$, due to eq. (2) above. This produces what Sudbery calls the effect E, i.e., that "previously unaffected counters start to register—without any physical agency having acted in this region".[266]

Having given this rather precise presentation of the experiment, Sudbery gives his criticism of the argument:

> The essential ingredients of this deduction are: (i) the inverse relation between Δy and Δp_y, the uncertainties in position and momentum; and (ii) the correlation between the positions of two particles that have interacted in the past. Neither of these is universally true, whatever interpretation of quantum mechanics is in question; each of them holds only in certain special circumstances. According to the Copenhagen interpretation, they do not hold simultaneously; hence Popper's deduction of the effect E is not valid within this interpretation.[267]

Next Sudbery gives the justification for the above claim. He starts by explaining why the uncertainty relation (i) is not universally true. In doing this he commits one of the mistakes committed by Krips, but nevertheless he arrives at a correct conclusion. He says that the inverse relation between Δy and Δp_y would always

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264 Sudbery (1985: 471)

265 (*Ibid.*)

266 (*Ibid.*)

267 (*Ibid.*)

hold if the uncertainty relation (S.2) instead of being an inequality, was an equation. As explained in section 4.1.1. above, indeed the uncertainty relation is not an equality but an inequality, but that does not stop anyone, like Popper did, from considering an ideal experiment where the experimental uncertainties have been minimized, and the equality holds. It is useful to keep in mind that Popper was considering an idealization of the experiment without worrying too much about the details. Furthermore, whether the equality holds or not is only a matter of the experimental arrangement, and not a matter of which wavefunction we are going to apply to it, as Sudbury claims. Even for a non-ideal experiment, Popper could have always assumed, as I have done in my (4.1.1), that all the experimental errors are incorporated into a constant, before and after the slit is narrowed, so as to keep the character of the inverse relation for the relation. As we shall see, Sudbery predicates his argument on the premise that both the uncertainties are simultaneously infinite, and this is the wrong premise to start his argument.

He says that for a particle going through a slit, the width Δy can be reduced without increasing Δp_y, as long as Δp_y is large before the narrowing of the slit, and this is what happens when the particles are described by eq. (S.1) while entering the slits. And he adds:

> The uncertainties $\Delta y_{\rm A}$, $\Delta y_{\rm B}$, $\Delta p_{y{\rm A}}$ and $\Delta p_{y{\rm B}}$, calculated from this wave function are all infinite; thus, narrowing slit A has no effect on the range of momenta of the particles at either A or B, since these ranges cannot be increased any further. The particles approach A and B at all angles, and all the counters register throughout the experiment.[268]

But this is plainly false: Reduction of the A-slit amounts to $\Delta y_{\rm A}$ changing between two finite values. And because of the delta function in (S.1) the same holds for $\Delta y_{\rm B}$. Besides, as explained earlier, Popper used the assumption of some counters not firing initially, but this is unnecessary, as an increase in scatter can be demonstrated without it, so long as the distribution of firings changes. Of

[268] (*Ibid.*: 472)

course, as we shall see in the next section (4.3), this is still a problem for Popper's argument, but this does not change the fact that Sudbery's argument is based on an erroneous premise.

Nevertheless, next comes Sudbery's conclusion to his argument which, despite the above error, is correct. Sudbery goes on to say that the change of the width of the slit can only change the range of momenta, if the particles have a small range of momenta to start with, just as Popper had thought. He accepts that if the momenta are directed along the x-axis, then only the central counters will fire with the slit wide and the other counters will start to fire when the slit is narrower. But this, he says, only happens when the wave function is not the one given by (S.1) but a different wavefunction which, as he points out, does not have the properties of the EPR wave function that the wavefunction in (S.1) has:

$$\psi'(\mathbf{r}_\mathrm{A}, \mathbf{r}_\mathrm{B}) = \phi_1(x_\mathrm{A})\,\phi_2(x_\mathrm{B}) \qquad (S.3)$$

It is indeed true that a delta function in the wavefunction does deny Popper his conclusion that we can predict both the position and the momentum of the particles, but this does not happen for the reasons given here by Sudbery. Starting from the wrong premises, we can always arrive at a true conclusion, but that does not mean that we have given a proof for that conclusion.

Next Sudbury turns to the second essential ingredients of Popper's deduction. Curiously enough while he said in the earlier quote that this second ingredient is "(ii) the correlation between the positions of two particles that have interacted in the past",[269] he now says that (ii) is "the correlation between the positions and momenta of the particles".[270] Sudbery says that the correlation between the positions and momenta is a property that holds, not generally for particles that

[269] (*Ibid.*: 471)

[270] (*Ibid.*: 472)

have interacted, but only for particles that are described by the wavefunction (S.1), and that such a correlation would be destroyed by any interaction such as a measurement. He gives an example of how such a disturbance can take place: He considers collimating one of the two beams; this demonstrates the momentum correlation but destroys the position correlation. The result is that the wavefunction is now reduced to (S.3) and the subsequent y_A measurement does not give information about y_B. He concludes that Popper cannot justify his essential claim that "narrowing the slit at A will cause new counters to register at B".[271]

Here Popper could retort that he is not interested in the initial spreads in momenta, so he does not see the need to collimate the beams in the first place. This is because Popper has assumed a point source in the first place. And this should be enough for his argument. In order to answer this, it is not enough for Sudbery to say that "But in this case the wave function of the particles is not [(S.1)] but [(S.3)]",[272] as he does here. Sudbery needs to say explicitly why Popper cannot run his argument with a point source, as we shall see in the next section, while examining Redhead's paper. However, Sudbery summarises correctly the reasons why the experiment cannot work in the way Popper has set it out. He writes:

> To summarize: If the particles approach the slits in the EPR wave function, so that observation of one particle gives information about the other, then the spread of the counters that register particles does not depend on the width of the slit. Conversely, if the experiment is arranged so that the spread of counters does depend on the width of the slit, the observation of one particle gives no information about the other.[273]

After the discussion of what he calls the two essential ingredients of Popper's deduction, Sudbery proceeds with a more general discussion of the experiment.

[271] (*Ibid.*)

[272] (*Ibid.*)

[273] (*Ibid.*)

For this discussion there are three essential elements which are relevant: The predictions of quantum mechanics (it is worth mentioning here that Sudbery does not make a distinction between the theory and the Copenhagen interpretation), the idea that every measured quantity has a definite value before it is measured, and the non-existence of action at a distance.

Curiously enough he puts forward the following thesis concerning these three essential elements: Although they are inconsistent when one discusses the experiments, like Aspect's, which involve three incompatible observables (like the components of spin and polarization), this inconsistency is not present when there are only two incompatible observables involved. So, for Sudbery, given the predictions of quantum mechanics, the Popper experiment can be understood by supposing, as Popper would like to, that for each particle, one does not need to postulate action at a distance, in order to attribute simultaneous values to y and p_y, and maintain that the uncertainties in the Heisenberg relations can be understood as merely a statistical scatter of those values in a repeated experiment (just like Popper would wish to!). This is indeed curious given that his summary of his previous discussion amounts to saying that the two variables, y and p_y, cannot be measured simultaneously. And this is happening in exactly the way that has led the Copenhagen fathers to deny the simultaneous existence of y and p_y, together with this interpretation for the uncertainties.

So it is hard to understand how the three ideas are inconsistent only when there are three observables involved, and not in this case that involves only y and p_y. On the other hand, even if we do not see the contradiction between the simultaneous existence of y and p_y and the predictions of the Copenhagen interpretation, there is still the incompatibility of Popper's understanding of the scatter relations and the non existence of action at a distance: If the scattering of the particles is produced, as Popper wants, by the physical collision with the slit, then, in the absence of a slit it hard to see how one can explain the increase in scatter if action at a distance is absent.

In a parenthesis Sudbery says "This is no surprise: it was after all from the conjunction of the proposition that there is no action at a distance with the predictions of quantum mechanics, that is, the existence of the correlations, that Einstein, Podolsky, and Rosen argued that simultaneous values of y and p_y must exist. The argument would have lost its force ifthe premises had been inconsistent.".[274]

That is, he suggests that we should not be surprised by his conclusion that the correlation between values of y (or p_y) can be understood by the supposition that the particles were produced with correlated values for both y and p_y, and therefore no action at a distance is needed, since it is by joining together the absence of action at a distance with the existence of the correlations as predicted by quantum mechanics, that EPR argued for the simultaneous existence of y and p_y. But here Sudbery misses the point: The correlation is not an outcome of quantum mechanics in the EPR argument but a consequence of some conservation principle. Because of the *ad hoc* denial of action at a distance by Einstein, the EPR argument entails the incompleteness of the Copenhagen interpretation, exactly because of its denial of the simultaneous existence of those values. So it would indeed be surprising if there was no incompatibility between this simultaneous existence and the predictions of quantum mechanics. Furthermore Popper does not need action at a distance in order to explain the correlation between the values of y (or p_y) on either side, Sudbery suggests here, but to explain the increase in scatter in the absence of a slit that would provide the physical reason for the scatter. So given Popper's premises, action at a distance is indeed necessary, and the flaw of

[274] (*Ibid.*: 473)

his argument does not lie in his concluding it, given his premises, together with the unfavourable—for him—outcome of the experiment.

4.2.2. Sudbery's Discussion of Popper's Other Views vis.-à-vis. the Experiment

Next Sudbery wants to show that Popper's attitudes with respect to the experiment conflict with some of the stances he has taken on other issues, and he proceeds to discuss these. He first notes that Popper makes a distinction between the original version of the EPR argument and its spin version, and bases this distinction on his characterisation of spin as being "really something very queer". Because of this Popper thinks that spin is possibly affected by action at a distance over small (but not large) distances. Sudbery dismisses this difference between the two versions of the experiment by arguing that large distances are needed only because the EPR experiment demands a possible change for the set-up between emission and detection of the particles.

Irrespective of this, Sudbery notes that the Aspect experiments have violated Bell's inequalities, and since they are derived from the assumptions of the simultaneous existence of all the definite values for the experimentally determined properties and of the non-existence of action at a distance, one of the two has to go. Quantum mechanics denies the first and is therefore compatible with the second, while Popper denies the second because he wants to retain the first. Sudbery finds this insistence of Popper's puzzling, because, as he says, "it is at best an awkward companion to views that he has defended vigorously in other questions".[275] In order to show this he is going to base the discussion on the assumption that particles always have simultaneous positions and momenta. He calls this the "hidden variables hypothesis".

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[275] (*Ibid.*: 474)

The first issue which Sudbery thinks show a conflict between Popper's views is that of *testability.* For Sudbery it is a puzzling fact that Popper would not take a hint from the refutations that the experimental tests had offered to statements that attribute positions and momenta to particles. He thinks that the man who wrote about conjectures and refutations and 'how to demarcate scientific statements from others' must be the first to acknowledge that given the experimental evidence, such statements must have lost their scientific status by now. Sudbery is also puzzled by Popper's inability to understand Bohr's and Heisenberg's pronouncements that a particle cannot have simultaneous values of x and p, since statements about them would fail experimental tests.

There are a couple of points which Popper could have raised to that objection. First, he could have said that all he was trying to do was to test Bohr's and Heisenberg's statement. Given his methodology this is not only a legitimate thing to do, but also a necessity. They say a particle cannot have simultaneous values of x and p. If this is a scientific statement then it must be tested rigorously. If all the tests have so far failed to refute the statement, then Popper is still entitled to think that those tests were not clever enough and to propose a new test. So he sets up an experiment to test this statement. If the experiment has a positive result and a particle is found to have simultaneous values for x and p, then the statement has failed the test and it should no longer be considered a scientific one. If the refutation fails then the statement, like all other scientific hypothesis, is still not rendered true but is instead considered not yet refuted. There is nothing in this procedure which conflicts with Popper's methodology; on the contrary it is a textbook case.

Second, Sudbery says that

> A particular statement of the form "this particle now has position x and momentum p" will, if subjected to experimental test, usually be found to be false. A general statement

> of the form "all particles prepared by experimental procedure E have position x and momentum p" will, if thoroughly tested, certainly be found to be false; for the tests include measurements of position and measurements of momentum, and it follows from the uncertainty relation that one of these is sure to give falsifying instances.[276]

But it is plainly wrong to say that such a statement is false. We can never prove that the particle does not have position x and momentum p. All we can prove is that they cannot be measured in a specific experiment. We can never prove with an experiment the non-existence of green swans. All we can prove is the existence of black or white swans. And given the fact that such a statement can never be falsified, Popper could never have proposed it as a scientific statement. The only thing that Popper could hope to have done, and the thing that he was proposing to do with his experiment, was to falsify the negation of this statement, as this was proposed by Bohr and Heisenberg. Besides, Popper understands the uncertainty relations as applying to ensembles of particles and not to single systems. So the uncertainty relations for him are far from proving the falsity of the above statements. What is happening here is that Bohr and Heisenberg have proposed an interpretation of a theory that accords with experimental results. But this is far from proving the falsity of the above statement.

So it is far from being the case that Popper's insistence on disproving the Bohr and Heisenberg is in any conflict with his general demarcation criteria for scientific statements.

The second issue which Sudbery thinks Popper's attitudes are at odds with is that of *realism.* Sudbery states that he tried not to use this ontological terminology in a discussion about epistemological matters. But here he points out that although physical understanding for Popper does not mean the building of models, he still insists that particles have definite position and momentum. He asks why should particles have these more than they should have colour and smell.

[276] (*Ibid.*: 474)

He even goes on to mention that given Zeno's paradox, velocity is an intuitively puzzling concept. So, if physical understanding is not ultimately linked to building models, and in any case velocity is puzzling, he fails to see why Popper should insist on them.

It has to be pointed out that the conflict which Sudbery here refers to is very hard to see. If one finds puzzling the concept of velocity, this does not mean that Popper did too. And if one is happy to accept that colour and smell are equally important to one's physical understanding as position and momentum are (as Sudbery does when he asks why Popper thinks that particles should have position and momentum more than they should have colour and smell), this again does not show any inconsistency in Popper's thinking. The only hint of such an inconsistency comes from pointing out that Popper does not require the building of models for his physical understanding. So, why the attachment to those two concepts?

The reason why position and momentum are different from colour and smell (to the effect that Popper does require for them such models) is that an object either has them or not. It is not the case, as with position and momentum, that the object has the one and has the other but does not have both, on certain views of reality. The fact that Popper wants to retain them as part of our physical understanding of the world does not have to do with linking this understanding with models. But if one's understanding is linked to particles having positions and to particles having momenta, then it is a natural step to think of particles having positions and momenta before one starts to build models to manipulate them. Again, there seems to be no incompatibility between Popper's views as Sudbery alleges here.

The final issue where Sudbery thinks that Popper holds contradictory views is that of *propensities.* The contradiction which Sudbery sees is based on his idea that if particles are given simultaneous position and momentum, we "assimilate them to the particles of Newtonian mechanics".[277] He points out that Popper thinks of Newtonian mechanics as a "prima facie deterministic theory". And in such a theory:

> ...talk of propensities sounds odd: there is no occasion to talk of the propensity of a particle to *take up* certain states (Popper 1982a, p. 126) if it is already in a definite (but unknown) state and there are no quantum jumps (Popper 1982a, p. 135).[278]

There are two misunderstandings here: First, as he points out at the same page where he characterises Newtonian mechanics as a "prima facie deterministic theory", his "own view is that *indeterminism is compatible with realism*".[279] Second, in the *Schism*, Popper does not deny the existence of quantum jumps. He says, "I do not feel any distaste for them—I should be perfectly happy to accept them if physicists could show good reasons for doing so".[280] In the same section he pointed out that no such reasons (at least none convincing to him) are given to the two replies which Schrödinger received from Born and Heisenberg when he, Schrödinger raised some doubts. He discusses the replies at length, but that discussion, together with the more general one of whether he succeeds to give a coherent account of quantum mechanics as a realistic and at the same time indeterministic theory is not for the present. What can be said, however, is that contradiction between propensities and realism in Popper's views which Sudbery alleges simply does not arise given a thorough reading of Popper.

[277] (*Ibid.*: 475)

[278] (*Ibid.*)

[279] Popper (1982*b*.: 176)

[280] (*Ibid.*: 135)

4.2.3. Conclusion of Discussion of Sudbery's Paper

The first criticism that Sudbery presents for the Popper experiment is that of pointing out that the uncertainty relations should not be read as equalities, but as inequalities. Although this is a valid point, I have argued in the previous section (4.1) that this can be considered as not affecting the argument.

Sudbery's second objection is to point out that either one can hold that the observation of one particle gives information about the other in the pair, or that the counters firing depends on the width of the slit, but not both. It has been argued here that this conclusion is correct but that Sudbery's argument is based on false premises. Nevertheless, the criticism only holds for Popper's version of the experiment, but perhaps it can be overcome in the way that Kim and Shih have done in their version.

Sudbury's objection concerning testability can be turned on its head. Popper has not proposed a *scientific theory* here that was tested by the Aspect experiment and failed. Popper is proposing a *test* for the Copenhagen interpretation. So long as the test can be made to be consistent, it's a legitimate thing for Popper to propose and it does not come in conflict with his attitude towards scientific theories.

Furthermore, on the issue of realism, Popper's insistence on the reality of position and momentum does not stem from an insistence on pictures and metaphors, but on his reluctance to accept that particles have either the one or the other but not both. That is, for Popper, the problem is the alleged complementarity between the two objects.

The title of Sudbery's article, 'Popper's Variant of the EPR Experiment Does Not Test the Copenhagen Interpretation', is found not to be justified, except for the issue of the interplay between the correlation and the firing of counters depending on the width of the slit; but even there the set-up of Kim and Shih attempts, as we shall see, to overcome that problem.

4.3. Redhead on the Popper Experiment

Another reaction to the Popper experiment which can be found in the philosophy literature, was given by M. Redhead in a volume which includes a number of papers to Popper's philosophy.[281] As the title ('Popper and the Quantum Theory') suggests this paper looks more generally at some of Popper's arguments concerning the quantum theory and is not focused solely on the Popper experiment. Nevertheless, the paper does look at some length on the experiment, as it is quite characteristic of Popper's general attitude towards the theory. The paper is in fact quite critical towards Popper's arguments, although Redhead notes that "[Popper] would have applauded this",[282] and that his regret is that Popper would not have the chance to respond to this paper.

4.3.1. Earlier Versions of the Popper Experiment

Redhead starts by describing the earlier attempts by Popper in providing an experiment that will test the Copenhagen interpretation. Popper made his scientific debut as early as 1934 with a short paper which he later admitted was 'a gross mistake'. Popper accepted Einstein's criticism of this earlier version of the experiment, and although Redhead speculates that it might have influenced Einstein because it 'had anticipated an important ingredient in the famous Einstein-Podolsky-Rosen (EPR) paper of 1935' he mentions that Popper had dismissed this possibility.

Briefly, the early experiment involves the idea of using the laws of conservation of energy and momentum in a situation where two particles, A and B, which are initially in momentum and position eigenstates respectively, scatter. By doing

281 Redhead (1995)

282 (*Ibid.*: 163)

this Popper was hoping to retrodict the position of the particle B which, after the collision was in a momentum eigenstate by successive position and momentum measurements performed on the A particle.

Redhead points out that the flaw in this argument is that the measurement of the momentum of the B particle prevents us from retrodicting its position before this measurement takes place. The way this is done by the EPR set up is by inferring one of the two properties of B by a measurement on A. Of course the simultaneous prediction is impossible, as this would amount to a conceptual selection of a 'super-pure' ensemble of the B particles and that contradicts a *theorem* of the formalism (and not of the Copenhagen interpretation) prohibiting such states.

Before starting his description of the recent version of the Popper experiment, Redhead makes the following observation regarding Popper and thought experiments: When it comes to thought experiments, Popper warns that the introduction of an idealisation should only be done if such an idealisation is favourable to the opponent or if the opponent would have to accept them. Despite that, Redhead notes, it looks as though Popper himself did not take his own advice on this matter.

4.3.2. Redhead's Criticism of the Popper Experiment

Next Redhead describes the Popper experiment. The experiment, according to Redhead's description, consists of a source emitting pairs of particles in an EPR state having their positions and momenta correlated. They are selected by slits A and B and then absorbed by coincidence counters arranged in a semicircle behind the slits. Given the correlation, by narrowing the A slit we also constrain the B particles with respect to their y-coordinate, and thus increase our knowledge of y and so we will have, according to the Copenhagen interpretation, a wider scatter in the transverse momentum, so counters that do not fire before the narrowing of the slit will have to fire now. Redhead concludes the description:

> Popper is convinced that this effect would not happen, so if performed the experiment would, according to Popper, constitute a decisive refutation of the Copenhagen interpretation.[283]

After he completes the description, Redhead states the flaw in Popper's argument: Popper "misunderstands the nature of the EPR correlations".[284] (Redhead also refers to the two papers by Krips and Sudbery, examined earlier as making similar points to his. As we shall see Redhead's points are different from Krips', as they have been shown to be misguided earlier. With respect to Sudbery's paper, we have seen that he makes the comparison with Popper's other views which is rather missing the point, but he does come to the same criticism as Redhead, although he predicates it on an erroneous assumption.) Redhead proceeds to explain that if we write down the quantum mechanical state that can describe a pair of particles which are in an EPR state and have correlated position or momentum values, then the effects predicted by the *formalism*—not by any specific interpretation—are the ones which Popper predicts:

> The EPR state at the time $t = 0$ of emission of the two particles, in respect of the transverse y-dimension is of the form
>
> $$|\Psi_0\rangle = \int_{-\infty}^{\infty} |y\rangle \otimes |y\rangle \, dy \qquad (R.2)$$
>
> $$= \int_{-\infty}^{\infty} |p_y\rangle \otimes |-p_y\rangle \, dy \qquad (R.3)$$
>
> The form $(R.2)$ demonstrates the position correlation and the form $(R.3)$ the momentum correlation.
>
> So the EPR source, according to $(R.2)$ is not a point source, like S in Figures 2 and 3, but an infinite *incoherent* line source. Why incoherent? Because for any observable A_1 of particle 1, for exam$\langle\Psi_t|A_1|\Psi_t\rangle = \int_{-\infty}^{\infty} \langle y_t|A_1|y_t\rangle \, dy$me t is
>
> $$\langle\Psi_t|A_1|\Psi_t\rangle = \int_{-\infty}^{\infty} \langle y_t|A_1|y_t\rangle \, dy \qquad (R.4)$$

[283] (*Ibid.*: 168)
[284] (*Ibid.*)

where $|y_t\rangle$ is the time-evolved state at time t which starts at $t = 0$ as an eigenstate $|y\rangle$ of position. Similarly for observables referring to particle 2, so expectation values are *additive*—there is no interference between Schrödinger waves originating at different values of y.[285]

This means that, in order to have the desired values, the EPR state has to be described as an infinite line source. But if that is the case, the particles can then approach the slits not just from the centre point between the two slits, but from any point above and below it. This gives a different geometry than the one which Popper had envisaged which makes all the counters fire irrespective of the width of the slit. The conclusion drawn by Redhead from this is that the formalism fails to predict what he calls the 'Popper effect', namely the firing of counters after we narrow the slit which did not fire when the slit was wide. And since the formalism does not predict it, Popper could not expect the Copenhagen interpretation to predict it, and then be tested against the failure or not of the effect to materialise. The interpretation can only interpret what the formalism predicts.

Redhead then reports a private discussion he had with Popper concerning this point and his diagrams that showed how the 'Popper effect' failed to materialise according to the theory. But, as Redhead reports, Popper "refused to accept that [Redhead] had a general argument that narrowing slit A could not make a counter fire behind B, that would not have fired when A was wide".[286]

Popper countered that maybe a different geometry would bring back the effect he wanted. The obvious choice for this would be to consider a source of finite height:

But truncating the EPR source by replacing the infinite limits in Eqn (2) by finite limits, then we lose the momentum correlation, i.e. we can no longer rewrite (2) in the form (3). In other words, with a truncated source, a narrow beam on the right ... does not correlate with a narrow beam on the left, with resulting diffraction as Popper supposed.[287]

[285] (*Ibid.*: 168–9)
[286] (*Ibid.*: 169)

[287] (*Ibid.*)

This means that if the height of the source is small, and of the same order as the width of the slits, then obviously the source cannot "see" the counters behind the slit that are at large angles, but, depending on how wide the slits are open, it can only "see" the ones that are at small angles. Unfortunately for Popper this does not do the trick. The effect of making the source shorter is that in the above theoretical description the limits from minus infinity to plus infinity would have to be replaced by some finite numbers. This means that the position eigenstate in (R.2) cannot be rewritten as the momentum eigenstate in (R.3), and the two systems do not have correlated y-momenta. Since the y-momenta do not have to be equal for a pair of particles that go towards the opposite slits, the particles that go through the A slit would not have counterparts that would go *only* through the mirror image of the A-slit on the B side, but they would go through a wider slit due to the fact that their y-momenta might be larger, or in the opposite direction, than those of the A-particles. This means that with a point source we have no longer acquired as accurate a knowledge of the y-component of the position for the B-particles as we have for the A-particles in the absence of the B-slit. Consequently there would be no more scatter according to the Copenhagen interpretation.

But this was not the end of the matter for Popper. Redhead reports that he was still unconvinced, and in order to convince him he provided a general proof that the 'Popper effect' cannot really happen. This proof utilises the fact that to narrow the A slit amounts to the selection of a subensemble on the B-side. That is, we are selecting fewer of the particles that would otherwise hit the B-counters coincidentally with particles on the A-side. This means that the narrowing of the A-slit makes the B-side counters fire less during the same time interval, and, at the same time, fewer of the particles that would otherwise (with a wider A-slit) make the B-side counters fire, do so.

Now, if the 'Popper effect' amounts to making some B-side counters at some wide angles fire when the A-slit is narrow, given that they do not fire when the A-slit is wide, then it amounts to selecting the B-side particles that would go to those angles from the ensemble of particles that had no particle going there in the first place. But to select a group of particles some of which do x from a bigger group of particles that do not do x is impossible! In Redhead's words:

> Consider any observable $\mathcal{A}$ for the particle passing through the A slit with eigenvalues a_i similarly for $\mathcal{B}$ on the left with eigenvalues b_j. Then since $\mathcal{A}$ and $\mathcal{B}$, commute we can write, in an arbitrary state $|\Psi\rangle$
>
> $$\mathrm{Prob}^{|\Psi\rangle}(b_j) = \sum_i \mathrm{Prob}^{|\Psi\rangle}(b_j|a_i) \times \mathrm{Prob}^{|\Psi\rangle}(ai) \qquad (R.5)$$
>
> Now suppose $\mathrm{Prob}^{|\Psi\rangle}(b_j) = 0$ and $\mathrm{Prob}^{|\Psi\rangle}(a_i) \neq 0$ for some particular index i. Then (5) implies that, under these conditions, $(b_j|a_i) = 0$, i.e. selecting a subensemble on the left by conditionalising on the right with an event that has a non-zero probability of occurring, cannot convert a zero probability for b_j into a non-zero probability for b_j.[288]

And since those events (the firings of counters at wider angles) are not expected to be produced according to the theory, it is wrong to expect their absence to count as a test for the Copenhagen interpretation.

Redhead's report from that conversation ends here (leaving out Popper's opinion on this last argument). Before leaving the subject of the experiment, Redhead also notes that the Popper effect can in fact be produced by a thicker A-slit (with thickness larger that its width) which gives a larger ensemble of particles that will make the A-side counters fire. The larger ensemble of particles originates from points in the infinite line source that is prevented from producing particles which would reach the counters when the slit is wide because of the thickness of the slit. But when the thick slit is narrowed the particles which originate from those points would reach the counters due to diffraction effects that are produced by the thick slits, and at the same time their counterparts on the B-side would

[288] (*Ibid.*: 170)

reach counters at larger angles whereas with the A-slit wide those particles are not counted. So the 'Popper effect' is produced but of course not in any way that is useful to Popper's argument. Redhead then moves on to more general considerations concerning Popper's views on the quantum theory.

4.3.3. Indeterminism and Propensities, State preparation and Measurement, Nonlocality and Bell's Inequality.

Redhead points out that Popper's approach to quantum theory rests on two key ingredients: *indeterminism* and *propensities*. Furthermore he also discusses the issues of state preparation and measurement, and nonlocality and Bell's Inequalities.

There are a number of reasons that Popper uses for his support of indeterminism, both epistemologically and metaphysically. Epistemologically, these reasons do not only apply to quantum physics but also to classical physics. For example, the issues of classical instabilities, of self-prediction, or of a prediction that influence the predicted event. Metaphysically, for Popper, determinism amounts to the unreality of time, together with a denial of human freedom. Furthermore, although Popper is on Einstein's side when it comes to attacking the Copenhagen interpretation, especially on the issues of the theory's finality, he opposes Einstein's defence of determinism.

Given that to Popper the world is indeterministic, probabilities cannot merely reflect our ignorance of some initial conditions that would affect a future event, but in some cases they have to reflect some genuine feature of the world. Since the state of our knowledge cannot have some physical effect on events that are outside our influence, probabilities cannot be epistemic. Furthermore, Popper found the frequency view of probability inadequate to explain the emergence of some stable limiting frequencies for particular kinds of events,[289] and this led

[289] In his (1983: 283–87), for example, Popper claims that the frequency interpretation can only

him to his propensity interpretation of probability, which he thought applies to quantum theory.

Furthermore, Heisenberg's uncertainty relations, for Popper, show our inability to prepare ensembles with precise positions and momenta, and not limitations on the act of measurement. These two kinds of interactions disturb the particles in an intrinsically stochastic manner, but they otherwise behave classically between interactions. Now propensities become manifestations of properties pertaining not only to particles but to them together with the environment with which they interact. Furthermore, the collapse of the wave function becomes for Popper a consequence of changing the conditions for a probability distribution before and after the act of measurement. Redhead points out that, when this view is considered in the context of joint probability distributions, it does eventually bring Popper into contradiction with a theorem by Fine,[290] showing that those joint distributions do not obey all the properties for probability distributions.

For the other important issue which arises for any realist interpretation such as Popper's, the proofs of nonlocality through violations of the Bell inequality, Popper had two kinds of reply. First, instead of interpreting the experiments as proofs of nonlocality they could be seen as tests between the Lorentz and the Einstein interpretations of relativity. Second, Popper was keen to exploit the possibility of technical flaws in the proofs of the impossibility of local hidden-variable theories.

Redhead concludes his paper by paying tribute to Popper for his contributions to the philosophy of quantum mechanics (such as the important distinction

deal with singular probability statements as if they were grammatically singular and that the only way for an event to be attributed a probability is if it were "an element of a sequence of events with relative frequency". On the other hand his propensity interpretation attributes probability to single events as a "representative of a *virtual or conceivable sequence* of events" rather than an actual sequence.

[290] Fine (1973)

between state preparation and measurement) and for his lone battle against the view of the finality of the Copenhagen interpretation. And Redhead added that, despite the technical flaws in some of the arguments which he used, his influence has definitely been beneficial for the subject.

4.3.4. Discussion of Redhead's Paper

Returning to the subject of the experiment, this last conclusion of Redhead's finds an acute application. Redhead shows how Popper's argument fails when it comes to a closer examination that would take into account the technical details of the mental construction that Popper invented. But there is no denying that the argument has brought to the surface some of the detailed issues which pertain to the subject. Moreover, the examination of the two previous papers has indicated that although flawed, the argument is notorious in not allowing the discovery of the flaw involved. A very careful investigation is required here.

The discussion reported by Redhead between himself and Popper tells a rather strange story: First, Popper was presented with an argument, based on the quantum *formalism*, which shows that narrowing the A-slit does not give us any increased knowledge of the position of the B-particles, but this does *not* convince him. Then Popper was presented with an argument, based on the classical probability theory, showing that the 'Popper effect' is not expected to happen. In the absence of any reported retort by Popper it can be assumed that Popper conceded defeat at this point. But the fact that he might have been convinced by the classical argument and not by the one that is based on the quantum formalism looks rather strange. After all, he was arguing that he does not expect the effect to happen based on his realist intuition, and this intuition goes hand in hand with the classical probability premises. So in a sense this probabilistic proof argues Popper's Point: *If* classically we do not expect the event to happen, *and* the Copenhagen interpretation predicts it, *and* the experiments do not verify it, *then* we have a test which refutes the Copenhagen interpretation and vindicates Popper. *If* on the other hand, the tests show that the effect does materialise, *then* we have yet another test which goes against our classical intuitions and supports the Copenhagen interpretation. So following the logic of the Popper experiment the second argument should not have mattered so much.

Furthermore, as pointed out earlier, the 'Popper effect'—the firing of the counters after narrowing the slit that did not fire before—is not really essential to Popper's argument concerning his experiment. All Popper needs to do for his argument is say that, due to the increased knowledge which is acquired for the positions of the B-particles due to narrowing the A-slit the *scatter* of particles is increased. But, as proven earlier, this increase in scatter can be demonstrated with the same counters firing, as long as the distribution of firings change so that, on average, more firings are observed in wider angles and less at smaller angles. The 'Popper effect' is not really essential to Popper's argument.

The real problem for Popper's argument stems from the first argument which Redhead presented to him. So one would expect the story to be rather different. Popper should have accepted the second argument as arguing *for* his position, and should have conceded defeat on the first argument. This is because the first argument is fatal for his position.

But could Popper argue that this was his argument in the first place? As noted in section 3.2 concerning the possible outcomes of the experiment, the quantum formalism, unlike the Copenhagen interpretation, yields possibility 1, namely that the 'Popper effect' would not appear. In Popper's words:

> If, as I am inclined to predict, the test decides against the Copenhagen interpretation, then this does not mean that quantum mechanics (say, Schrödinger's formalism) is undermined; although it would mean that Heisenberg's claim is undermined that his formulae are applicable to all kinds of indirect measurements (a claim which the adherents of the

Copenhagen interpretation—such as von Neumann—would undoubtedly regard as part of quantum mechanics).[291]

Redhead argues that "the 'Popper effect' is not predicted according to the formalism of quantum mechanics, and hence not predicted according to any interpretation of that formalism; in particular, it is not predicted by the Copenhagen interpretation",[292] but this was not enough for Popper and his quote above may explain why. Maybe he did want to say that that was exactly his argument. But this is not an avenue open to Popper. The reason for that, is that the formalism does not just preclude the 'Popper effect', but it does so by denying the increased knowledge of the position of the B-particles when the A-slit is narrowed. It is not just that the Copenhagen interpretation does not predict something which the formalism predicts will not happen; but it does not have to explain the 'Popper effect' since the reason that Popper alleges that forces Copenhagen interpretation to predict it, namely the increase in knowledge, does not follow from the theory.

To make this point more clear, Popper's argument is the following:

1. The formalism (and his realist interpretation of the formalism) does not predict the increase in scatter (or the 'Popper effect').
2. The formalism and his realist interpretation of the formalism do predict an increase in knowledge of the y-position of the B-particles when the A-slit is narrowed.
3. According to the Copenhagen interpretation any such increase in knowledge will be followed by a decrease in the knowledge of the incompatible observable, the momentum p_y, and will therefore produce an increased scatter for p_y.
4. *Thus*, if the test does not show this increase in scatter for p_y, then the

[291] Popper, (1982: 29)
[292] (*Op. cit.*: 169)

Copenhagen interpretation of the formalism (but not the formalism itself) is refuted and his interpretation vindicated .

This is a valid argument. And it is immune to the second argument put forward by Redhead. But the first argument by Redhead is fatal to the conclusion, as it renders the second premise false: There is no increase in knowledge. And therefore there is no increased scatter for p_y according to the Copenhagen interpretation.

Popper failed to see this. Of course he also failed to see that the first premise also brings about some problems for the formalism should the increase in scatter for p_y *be* observed. But the interesting point is that he might have also failed to see how he could use this to make a slightly different argument. This will be expanded on later.

The discussion between Popper and Redhead is reminiscent of the exchange between Einstein and Bohr during the 1930 Solvay Conference on magnetism. Einstein presented his counterexample to the uncertainty principle. A box with a small shutter on the wall contains some photons one of which is allowed to escape through the shutter during a very short precisely measured time interval. Weighing of the box before and after the escape allows, through Einstein's equation, $E = mc^2$, the precise measurement of the photon's energy. Einstein concluded that this allows a violation of the uncertainty principle because both the time of passage and the energy of the photon can be measured with an arbitrary precision.

Bohr came back the next day and presented his counterargument. He showed, by taking into account the particular measuring procedures that are involved in the above thought experiment, that there is a fundamental limit in the accuracy of the time measurement due to variations in the rate of the clock in varying gravitational fields. Ironically enough he showed this by an appeal to Einstein's theory of relativity.

There are noticeable similarities in the two stories. Einstein, like Popper, presents a thought experiment which aims to show that we could overcome the uncertainty principle by an ingenious design which would allow the measurement of two incompatible observables, one directly and one indirectly. By making an idealisation neither take into account all the technical details of their set up. Then Bohr, like Redhead, looks into those details and shows that the original presentation overlooks some detail that means that the indirect measurement cannot be performed to the desired accuracy. Unfortunately the similarities stop here, as Popper does not follow the example of his hero in conceding that this technical detail does indeed neutralise the argument.

Maybe Popper could have followed Einstein even further. Einstein abandoned his attempt to defeat the uncertainty principle and, given his conviction for determinism and the theory's inherent indeterministic nature, sought to show that the theory is incomplete, by the EPR thought experiment. Popper does not oppose the theory on the same grounds. He has no qualms with indeterminism. Had Popper accepted that, technically, the theory shows that it is not possible to deduce the position of the B-particle from a measurement of the A-particle if the source is a point source, he might have extended the argument to show that this means the inability of the theory to describe a point source which emits particles that have both position and momentum correlations although such pairs are described for infinite sources.

This brings about a different point that Redhead has made. As noted earlier he remarked that, although when it comes to thought experiments Popper wants to allow the introduction of idealisations only when these are favourable to the opponent or if the opponent would have to accept them, it looks like he has not followed his own advice. This remark becomes clearer now. When Popper introduces the idea of narrowing the A-slit and so deducing the position of the B-particle, he is thinking of them as classical particles with perfectly correlated positions and momenta. Had he followed his own advice, he should have immediately conceded to Redhead that, since he is talking about quantum particles, he should accept the quantum description and drop the classical assumption. And since the quantum description does not allow him to deduce the indirectly measured position, Popper should have been immediately convinced by Redhead's first argument.

Yet another point concerning idealisation can be made with respect to the 'Popper effect'. As noted, the 'Popper effect' amounts to Popper making the assumption that he can possibly have two different widths of the slits w_1, and w_2, with $w_1 > w_2$, such that when particles are going through w_1 some of the extreme counters do not fire, and when particles are going through w_2 those same extreme counters do fire. We have seen how this assumption causes trouble for Popper's argument and how a detailed analysis makes it unnecessary for his argument.

4.3.5. Conclusion

It was pointed out in the previous discussion that the false assumption of a point-like source, that is crucial for Popper's argument, is problematic in a classical manner: Since the source does have a finite size, it follows that the image of the slit A, when reflected through the source, does not have the same size at the plane of the virtual slit B. Redhead gives to this problem for Popper's argument its quantum description: if the source has a finite size then the position eigenfunction in (R.2) cannot be rewritten as the momentum eigenfunction in (R.3), and so the y-momentum correlation is lost. It should be noted that in the two descriptions the problem is the same but inverted: For the classical description the size of the source is *finite and therefore non-zero and too big*; for the quantum description the size of the source is *finite and therefore too small.*

This problem is fatal for Popper's argument. But, as we shall see, Kim and

Shih have argued that this problem can be solved by a technical addition to the set-up of the experiment. It has also been argued here that the various other problems for Popper's argument that were pointed out by Sudbery and Krips were invalid or misguided. What is important here is that, *provided that the problem of the source can be solved*, the Popper argument can provide a crucial test for the Copenhagen interpretation.

Chapter 5: Physicists on the Popper Experiment

The Popper experiment also created reactions in the physics literature. These can be separated in two groups: First, the reactions that appeared before the Kim and Shih experiment which dealt with the analysis of the thought experiment, and second the reactions that appeared after the Kim and Shih experiment which mainly tried to deal with the results of the experiment and their interpretation.

The first group is examined in this chapter, with the examination of the papers by Collett and Loudon[293] and Peres.[294] Popper actually replied[295] to the Collett and Loudon paper and they added their reply[296] in the same issue of *Nature*; Popper added a correction[297] in the next issue.

The second group will be examined after the Kim and Shih experiment has been discussed.

5.1. Collett and Loudon on the Popper Experiment

An important reaction in the physics literature is that of Collett and Loudon. They try to show, by a more quantitative analysis than the one which Popper had provided, that Popper's proposed experiment does not constitute a crucial test of the Copenhagen interpretation of quantum mechanics. Their argument rests ultimately on the problem presented by the finite size of the source that has been discussed earlier.

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[293] Collett and Loudon (1987*a*)

[294] Peres (2002)

[295] Popper (1987*a*)

[296] Collett and Loudon (1987*b*)

[297] Popper (1987*b*)

5.1.1. Collett and Loudon Analysis of Popper's Argument.

Collett and Loudon open their paper by giving a summary of Popper's argument. They start their analysis by referring to the source which emits pairs of particles in opposite directions. They use Popper's example of positronium which decays into pairs of γ-rays. They describe the set-up with the left slit open, the right slit at some width s_R giving us knowledge of the position in the y-direction, and the coincidence detectors arranged in a semicircular fashion behind the slits. Due to the opposite momenta of the pairs of particles, the measurement on the right gives us an indirect measurement for the particle on the left, and this, according to Popper, should give an increased scatter on the left.

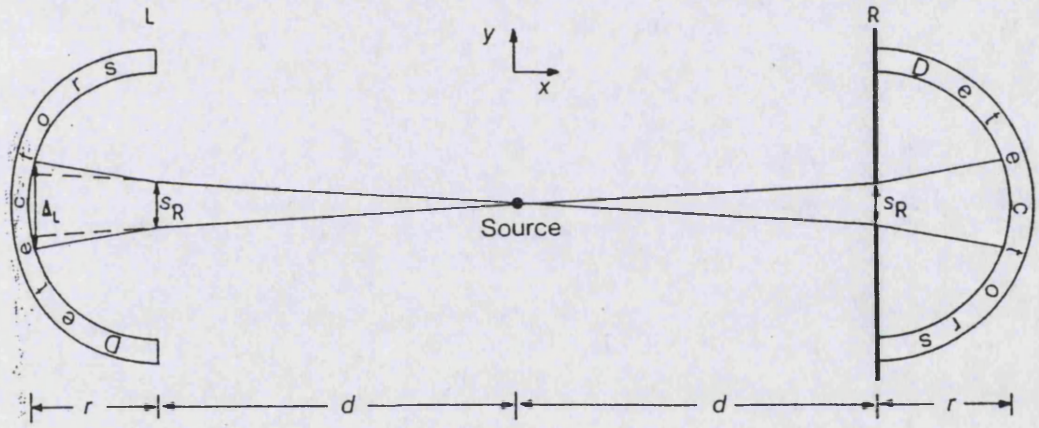

Figure 2. Collett and Loudon's representation of Popper's experiment.[298]

But after they give this qualitative description of the experiment, Collett and Loudon give a more quantitative presentation of Popper's argument. They present a calculation of the range of activated detectors, Δ_L, by which they mean the effective cross-sectional height of the beam of particles as they are very close to the detectors. This, they say, according to the standard interpretation, is given

[298] (*Ibid*: 671)

by:

$$\Delta_L^2 = \left(\frac{d+r}{d}s_R\right)^2 + \left(\frac{r\lambda}{4\pi s_R}\right)^2 \qquad (CL.1)$$

where λ is the particle wavelength, d is the distance between the source and the slits, and r is the radius of the semicircle of detectors. They explain that

> The first term on the right-hand side of equation (1) is the geometrical contribution of the y-coordinate uncertainty and the second term is that of the y-momentum uncertainty produced by the virtual slit at L.[299]

For convenience's sake they rewrite the above equation in dimensionless form, by a conversion of all lengths in units of

$$(d\lambda/4\pi)^{1/2} \qquad (CL.2)$$

so that we get

$$\Delta_L^2 = \left(1+\frac{r}{d}\right)^2 s_R^2 + \left(\frac{r}{d}\right)^2 \frac{1}{s_R^2} \qquad (CL.3)$$

This gives the variation of Δ_L^2 on the left with the width of the source s_R on the right. If the width is small enough:

$$s_R < [r/(d+r)]^{1/2}, \qquad (CL.4)$$

then the second term dominates and so Δ_L^2 increases when the width s_R decreases. They add that if the effect of 'mere knowledge' as Popper puts it was absent then the two are proportional. This of course is to be expected as, in that case, Δ_L would measure the image of the source through the slit on the projectors. Collett and Loudon then proceed to criticise the argument by showing its flaw.

Before discussing their criticism, it is instructive to try to deduce their first equation. First we should mention that in their derivation, Δ_L refers to the vertical length that will be hit by the particles, on a screen which is a distance r from the slits. This of course introduces an approximation since the detectors are

[299] (*Ibid.*: 671)

not on a vertical screen but arranged on a semicircle. The approximation between the vertical distance and the angle θ at which the detectors are situated is good enough as long as we refer to small angles but it fails at larger angles.

Now, the first term represents the image of the source through the slit on the detectors. This can be deduced by considering, on the right side, the slit being at a distance d from the source and the screen of counters at a distance $d + r$. Then, by similar triangles, for the image of the slit, Δ_{Rg} (the subscript Rg for the geometrical term on the right) we would have:

$$\frac{\Delta_{Rg}}{d+r} = \frac{s_R}{d} \qquad \Rightarrow \qquad \Delta_{Rg} = \frac{d+r}{d} s_R$$

By equating the image of the slit on the right with the image of the virtual slit on the left we get

$$\Delta_{Lg} = \frac{d+r}{d} s_R$$

This indeed gives the term in the first parenthesis. But the fact that the term is inside the parenthesis, and the parenthesis is squared, means they are treating this as a variance (otherwise twice the cross product is missing). But what they are calculating here is the width of the distribution, not its variance. If we were to assume that the particles are uniformly distributed within $(d + r/d)s_R$, then the variance is equal to $[(d + r/d)s_R]^2/12$; i.e., there is a factor of 12 missing from their calculation. But that is not so important.

What is more important is the second term. Collett and Loudon tell us that this term represents the contribution of the y-momentum uncertainty produced by the virtual slit on the left. The calculation is done based on the size of the slit on the right s_R. The term inside the parenthesis involves the radius r, i.e., the distance between the slit and the counters. This means that their calculation must have proceeded as follows: The particle travels at some angle from the slit to the screen of the detectors. During this flight it covers some horizontal distance r

and some vertical distance $\Delta_{Ru}/2$ (the subscript Ru for the uncertainty term on the right). This distance is half the Δ_{Ru} since there are particles travelling in the negative y direction as well. This distance would be proportional to time of flight and to the momentum in the y direction:

$$\frac{\Delta_{Ru}}{2} \sim t \cdot p_y$$

Now the vertical momentum can be found from the uncertainty principle, so we have:

$$\frac{\Delta_{Ru}}{2} \sim t \cdot \frac{\hbar}{s_R}$$

Also the time of flight would be proportional to the horizontal distance r and inversely proportional to the horizontal momentum p_x, and for this they use the $p = h/\lambda$ formula:

$$t \sim \frac{r}{p_x} = \frac{r\,\lambda}{h}$$

So putting this together we get:

$$\frac{\Delta_{Ru}}{2} \sim \frac{r\,\lambda}{h}\frac{\hbar}{s_R} = \frac{r\,\lambda}{2\pi s_R}$$

By equating the uncertainty on the left with the uncertainty on the right we get

$$\Delta_{Lu} \sim \frac{r\,\lambda}{\pi s_R}$$

which is the second term with a factor of $1/4$ missing. By assuming that the squares of these two lengths, Δ_{Lg} and Δ_{Lu}, are variances and adding them we get their equation $(CL.1)$ except the factor of 4. But the problem here is that they assume that the particles approach the slit with some momentum associated with their wavelength λ and they carry on past the slit with the same horizontal momentum. At the slit, however, they undergo a collision and this might change their momentum in such a way that their horizontal momentum might even become zero.

But the most important problem in the above derivation is that it makes vertical distance travelled by the particles dependent on the wavelength λ of the particles. That is, the faster they go the further they travel vertically in the time interval that they take to travel the horizontal distance. But the particles in this case are photons whose velocity does not depend on their momentum.

Furthermore they derive their third equation by measuring all lengths in units of $(d\lambda/4\pi)^{1/2}$, but this assumes that the wavelength of the photons is a constant, something which is not the case.

The correct way to understand this experiment is to consider the geometrical image of the slit and then consider the particles which reach the counters outside that geometrical image as having a vertical momentum which is explained by the uncertainty principle. But this vertical momentum has values which are inherently uncertain. Furthermore even if we assume the average value of momentum as this is given by the uncertainty principle there is no way to determine the vertical distance travelled if the screen was a vertical one, as this would also depend on the horizontal component of the momentum and this is not predetermined.

If the particles are photons, Collett and Loudon cannot use the above derivation because photons do not travel at a speed determined by their wavelength and momentum. If the particles are not photons Collett and Loudon still cannot use the above derivation because the horizontal momentum before the slit does not equal the horizontal momentum after the passage through the slit as the collision with the slit will in general alter that momentum.

5.1.2. Collett and Loudon Criticism of Popper's Argument.

Collett and Loudon go on to present the flaw in Popper's argument, which is that the argument assumes that the source is resting at a point. As they say: "A more careful analysis must allow for uncertainty-principle limitations on the accuracy with which the state of the source can be specified."[300] This is shown in their modified schematic of the Popper experiment (Figure 3 below) and has the result of not two but three contributions to the spread of activated detectors on the left. The first is the geometric term. Since the source has finite width Δy the right slit will have an image through the source that is not equal in height with the height of the slit on the right, s_R. Collett and Loudon tell us that this geometric term is given by

$$\Delta'_L = 2\Delta y + s_R + (r/d)(\Delta y + s_R) \qquad (CL.5)$$

Indeed by considering the extension of the line that starts on the upper limit of the right slit and passes from the lower limit of the source, and the extension of the line that starts on the lower limit of the right slit and passes from the upper limit of the source, we can find the height of the projection of the right slit on the vertical screen as the length of the line between these two lines at a distance $d + r$ from the source.

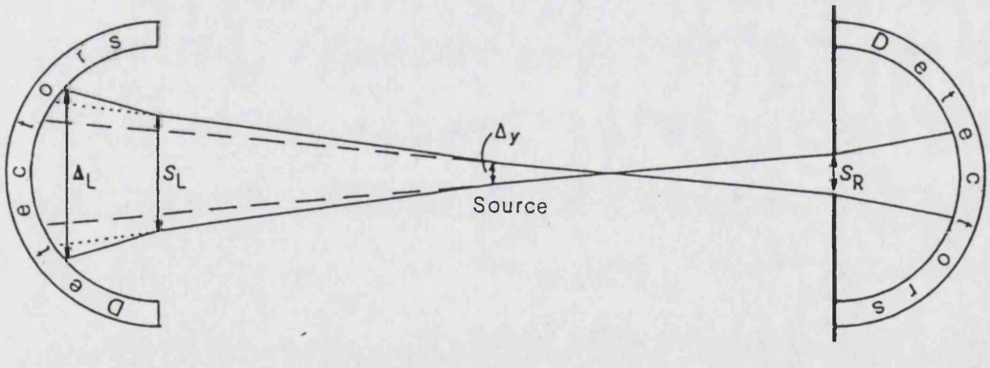

Figure 3. Collett and Loudon's modified representation of Popper's experiment.[301]

300 (*Ibid.*: 671)

301 (*Ibid*: 671)

These two lines together with the right slit and the projection on the screen, form two similar triangles with the slit and the right screen as their bases. The height of the smaller one, call it x, can be found by considering the two triangles between the source and the right slit:

$$\begin{aligned}\frac{\Delta y}{d-x} = \frac{s_R}{x} \qquad &\Rightarrow \qquad \frac{d-x}{x} = \frac{\Delta y}{s_R} \\ &\Rightarrow \qquad \frac{d}{x} = \frac{\Delta y + s_R}{s_R} \\ &\Rightarrow \qquad x = d\frac{s_R}{\Delta y + s_R}\end{aligned}$$

Using this we can find the height Δ_L':

$$\frac{\Delta_L'}{2d+r-x} = \frac{s_R}{x} \qquad \Rightarrow$$

$$\Delta_L' = \frac{s_R}{x}(2d+r-x) = \frac{s_R}{d\frac{\Delta y + s_R}{s_R}}\left(2d+r-d\frac{\Delta y + s_R}{s_R}\right)$$

$$= (\Delta y + s_R)\left(2 + \frac{r}{d} - \frac{s_R}{\Delta y + s_R}\right)$$

$$= 2\Delta y + s_R + \frac{r}{d}(\Delta y + s_R)$$

as given by Collett and Loudon.

Then there is the second term that comes from the momentum that is related to the height of the source uncertainty. Collett and Loudon tell us that this term relates to the uncertainty in such a way as to give a relaxation of the requirement for the two momenta to be exactly correlated, and is given by

$$\Delta_L'' = (d+r)\lambda/4\pi\Delta y \qquad (CL.6)$$

Again the calculation is similar to the second term in their first equation. The same criticism as before applies here, as the length is again dependent on the wavelength.

Finally the third term is due to the virtual slit that gives a term that is the same as the second term in their first equation, but for the width of the slit, which is now different:

$$\Delta_L''' = r\lambda/4\pi s_L \qquad (CL.7)$$

For this width s_L their calculation involves two terms: one isthe geometric one whose calculation is very similar to the one for Δ_L', and the other comes from the momentum that is responsible for the term in eq. $(CL.6)$. Again, as in all previous cases, Collett and Loudon do not give the calculations for s_L but they present it in a squared form. Their equation reads:

$$s_L^2 = (2\Delta y + s_R)^2 + (d\lambda/4\pi\Delta y)^2 \qquad (CL.8)$$

So finally by using this last equation in eq. $(CL.7)$, and by rewriting everything in the dimensionless form from eq. $(CL.2)$, they produce the following dimensionless expression:

$$\Delta_L = \left\{2\Delta y + s_R + \frac{r}{d}(\Delta y + s_R)\right\}^2 + \frac{(d+r)^2}{d^2(\Delta y)^2} + \frac{(r/d)^2}{(2\Delta y + s_R)^2 + (\Delta y)^{-2}} \qquad (CL.9)$$

Collett and Loudon claim that differentiation can show that Δ_L as given by this expression is an increasing function of s_R. This expression, unlike eq. $(CL.3)$, is dominated by the geometric term and therefore the smaller the width of the right slit s_R, the smaller the range of the activated detectors:

> There is no choice of experimental parameters for which a reduction in the width of the slit causes an increase in the spread of particles on the opposite side of the source...
>
> The uncertainties in transverse momenta produced by the finite slit and source dimensions are of course similar to those obtained from classical diffraction theory, and the above results for Δ_L do not depend on Planck's constant.[302]

Furthermore it should also be mentioned that Collett and Loudon refer to the paper by Sudbery, commenting that his analysis lacks the detailed derivations

[302] (*Ibid.*: 672)

that they present, and that Sudbery's more qualitative conclusions do not agree with theirs. This is not exactly the case. Sudbery gives the following summary of his position:

> To summarize: If the particles approach the slits in the EPR wave function, so that observation of one particle gives information about the other, then the spread of the counters that register particles does not depend on the width of the slit. Conversely, if the experiment is arranged so that the spread of counters does depend on the width of the slit, the observation of one particle gives no information about the other.[303]

The second part of his conclusion corresponds to the situation that Collett and Loudon describe, because that situation is one where the observation of one particle gives information about the other that is not sufficiently precise to allow us to deduce a violation of the uncertainty principle.

5.1.3. Conclusion of Discussion of Collett and Loudon Criticism of Popper's Argument.

Collett and Loudon exploit the effect that the finite size of source would have on the range of counters that would fire. They produce three terms that contribute to this size, one being a geometric contribution, and the other two coming from quantum uncertainties.

The geometric argument is correct and gives the correct results. This would be enough to cause partial damage to Popper's argument, as it shows that as long as the source has some finite size, the precision with which the y-position of the particle goes through the virtual slit is less than that of the actual slit. So even if, as Popper predicts, the range of y-momenta does not increase, that does not *necessarily* result to a violation of the uncertainty principle. Unfortunately they did not argue this.

Their argument also involved the other two terms. There are some minor calculation errors, like the missing cross terms and some factors of 1/4, but they

[303] Sudbery (1985: 473)

do not affect the argument. But the derivations of these two terms are falsely predicated on the assumption that the vertical length travelled during a given time interval depends on the horizontal wavelength, and thus on the horizontal momentum. This is not the case for photons. Furthermore, even for massive particles the momentum after the slit is not equal to the momentum before the slit. Their calculation produces a result that is dominated by the geometrical term, and which gives a range that decreases with the real slit decreasing. However, their conclusion cannot be evaluated given the above error.

Nevertheless, the main point that Collett and Loudon are making is that the size of the source is a factor that can be problematic for Popper's argument. This point is correct irrespective of whether the above calculations are correct or not. But, as we shall see while examining the Kim and Shih experiment that point may not be fatal for Popper's argument. The essence of the argument can remain intact provided that this blurring of the image due to the size of the source is by some technical means taken care of.

5.1.4. The Following Discussion Between Popper and Collett & Loudon

As mentioned earlier, Popper had a chance to reply[304] to the Collett and Loudon paper, with their response[305] appearing in the same issue of *Nature*. Popper starts by giving a very short summary of his argument, and then notes that

> Collett and Loudon, although not very explicitly, agree with all this provided the source is fixed. But they say that the source must not be regarded as fixed: it is subject to (Heisenberg's) uncertainty principle. By an intricate analysis (open to severest criticism) they arrive, if I understand them correctly, at the conclusion that my prediction is tenable but that the prediction of the Copenhagen interpretation leads to a clash with it. So I must not claim that the experiment is crucial; or, as the put it: "In summary, it has been

[304] Popper (1987*a*)

[305] Collett and Loudon (1987*b*)

shown that source uncertainty effects in the experiment proposed by Popper remove the distinctive increase in left-hand beam divergence with reduction in right-hand slit width that he ascribed to the Copenhagen interpretation of quantum mechanics".[306]

Popper replies that "the source *is* fixed".[307] This is because for the source we have

$$\Delta y \Delta p_y = \Delta y \Delta v_y m \approx h \qquad (PCL.1)$$

$$\text{and so} \quad \Delta y \Delta v_y \approx h/m \qquad (PCL.2)$$

For Popper, this means that by a suitable choice of m, one can make h/m arbitrarily small, and he mentions as an extreme example that by fixing the apparatus on rock, m becomes effectively equal to the mass of earth.

This reply of Popper fails here. However one could fix the source of positronium, it would still be impossible to fix the position of individual atoms and molecules, and they would still be subject to the uncertainty principle. Furthermore, even if it was possible to do that, as Collett and Loudon note, this would destroy the momentum correlation of the pair and Popper's argument would not hold. As they put it:

> One of the attractive features of the experiment as originally proposed by Popper was that the use of positronium as a source of particles guaranteed that the two particles would come off in exactly opposite directions (in the centre of mass frame of the source). Using a more massive source, not totally annihilated by the pair production, would indeed allow better localisation of the source itself, but at the cost of introducing the complication of source recoil. The more massive the source, the less the effective constraint on the initial combined momentum of the particle pair (with respect to the centre of mass of the source). In the limit of a very massive source (say the Earth), the directions in which the two particles were emitted would effectively be uncorrelated, and no conclusion at all could be drawn about the position of one from the detection of the other.[308]

[306] (*Ibid.*: 675). In fact the Collett and Loudon paper does not contain the words "In summary". Furthermore, Popper has added a correction (Popper (1987*b*)) where he point out that in the above quotation the words *no longer* are missing from the sentence "the Copenhagen interpretation *no longer* leads to a clash with it."

[307] (*Ibid.*)

[308] Collett and Loudon (1987*b*.: 675–6)

They add that the same can be seen from considering the centre-of-mass position and momentum of the particle pair just after emission. They must obey the uncertainty relations irrespective of the source.

As noted earlier, Popper seems to fail to grasp this point. This was obvious in the discussion with Redhead refer to earlier. He seems to fail to see the difficulty that however accurately one could measure the position of one particle in the pair, in order to deduce from this the position of the other particle with the same accuracy, one needs to assume that one knows the position of emission with an infinite accuracy, and that this would mean that there is a large uncertainty in the y-momentum that would lead to a break down of the argument. In other words, Popper seems to fail to see that the only way that his argument could work is if the pair was *prepared* in a y-position state but that would destroy the y-momentum correlation.

When Popper talks of fixing the source by attaching it to a rock, he seems to fail to see that what matters here is not where the source is attached to, but with what accuracy can we know the initial position. This knowledge is directly related to the y-dimension of the source. And once our knowledge becomes accurate enough for the purposes of Popper's argument by making the y-dimension of the source short enough, we'll start losing the y-momentum correlation as this is predicted by the uncertainty relations.

Returning to Popper's reply to the Collett and Loudon paper, he then tackles the "geometrical" uncertainty that they discussed. He says that this geometric extension of the source is not very important since one can increase the x-distance d between the source and the slits. Collett and Loudon reply that this is indeed implicit in their eq. ($CL.5$), but that the important point here is the one from the uncertainty in p_y at the source. As they put it: "This includes a contribution from Δp_y from the source, which does not depend on d, and it is large if Δy of the source is small".[309] Popper also reiterates the point about his experiment being not a test for quantum mechanics, but a test between two interpretations of it.

5.2. Peres on the Popper Experiment

The second reaction in the physics literature, that does not respond to the results of the Kim and Shih experiment, although it appeared after they were published, is that of A.Peres.[310] In his paper he mentions the Kim and Shih experiment but only as being underway and he does not comment on the results. So although the paper is more recent than the Kim and Shih paper it can be classified as a pre Kim and Shih paper.

Peres' main point is that the problem with the analysis given by Popper is that it "involves counterfactual hypotheses and violates Bohr's complementarity principle".[311] In a sense, Peres thinks, this means that the argument actually supports Bohr's approach. This conclusion only comes after a long attempt to "analyze the meaning of what Popper wrote and to understand his line of reasoning."[312] So his paper is less concerned with the technical analysis of the Popper experiment and more with the conceptual analysis. Peres has also published a paper[313] in which he gives some technical results which can be applied in the analysis of this experiment.

In following Peres paper it is useful to bear in mind that his reaction comes from a background of a strong instrumentalism. He expresses this clearly at the end of his paper:

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309 (*Ibid.*: 676)

310 Peres (2002)

311 (*Ibid.*: 23)

312 (*Ibid.*: 23)

313 Peres (2000)

> ... according to the Copenhagen interpretation, as Bohr apparently understood it, quantum theory is not a description of physical reality. It also does not deal with anthropomorphic notions such as knowledge or consciousness. All it does is to provide correct answers to meaningful questions about experiments done with physical systems.[314]

Peres does give his own reading of what he means by what he calls 'Bohr's understanding of the Copenhagen interpretation', and we shall examine this below, but one cannot fail to wonder how one could separate the notion of 'meaningful questions about experiments done with physical systems' from the notions of knowledge and consciousness. Even if the experiment has been performed by a machine, it has to be designed by some human (or alien for that matter). Meaningful questions about its results have to be asked by someone. And this is the case irrespective of the intention of that someone.

In any case, Peres' contention that 'quantum theory is not a description of physical reality' would immediately find him in disagreement with Popper. It is doubtful whether Popper's argument can even start with this thesis in mind. If one accepts Peres' position as one's understanding of what a physical theory is, then the discussion of whether particles have position and momenta becomes an irrelevancy, since they do not exist if only the results of the experiments do. Of course this immediately creates a tension with the last part of the above quote: What physical systems? How do we know that there are any such systems? Under this thesis all we have is the quantum theory and the results of the experiments and no description of physical reality, so where are the quantum physical systems? To reverse a point which Peres will make further on, if they were classical systems the answer would be "here in front of our very eyes", but since quantum theory deals with unobservable entities, what is left is the answer "nowhere"! And we perform those experiments, not because we want to know something about them and their physical reality, but because of our curiosity as to what would happen

[314] Peres (2002: 34)

if such and such an arrangement is made. But from such a point of view, what would be the point to think or to perform an experiment which sets out to find whether particles can have simultaneous positions and momenta if they do not even exist?

This was the very absolutist attitude towards science which Popper wanted to question. Irrespective of the soundness of his arguments, it is difficult to see how he might be wrong. We cannot accept that view by fiat of authority (irrespective of whether Peres' interpretation of Bohr is correct or not). The view which says that a physical theory does not intend to describe the physical world goes against scientific practice of centuries. If Copernicus did not aim to describe what the planets really do but only to provide the correct answers to meaningful questions about their observed positions then why abandon the Ptolemaic system, given the fact that it provided the same answers to those questions? More succinctly, why did his contemporary scientists and their successors accept his system in view of the opposition of the Catholic Church and the consequences of such opposition? This is not to argue for a realist position with regards to the reality or otherwise of theoretical terms. But simply to point out that, if scientific practice is to make any sense, Popper correctly questions the view that Peres seems to take for granted, while given Peres' view the discussion loses all its meaning and becomes redundant.

At the same time, this does not mean that Peres' discussion does not have valid points to make. On the contrary, the main flaws in Popper's argument are to be found in his paper, although in a rather idiosyncratic fashion.

5.2.1. Peres' Analysis of the Popper Experiment

Peres considers Popper's experiment as a variant of the EPR experiment, where a source S emits pairs of particles with opposite momenta given by

$$\mathbf{p}_1 + \mathbf{p}_2 = 0 \qquad (P.1)$$

He points out that although Popper considers a source of positronium, this is in practice not feasible, as the resulting photons are far too energetic for realizing his experiment, and that it would be feasible to consider, as Kim and Shih have done, photons from parametric down-conversion in a nonlinear crystal. These would have the exact properties which Popper needs, at least in a frame with a constant velocity $c(\mathbf{p}_1 + \mathbf{p}_2)/(E_1 + E_2)$, in which Eq. $(P.1)$ holds.

Next Peres makes the expected point concerning the fact that Popper made the assumption that not only do the particles have equal and opposite momenta (at least in some frame of reference), but that they also move along the same axis. He notes that that would be the case only if they were classical particles, but in the quantum domain the momenta in Eq. $(P.1)$ are incompatible with $(\mathbf{p}_1 + \mathbf{p}_2) = 0$ and that for any axis we would have the following uncertainty relations:

$$\Delta(p_1 + p_2)\Delta(q_1 + q_2) \geq \hbar, \qquad (P.2)$$

and this "sets a limit on how precisely opposite the positions of the particles will be observed".[315] On this point, Peres further notes that:

> This issue was analyzed by Collett and Loudon (1987) who came to the conclusion that Popper's experiment ... could not give conclusive results. This is just one example of how hazardous it is to use classical reasoning when we discuss quantum phenomena.[316]

Indeed for both sides of the argument: Not only did it prove hazardous for Popper but also for Collett and Loudon as argued earlier. Popper's argument having

[315] (*Ibid.*: 24)

[316] (*Ibid.*: 23)

wrong assumptions means that any counterargument exploiting them will have their negation as true assumptions, but this does not make it a sound argument. Peres seems to share this point: He argues that Collett and Loudon's heuristic use of the "uncertainty principle" is not enough, and for rigorous results one should use the Schrödinger equation. As Redhead commented on Popper, it is not very clear whether Peres followed his own advice.

Next, Peres introduces two observers Alice and Bob. The screen with the slit is placed on Alice's side. This produces a diffraction of the order of λ/a where λ is the wavelength of the photons and a is the width of the slit. Here Peres points out that although Popper thinks that the inverse relation between scattering angles and the width of the slit is due to the uncertainty relations, the $\theta \simeq \lambda/a$ is a result from classical optics and the measurable λ is related to the momentum by $\lambda = h/p$ that follows from Einstein's $E = h\nu$, and all these predates the Heisenberg realtions. But he notes that the reason that Popper does this is to use the uncertainty relation for the virtual measurement of the position of the other particle, and to claim that according to the Copenhagen interpretation the scatter in momentum should increase.

At this point Peres mentions the criticisms from philosophers of science (Sudbery (1985) Krips (1984) Redhead (1995)), that the Popper experiment has given rise to, without giving a critical assessment of these. It is interesting that he quotes Krips as saying:

> I predict therefore (in opposition to Popper) that were it possible to perform Popper's experiment, then we would find [an increased angular broadening of the beam]; QT would be vindicated and Popper refuted.[317]

The interesting point is that on this Peres does not comment at all on the prediction that QT would be vindicated if we find that the beam was indeed broadened.

[317] (*Ibid.*: 26)

Furthermore, in the next section he actually criticizes the association of such an increase in scatter with the Copenhagen interpretation as 'absurd'.[318]

Peres only briefly criticises their raising "the issue of instantaneous action at a distance in relation to the possible outcome of Popper's experiment".[319] He notes that although they all seem to assume that "quantum mechanics prohibits the instantaneous propagation of observable effects",[320] this point needs to be proved but that all proofs which he is aware of are circular in that they assume no interaction between distant subsystems. He further notes that in a Popper experiment, communication between the observers actually has to happen since the counters are coincidence counters, but that does not allow the transfer of information.

5.2.2. Peres on Semantics

Peres is more interested in the logical structure of Popper's argument, so he is willing to put aside for a while all these technical points. The first semantical point he wants to make is that he disagrees with the association of the increase in scatter because of potential knowledge with the Copenhagen interpretation, and that Popper commits some kind of "credit misappropriation".[321] The reason given for that disagreement is that whatever the Copenhagen interpretation is it should be related to Bohr, and that Bohr (1935) has criticised the EPR argument an extension of which is the Popper experiment. And Peres adds: "I find it quite remarkable that an opinion which is diametrically opposite to Bohr's be called the "Copenhagen interpretation"."[322] The paragraph ends here and Peres then moves

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318 (*Ibid.*: 27)

319 (*Ibid.*: 26)

320 (*Ibid.*: 26)

321 (*Ibid.*: 27)

322 (*Ibid.*: 27)

on to challenge Popper on his use of the expression "we have measured q_y". But what Peres does here misses the point which Popper makes.

And the point that Peres is missing here is one that Bohr did not miss in his paper. What the EPR paper does, is to say that we can have an indirect determination of either the position or the momentum of a particle by a measurement on its counterpart. What Popper does, is to say that in a similar way if that was the case in his set up then we would be able, in principle, to violate the uncertainty principle, and in order to avoid this violation which the Copenhagen interpretation forbids, an increase in scatter needs to be observed. In his reply to the EPR paper, Bohr says—in agreement with Popper in the sense that the uncertainty created is due to the physical process rather than the acquired knowledge—that "the momentum exchanged between the particle and the diaphragm" will result to us having "voluntarily cut ourselves off from any possibility of taking these reactions separately into account in predictions regarding the final results of the experiment".[323] This means that any measurement on one particle of the pair will result in the loss of either the information regarding the point of origin, in which case the position of the other particle cannot be determined, or the ability to use the conservation of momentum, in which case the momentum of the other particle cannot be determined. So Bohr replies to Einstein, not by saying that Einstein has misunderstood the theory as Peres does in effect to Popper, but by saying that the proper application of the theory to the whole of the experimental process does not allow the indirect measurement of the observable of the undisturbed particle. By implication, if Bohr had not been able to break the link between the observed and unobserved particle he would have to admit that one could obtain a "basis ... for predictions regarding the location of the other particle".[324]

[323] Bohr, (1935*b*: 146)

[324] Bohr, (1935*b*: 147)

Now when Popper comes along and makes a similar but different argument to conclude that it is possible to make predictions for the location of the other particle, it is not enough to say that the Copenhagen interpretation does not make this prediction. In the same way that Bohr showed how this prediction is disallowed by the theory, one should show how this is the case also in the Popper experiment.

The second semantical point which Peres raises is with Popper's use of the phrase "we have measured q_y". He admits that everyone occasionally makes this mistake, but still says that "[w]hen we are discussing quantum theory, we should refrain from using classical terminology".[325] He goes on to say that one should use ordinary numbers when dealing with a classical system, but self-adjoint operators in a Hilbert space when dealing with a quantum system. Furthermore, if one was consistent about that, then there would be no need for a translation of the quantum language to the classical and no need for the "unfortunate introduction of the term "uncertainty" in that context".[326] That might indeed be the case, but so long as people have the freedom to chose the language they want, and so long as their argument can be understood, their terminology can remain a stylistic preference without any important consequence for the debate. The problems that exist for Popper's argument, or the EPR argument for that matter, cannot be dealt only by saying to them 'you should not say that', but by a proper argument showing where the mistake lies. To repeat, Bohr himself did not just say that one cannot say "we have measured q_y", but that one cannot say this because the physical situation is such that it does not permit the prediction of the value of the observable for the unobserved particle.

Peres next deals with the substance of the Popper experiment. The first point

[325] (*op. cit.*: 27)

[326] (*Ibid.*)

which he examines is whether the particles in a pair are aligned perfectly. He notes that eq. (P.2), which talks of "standard deviations of the results of a large number of measurements performed on identically prepared systems"[327] is the proper way of speaking of uncertainty, as the uncertainty principle is ill-defined and should only be used heuristically. He then interprets eq. (P.2), as saying that

> if an ensemble of pairs of particles is prepared in such a way that $(p_1 + p_2)$ is sharp, then the positions of the points halfway between the particles are very broadly distributed.[328]

In a sense this describes exactly the conclusion by Redhead. Peres adds that although eq. (P.2) talks about the momenta and the positions of the particles, it does not talk of their angular alignment. In his (2000) Peres has shown that "the allowed deviation from perfect alignment is of the order of $\Delta|p_1 + p_2|/|p_1 - p_2|$, which is much too small to be of any consequence in the present discussion".[329] Indeed this term is almost zero and in any case very small compared to the momenta involved. So having put aside that difficulty for Popper, Peres talks of a different one:

> Popper wants the particles to travel in straight lines over large distances, and he also wants them to diffract when they pass through narrow slits. These appear to be two contradictory demands, as discussed by Collett and Loudon (1987).[330]

But here Peres misses the point again. What Collett and Loudon show is not that there is an incompatibility between travelling in straight lines and diffracting through narrow slits, but that the virtual slit is not as narrow as Popper would like it to be. After all, particles that go through an actual slit do diffract. So irrespective of whether the particles do travel in straight lines or not, they will diffract when passing through an actual slit.

[327] (*Ibid.*: 28)

[328] (*Ibid.*)
[329] (*Ibid.*)

[330] (*Ibid.*)

In any case Peres is willing to grant this point. He is also willing to grant an instantaneous exchange of information between the observers, just to show that even then Popper's argument has a logical flaw. He agrees that, given those assumptions, a detection of a particle by Alice allows the certain prediction of a detection of a particle by Bob, had Bob placed a slit in the corresponding region. But this, he says, does not mean that a virtual slit is created by Bob's knowledge, such that particles passing through it would have to diffract. This Peres says is because if the slit is not actual but virtual, then Bob's knowledge is counterfactual. And the problem with such counterfactual knowledge is that there is plenty more of such knowledge that Bob could have had. For example, that halfway between the slit and the source the particle passes through a slit of half the size, and therefore it should scatter even more, etc. This he finds absurd, since there would be infinitely many such slits, all of which would result in a different angle. He thus agrees with Popper that the effect of increased scattering will not happen. But he wants to examine further Popper's contention that this would mean that the Copenhagen interpretation is wrong.

Peres here notes that various people have given different versions of it and that he is going to carry out his examination based on his version of the Copenhagen interpretation which he has based on Bohr's writings.

Before examining this it should be noted that the main point which Popper wants to make here is whether or not a violation of the uncertainty principle can be achieved. If this can be done, then the Copenhagen interpretation would be wrong irrespective of which version of the interpretation Peres has in mind.

Furthermore, there is a problem with the counterfactual knowledge that Peres alleges that Bob can have. Peres is making a mistake here. It is true to say that the event "A particle is detected behind Alice's slit" guarantees the counterfactual "if an identical slit had been placed by Bob in a symmetric position, then Bob would definitely be able to detect the other particle of that pair there". But that counterfactual does not also guarantee that "if he had placed a slit of width $a/2$ at a position whose distance from the source is one half of the distance of Alice's slit, then he would have been able to detect his particle within his slit with certainty".[331] This is because one cannot claim that just because Alice has detected her particle after it has passed through her slit, she also knows that she would have also detected the particle if she had placed a slit at half the distance through a slit of width $a/2$. And it is only through this last knowledge of Alice's that Bob could use the phase locking conditions to deduce the latter counterfactual. In any case this is what follows from the conclusion of his other article:

> When a particle decays into two fragments, the wavefunctions of the latter are spherical shells with expanding radii. In spite of this spherical configuration, the two particles can be detected only in opposite directions.[332]

Since Peres is in the context of quantum mechanics here, saying that "the two particles can be detected only in opposite directions" does not mean that if the one was detected at a distance d we know that it would have also been detected at $d/2$. So Peres is the one who uses erroneous argumentation here.

On the other hand there is no need to even consider the counterfactuals that Peres considers. All that is needed is to make the assumptions that Peres is willing to grant to Popper in order to see that there is nothing wrong with Popper's reasoning. That is, to assume that if Alice has detected a particle through her slit then "Bob would definitely be able to detect the other particle of that pair there".[333] In that case, upon repetition of the experiment, the standard deviation of y would be $\Delta y \leq a$. At the same time, the standard deviation of p_y, as

331 (*Ibid.*: 28–9)

332 Peres (2000: 991)

333 (*Ibid.*: 28)

Peres implies, would be less than the one that Alice would observe. So if the two standard deviations for Alice's particles are only due to the theoretical limits imposed by the uncertainty principle so that for her particles we would have $\Delta p_y \Delta y = \hbar$, then for Bob's particles we would have $\Delta p_y \Delta y < \hbar$. In that case, given that Δp_y and Δy are properly understood as standard deviations and not as mystical uncertainties, we would have a violation of the uncertainty principle, and since the Copenhagen interpretation supposes that the principle applies to all physical systems that are described by the formalism of the theory, it should be wrong.

Nothing in the above reasoning depends on any counterfactuals except the ones that Peres is willing to yield. The reason why Popper says that the Copenhagen interpretation implies an increase in scatter is in order to keep the product of standard deviations equal to $\hbar$. The problem with Popper's reasoning is not that it depends on the counterfactuals which Peres alleges, but that it is wrong to assume that the particles go through Bob's virtual slit just because their counterparts went through Alice's slits, due to the fact that the source is not a point source. And this is the same reason which Bohr argued for in the case of the EPR experiment. But what Bohr did not do is to say that the EPR argument is based on counterfactuals and for this reason it has to be discarded.

5.2.3. Peres' Version of the Copenhagen Interpretation

Peres states that theorists use the formalism of quantum mechanics for two reasons: First, to calculate relationships between physical constants, and, second, to make probabilistic predictions for the outcomes of tests involving physical systems prepared in specified ways. He hopes that this statement is uncontroversial and thinks that the remaining question is "whether there is more than that to say about quantum mechanics".[334] Furthermore, the description of the equip-

[334] (*Ibid.*: 29)

ment used to prepare and measure those systems is always given in classical terms. Peres quotes Bohr from his (1949) article where Bohr emphasises this point and he notes that Bohr talks of "classical terms" and "unambiguous language" but that he does not ever say whether "there are in nature classical systems and quantum systems".[335] Whether the language that is used is classical or quantum is based on our assessment: "It is according to our assessment of the physical circumstances that we decide whether the *q*-language or the *c*-language is appropriate".[336] And this is because physics is not an exact science but an approximate one. And to that he adds:

> Unfortunately, Bohr was misunderstood by some (perhaps most) physicists who were unable to make the distinction between language and substance, and he was also misunderstood by philosophers who disliked his positivism.[337]

It is not very clear what is the misunderstanding that some of the physicists and the philosophers have made here concerning language and substance. At least Peres does not explain what it is. It is certain that Popper does not like positivism. But it is a big leap to say that therefore Popper would disagree with the insistence of Bohr on unambiguous language and classical terms in the description of the apparatus of the experiments. In any case he uses classical terms to describe his, and he even insists on to the Δp_y's and Δy's as scatters. And nothing in Popper's argument hinges on which language is going to be used.

There is a theme in Peres article which starts emerging here. The article seems to suggest that if we take what Peres calls "Bohr's positivism" on board then Popper's argument would pose no problem to talk of. Irrespective of whether one should consider Bohr as a positivist or not, in that case Peres is missing the

[335] (*Ibid.*: 30)

[336] (*Ibid*)

[337] (*Ibid.*)

point: The whole argument is to decide whether we should accept positivism or realism. Or rather, in Popper's fashion, whether we have reason to refute positivism (since we—that is Popper—are realists). Of course if one includes positivism in the premises of the argument then positivism follows trivially as the conclusion. But that is not a move that one should be allowed to make, in the discussion of an experiment that tries to provide a refutation for positivism.

The first three sentences in the next paragraph of Peres' text is also interesting, especially given that Peres insists on following Bohr closely:

> It is remarkable that Bohr never considered the measuring process as a dynamical interaction between an apparatus and the system under observation. Measurement had to be understood as a primitive notion. Bohr thereby eluded questions which caused considerable controversy among other authors (Wheeler and Zurek, 1983).[338]

It would be remarkable indeed if Bohr had done so. But one can of course contrast these with the following statements of Bohr in his reply to the EPR argument, from where a rather different picture emerges.

> Let us begin with the simple case of a particle passing through a slit in a diaphragm, which may form part of some more or less complicated experimental arrangement. Even if the momentum of this particle is completely known before it impinges on the diaphragm, the diffraction by the slit of the wave giving the symbolic representation of its state will imply an uncertainty in the momentum of the particle, after it has passed the diaphragm, which is greater the narrower the slit.[339]

And half a page further:

> Then the momentum exchanged between the particle and the diaphragm will, together with the reaction of the particle on the other bodies, pass into this common support, and we have thus voluntarily cut ourselves off from any possibility of taking these reactions separately into account in predictions regarding the final result of the experiment,—say the position of the spot produced by the particle on the photographic plate.[340]

Although these passages are supposed to show "[t]he impossibility of a closer analysis of the reactions between the particle an the measuring instrument",[341]

[338] (*Ibid.*)
[339] Bohr (1935*b*: 146)
[340] (*Ibid.*)
[341] (*Ibid.*)

it is hard to understand them as anything but a description of "a dynamical interaction between an apparatus and the system under observation", as Peres tells us that Bohr never considers the measurement to be. Bohr indeed stresses that it is impossible to analyse the dynamical process further than a specific limit, but that the analysis cannot pass that limit does not necessarily mean that it is not a dynamical process whose analysis cannot move further than that limit. An interpretation of what Bohr says can be that "[m]easurement had to be understood as a primitive notion" as Peres says in the above passage, but the above words of Bohr do not force upon us that interpretation.

Peres then goes on to emphasise how measurement, being a primitive notion, had helped Bohr to avoid answering controversial questions, and how Bohr's and von Neumann's approaches can be reconciled by his approach of considering a dual description for the measuring apparatus. But he does not show the relevance of this approach to Popper's argument. Furthermore, avoiding answering the question does not mean that the problem is solved.

Next Peres says that the second ingredient of Bohr's approach is the principle of complementarity, "which asserts that when some types of predictions are possible, others are not, because they are related to mutually incompatible experiments".[342] He mentions that Bohr showed in his response to EPR how the choice of measurements made on one system affect the predictions that can be made about the other, and uses that for the Popper experiment:

> In Popper's experiment, Bob can predict what would have happened if he had placed slits of various sizes at various positions, or no slit at all. However, all these possible setups are mutually incompatible. In particular, if Bob puts in no slit at all, the result he obtains is not the one he would have obtained if he had put in a slit. Counterfactual experiments need not have consistent results (Peres, 1978).[343]

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[342] (*op. cit.*: 31)

[343] (*Ibid.*)

The problem here for Peres is that what he says about the Popper experiment is not analogous to what Bohr does with the EPR situation. What he says amounts to Bob having the same number of particles going through a real slit and the virtual slit and then different things happening. Popper agrees with that fully. But Bohr's argument is analogous to denying that the same number of particles would go through the two slits. So Peres' argument has the correct conclusion but is based on the wrong assumptions.

5.2.4. Peres' Concluding Remarks

Peres concludes his article by reporting what he calls "the result of a rigorous analysis of Popper's experimental setup, where only Schrödinger's equation is used, without invoking any controversial interpretation".[344] It is surprising that Peres goes on to give the analysis of a *different* experiment by Strekalov et al.[345] Strekalov et al. report that this is an experiment which "is very close to the original *gedankenexperiment* of Einstein, Podolsky, and Rosen"[346] At the same time Peres is aware of the Kim and Shih experiment which is intended to be a realisation of the Popper experiment, so he could have given his analysis of that experiment instead of the one by Strekalov et al.

Concerning the Strekalov et al. experiment, Peres comments that "The irony of the answer is that Bob does observe a diffraction broadening, as if he had a virtual slit!"[347] A similar effect is observed in the Kim and Shih experiment. In fact Strekalov et al. do report the diffraction pattern they observe from a single slit as well,[348] but Peres seems to have ignored that part of their report.

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[344] (*Ibid.*: 31)

[345] Strekalov, Sergienko, Klyshko and Shih (1995)

[346] (*Ibid.*: 3603)

[347] (*Op. cit.*: 31)

[348] (*Op. cit.*: 3601)

Peres then proceeds to give a brief presentation of the derivation for the transition probabilities for the process:

$$|\psi_0\rangle \rightarrow |\psi_t\rangle = U_t|\psi_0\rangle \qquad (P.3)$$

where $|\psi_0\rangle$ and $|\psi_t\rangle$ are Hilbert-space vectors with coordinate-space representation which is localized in the source and in the detectors that were excited, respectively. The $|\psi_0\rangle$ vector has to satisfy eq. $(P.1)$, in order to represent a pair of particles produced with the phase matching conditions discussed, and $|\psi_t\rangle$ has to be of the form

$$|\psi_d\rangle = |\psi_1\rangle \otimes |\psi_2\rangle. \qquad (P.5)$$

that "is a tensor product of two vectors, whose coordinate-space representations are well separated, since they are localized in the two detectors".[349] This means that $|\psi_t\rangle$ represents the state of the two detected particles at the detectors. Finally, Peres notes that one needs to compute the probability given by the Born rule for this evolution, and that this is illustrated in a figure that represents the Strekalov et al. experiment. The figure shows Alice's detectors acting as a source of light which passes through Alice's double slit, then through the source and finally creates an interference pattern at Bob's detectors. He explains this thus:

> For example, if we record all the detections on Bob's side that are in coincidence with one particular detector of Alice, then Bob will observe an ordinary double-slit interference pattern, generated by a "virtual" double-slit, that actually is Alice's real pair of slits.[350]

Peres also notes the necessity, pointed out by Hong and Mandel,[351] "that the region of the nonlinear crystal from where the rays emerge be very broad and the emergence point be undetermined", as well as that "each one of the two photons

[349] (*Op. cit.*: 32)

[350] (*Ibid.*: 33)

[351] Hong and Mandel (1985)

that pass through both slits must also originate in *both* regions of the source".[352] He then remarks that a similar analysis applies for the Popper experiment, but that a figure similar to the one he has presented for the two slit case is difficult to draw.

Peres finally gives the conclusion which was quoted at the beginning of this section. Unfortunately there is no explanation of why this conclusion follows from the fact that some other experiment has a result that is different from what Popper might have expected in his experiment. There is no explanation as to why an interference pattern created by Alice's double slit would be contrary to what Popper predicted or in accordance with the Copenhagen interpretation.

But even the assertion that all that the interpretation does is 'to provide correct answers to meaningful questions about experiments done with physical systems' is questionable in the context of this experiment because it was not the interpretation that provided the correct answers here, but the formalism itself.

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[352] (*Op. cit.*: 33)

Chapter 6: Background to Non-Linear Quantum Optics

The Kim-Shih experiment is based on the production of entangled photon pairs. Production of pairs of particles that have this property is commonly achieved using the process of Spontaneous Parametric Down-Conversion (SPDC) that can act as a simple and efficient source of entangled photon pairs. The uses for these pairs are widespread, and range from the examination of quantum-mechanical foundations, to applications in optical measurements, spectroscopy, imaging, and quantum information. The background theory of SPDC can be found, for example, in the books of Baldwin,[353] Klyshko,[354] and Yarin.[355] An article that deals exclusively with the theory of SPDC is that of Hong and Mandel.[356] The interference of independent laser beams has been reported as early as 1967,[357] but the aspect imaging by the use SPDC has been reported more recently by Strekalov et. al.[358] and Pittman et. al.[359] Furthermore a recent popular science book that deals with the notion of entanglement is that of Aczel.[360] This chapter examines briefly the notion of entanglement and gives a summary of the theory of the SPDC processes.

The notion of Entanglement is intricately connected with the notion of interference described by Feynman in his famous *Lectures in Physics*, as containing "the only mystery"[361] of quantum mechanics. Even if one does not take such an

[353] Baldwin (1969)

[354] Klyshko (1988)

[355] Baldwin (1989)

[356] Hong and Mandel (1985)

[357] Pfleegor and Mandel (1967)

[358] Strekalov et. al. (1995)

[359] Pittman et. al. (1995)

[360] Aczel (2003)

[361] Feynman, Leighton, and Sands (1965)

extreme view, nonclassical interference effects can give key insights into the quantum nature of particle interactions, and the explanation of such effects is crucial for the understanding of quantum dynamics.

Entanglement is a property that can be exhibited by a system with two or more subsystems. For such a system consisting of two subsystems, the particles 1 and 2, the two subsystems are said to be entangled if a measurement of the state of the one determines the state of the other. For example, if particle 1 can be in one of two states, say $|\oplus\rangle$ or $|\ominus\rangle$, and particle 2 can be in one of the two states, $|\otimes\rangle$ and $|\odot\rangle$, then the two subsystems are entangled if the state of the total system is such that any measurement will necessarily yield either $|\oplus\rangle|\otimes\rangle$ or $|\ominus\rangle|\odot\rangle$. That is, the state of the total system can be the superposition of $|\oplus\rangle|\otimes\rangle$ and $|\ominus\rangle|\odot\rangle$, namely

$$|\oplus\rangle|\otimes\rangle + |\ominus\rangle|\odot\rangle \tag{6.1}$$

which is in an entangled state. This means that while the states $|\oplus\rangle|\otimes\rangle$ and $|\ominus\rangle|\odot\rangle$ ascribe definite properties to the two particles 1 and 2, the entangled state does not, since it constitutes a superposition. At the same time if the system is in the above state it is certain that if a measurement on particle 1 yields the result $|\oplus\rangle$ then a measurement on particle 2 will yield $|\otimes\rangle$, and if a measurement on particle 1 yields the result $|\ominus\rangle$ then a measurement on particle 2 will yield $|\odot\rangle$.

More specifically, photon pairs from type II SPDC (which is used in the Kim-Shih experiment) are generated in a quantum state that can be entangled in frequency, wave vector, and polarization. The state function of the photon pair generated in SPDC is completely characterized by three functions: The spectral profile of the pump, the longitudinal distribution of nonlinear susceptibility, and the dispersion in the generation medium. In principle, one could arbitrarily weigh the spatiotemporal distribution of the two entangled photons (the signal and idler modes) by a choice of these three functions.

6.1. Quantum interference

Interference effects can be observed, for example, in a standard (Mach-Zehnder) optical interferometer. For such a device, both classical and quantum mechanical pictures give the same and correct answer for interference effects and there is no distinction between the two predictions in the case of a single photon. A single photon interference effect is therefore often referred to as classical interference. In order to observe a truly quantum mechanical interference effect for which a classical picture fails, a two (or more) photon interferometer is needed.

6.1.1. One photon interference

In an optical interferometer an incident wave A_a is split into the two partial waves A_1 and A_2 by the first (input) beam splitter which splits the beam in two 50%-50% partial waves. The indices 1 and 2 indicate the two different paths of the interferometer, and the A_1 partial wave follows the path which the incident wave A_a would have followed should there have been no beam splitter, while the A_2 partial wave follows the path that the incident wave A_a would have followed should there have been a reflective mirror in the place of the beam splitter. The partial waves, while travelling through their paths, undergo independent phase shifts which can be represented by a single phase-shift ϕ, and then they are recombined by a second (output) 50%-50% beam splitter. Finally, once they are recombined, they are detected by two detectors D_A and D_B. Classically this can be thought of as an interference between the partial waves propagating in the two arms.

Quantum mechanically, though, a different view emerges: First, the input state $|\psi_{\text{in}}\rangle$ represents an incoming beam of particles which is incident on the beam splitter and is leaving the splitter as if coming from two orthogonal directions:

$$|\psi_{\text{in}}\rangle = |1\rangle_a |0\rangle_b \tag{6.2}$$

This ket vector is a product vector of two incident beams, one coming from the direction of the incident beam ($|1\rangle_a$), and the other from the direction that the reflected beam would be coming from ($|0\rangle_b$).

The input beam splitter is expressed by the unitary operator $\widehat{U}_{BS} = \widehat{U}_1$ which is represented by the matrix:

$$\hat{U}_{BS} = \frac{1}{\sqrt{2}}\begin{pmatrix} 1 & -1 \\ 1 & 1 \end{pmatrix} \tag{6.3}$$

The unitary matrix is represented for a suitable representation of the basis of the two orthogonal ket vectors $|1\rangle_a\,|0\rangle_b$ and $|0\rangle_a\,|1\rangle_b$, i.e., for:

$$\begin{pmatrix} 1 \\ 0 \end{pmatrix} = |1\rangle_a\,|0\rangle_b \qquad \text{and} \qquad \begin{pmatrix} 0 \\ 1 \end{pmatrix} = |0\rangle_a\,|1\rangle_b \tag{6.4}$$

The beam splitter creates a linear superposition state for each individual input photon. This is represented mathematically by the action of the operator $\widehat{U}_{BS}$ on the input state:

$$\begin{aligned} |\psi_{\text{in}}\rangle \rightarrow |\psi_1\rangle =& \widehat{U}_{BS}|\psi_{\text{in}}\rangle \\ =& \frac{1}{\sqrt{2}}\begin{pmatrix} 1 & -1 \\ 1 & 1 \end{pmatrix}|1\rangle_a\,|0\rangle_b = \frac{1}{\sqrt{2}}\begin{pmatrix} 1 & -1 \\ 1 & 1 \end{pmatrix}\begin{pmatrix} 1 \\ 0 \end{pmatrix} \\ =& \frac{1}{\sqrt{2}}\begin{pmatrix} 1 \\ 1 \end{pmatrix} = \frac{1}{\sqrt{2}}\left[\begin{pmatrix} 1 \\ 0 \end{pmatrix} + \begin{pmatrix} 0 \\ 1 \end{pmatrix}\right] \\ =& \frac{1}{\sqrt{2}}\left\{|1\rangle_a\,|0\rangle_b + |0\rangle_a\,|1\rangle_b\right\} \end{aligned} \tag{6.5}$$

Then, the probability amplitudes of the two states acquire a relative phase shift ϕ. This is expressed by the unitary operator $\hat{U}_{PS}$ which is represented by the matrix:

$$\widehat{U}_{PS} = \begin{pmatrix} 1 & 0 \\ 0 & e^{i\phi} \end{pmatrix} \tag{6.6}$$

This gives the superposition:

$$
\begin{aligned}
|\psi_1\rangle \to |\psi_2\rangle =& \widehat{U}_{PS}|\psi_1\rangle \\
=& \begin{pmatrix} 1 & 0 \\ 0 & e^{i\phi} \end{pmatrix} \frac{1}{\sqrt{2}}[|1\rangle_a |0\rangle_b + |0\rangle_a |1\rangle_b] \\
=& \frac{1}{\sqrt{2}} \begin{pmatrix} 1 & 0 \\ 0 & e^{i\phi} \end{pmatrix} \begin{pmatrix} 1 \\ 1 \end{pmatrix} = \frac{1}{\sqrt{2}} \begin{pmatrix} 1 \\ e^{i\phi} \end{pmatrix} \\
=& \frac{1}{\sqrt{2}} \left[\begin{pmatrix} 1 \\ 0 \end{pmatrix} + \begin{pmatrix} 0 \\ e^{i\phi} \end{pmatrix} \right] \\
=& \frac{1}{\sqrt{2}} \left[|1\rangle_a |0\rangle_b + e^{i\phi} |0\rangle_a |1\rangle_b \right]
\end{aligned}
\tag{6.7}
$$

Finally, the process that beam undergoes while passing through the second output beam splitter, is expressed by the unitary operator $\widehat{U}_2 = \widehat{U}_1^+ = \widehat{U}_{BS}^+$ which is represented by the matrix

$$
\hat{U}_{BS}^+ = \frac{1}{\sqrt{2}} \begin{pmatrix} 1 & 1 \\ -1 & 1 \end{pmatrix}. \tag{6.8}
$$

This matrix mixes the two incident states to produce the final output state:

$$
\begin{aligned}
|\psi_2\rangle \to |\psi_{\text{out}}\rangle =& \widehat{U}_{BS}^+|\psi_2\rangle \\
=& \frac{1}{\sqrt{2}} \begin{pmatrix} 1 & 1 \\ -1 & 1 \end{pmatrix} \frac{1}{\sqrt{2}} \left[|1\rangle_a |0\rangle_b + e^{i\phi} |0\rangle_a |1\rangle_b \right] = \frac{1}{2} \begin{pmatrix} 1 & 1 \\ -1 & 1 \end{pmatrix} \begin{pmatrix} 1 \\ e^{i\phi} \end{pmatrix} \\
=& \frac{1}{2} \begin{pmatrix} 1 + e^{i\phi} \\ -(1 - e^{i\phi}) \end{pmatrix} \\
=& \frac{1}{2} \left[\left(1 + e^{i\phi}\right) |1\rangle_a |0\rangle_b - \left(1 - e^{i\phi}\right) |0\rangle_a |1\rangle_b \right]
\end{aligned}
\tag{6.9}
$$

To summarise, the whole process that the incident beam is subjected to by the interferometer are given by:

$$
\begin{aligned}
|\psi_{\text{in}}\rangle = |1\rangle_a |0\rangle_b \quad &\xrightarrow[\widehat{U}_1]{} \quad \frac{1}{\sqrt{2}}\{|1\rangle_a |0\rangle_b + |0\rangle_a |1\rangle_b\} \\
&\xrightarrow[\phi]{} \quad \frac{1}{\sqrt{2}}\{|1\rangle_a |0\rangle_b + e^{i\phi}|0\rangle_a |1\rangle_b\} \\
&\xrightarrow[\widehat{U}_2]{} \quad \frac{1}{2}\Big\{ \left(1+e^{i\phi}\right) |1\rangle_a |0\rangle_b - \left(1-e^{i\phi}\right) |0\rangle_a |1\rangle_b \Big\} = |\psi_{\text{out}}\rangle
\end{aligned}
\tag{6.10}
$$

Finally, the resulting wavefunction, together with its dual

$$
\langle\psi_{\text{out}}| = \Big(|\psi_{\text{out}}\rangle\Big)^{\dagger} = \frac{1}{2}\Big\{ \left(1+e^{i\phi}\right) {}_b\langle 0|\, {}_a\langle 1| - \left(1-e^{i\phi}\right) {}_b\langle 1|\, {}_a\langle 0| \Big\} \tag{6.11}
$$

and the following adjoint operators

$$
\hat{n}_a \equiv \sum_n n|n\rangle_{a\,a}\langle n| \otimes \hat{I}_b \qquad \hat{n}_b \equiv \hat{I}_b \otimes \sum_n n|n\rangle_{b\,b}\langle n| \tag{6.12}
$$

can be used for the detection probability at the two detectors: For the detector a the probability is given by

$$
\begin{aligned}
P_a &= \langle\psi_{\text{out}}|\, \hat{n}_a |\psi_{\text{out}}\rangle = \Big|{}_a\langle n|\, {}_b\langle 0|\psi_{\text{out}}\rangle\Big|^2 = \frac{1}{4}\Big|1+e^{i\phi}\Big|^2 \\
&= \frac{1}{2}(1+\cos\phi),
\end{aligned}
\tag{6.13}
$$

and for the detector b it is given by

$$
\begin{aligned}
P_b &= \langle\psi_{\text{out}}|\, \hat{n}_b |\psi_{\text{out}}\rangle = \Big|{}_a\langle 0|\, {}_b\langle n|\psi_{\text{out}}\rangle\Big|^2 = \frac{1}{4}\Big|1-e^{i\phi}\Big|^2 \\
&= \frac{1}{2}(1-\cos\phi),
\end{aligned}
\tag{6.14}
$$

So the probability of detecting a photon at the detectors D_A and D_B oscillates according to $P_A = (1/2)(1+\cos\phi)$ and $P_B = (1/2)(1-\cos\phi)$ respectively. This means that what interfere with each other are not the partial waves of the incident wave, like in the classical case, but the probability amplitudes of a linear superposition states of $|1\rangle_a |0\rangle_b$ and $|0\rangle_a |1\rangle_b$. The origin of interference seems to be the lack of information concerning the path (upper or lower arm) that the photon takes before detection. In order to observe the interference effect, there should be two indistinguishable paths for the photons to take, in the sense that there should be no way for the experimenter to determine which one did the photon travel along.

6.1.2. Two photon interference

In a two-photon interferometer utilizing parametric down-converters an intrinsic quantum effect shows up. This can be seen in an experiment in which two similar second-order nonlinear crystals NL1 and NL2, both functioning as parametric down-converters, are optically pumped by light of frequency ω_0 derived from the same laser beam. In this case, parametric down-conversion can occur at NL1 with the simultaneous emission of a pair of signal s_1 and idler i_1 photons, or down-conversion can occur at NL2 with the simultaneous emission of a pair of s_2 and i_2 photons (the probability of simultaneous emissions from both NL1 and NL2 may be regarded as negligibly small). In the parametric down-conversion process, the energy and momentum conservation laws hold, i.e. $\omega_0 = \omega_s + \omega_i$ and $k_0 = k_s + k_i$. By allowing the signals s_1 and s_2 to come together and mix at the $50\% : 50\%$ beam splitter BS_A, in such a way that the combined field falls on detector D_A, and the idlers i_1, i_2 to come together at BS_B and the combined idler field to fall on detector D_B, the counting rates of D_A and D_B can be examined for interference.

The experimental results show that although the single photon counting rates R_A and R_B of detectors D_A and D_B do not show any interference, their combined rate does: When the single photon counting rates R_A and R_B of detectors D_A and D_B are plotted against the optical path difference between the pump beams reaching NL1 and NL2 created by the displacement of the pump beamsplitter

BS_p, both superposed light intensities are constant and independent of the path difference, so that s_1 and s_2 do not interfere, and neither do i_1 and i_2. But when the two-photon coincidence counting rate R_{AB} for detectors D_A and D_B is plotted together against the optical path difference, then an unmistakable interference pattern of the expected periodicity is observed. This happens even though the pairs s_1-s_2 and i_1-i_2 are mutually incoherent.

For a simplified theory explaining these results, the down-converted fields can be treated as monochromatic. The interaction Hamiltonian for a parametric down-conversion process for this case is $\mathcal{H} = \hbar g(\epsilon_p^* \hat{a}_s \hat{a}_i + \epsilon_p \hat{a}_s^+ \hat{a}_i^+)$. For $|\psi_1\rangle$ and $|\psi_2\rangle$ being the quantum states of the down-converted fields in the interaction picture produced by nonlinear crystals NL1 and NL2, we have

$$|\psi_1\rangle = M_1|0\rangle_{s_1}|0\rangle_{i_1} + \eta_1\epsilon_{p_1}F_1|1\rangle_{s_1}|1\rangle_{i_1} \tag{6.15}$$

$$|\psi_2\rangle = M_2|0\rangle_{s_2}|0\rangle_{i_2} + \eta_2\epsilon_{p_2}F_2|1\rangle_{s_2}|1\rangle_{i_2} \tag{6.16}$$

Here ϵ_{p_1} and ϵ_{p_2} are the complex classical pump amplitudes at NL1, NL2. η_1, η_2, are dimensionless factors such that $|\eta_1|^2$ and $|\eta_2|^2$ give the down-conversion efficiencies, i.e., the fraction of incident pump photons that convert to signal and idler photons. F_1, F_2 are constants characterizing the spectral filtering functions. M_1, M_2 are complex coefficients which are very close to unity in practice.

Terms for including more than two photons per channel such as $|2\rangle_s|2\rangle_i$, can be neglected. If $\hat{E}_A^{(+)}$, $\hat{E}_B^{(+)}$ are the positive frequency parts of the total electric fields at detectors D_A, D_B, then

$$\hat{E}_A^{(+)} = K(\hat{a}_{s_1} + i\hat{a}_{s_2})e^{-i\omega_s t} \tag{6.17}$$

$$\hat{E}_A^{(+)} = K(\hat{a}_{i_1} + i\hat{a}_{i_2})e^{-i\omega_i t} \tag{6.18}$$

where K is a constant determined by the mode volume and photon energy. A photon in the incident light is converted into a photoelectron in the detector, in which the process of stimulated absorption takes place. The Fermi's golden rule transition rate is formulated as

$$W = \frac{2\pi}{\hbar^2} \sum_{R_f} \sum_{f} |\langle R_f|_a \langle f|\hat{D}\hat{E}|R_i\rangle|i\rangle_a|^2 \tag{6.19}$$

where $|i\rangle_a = |g\rangle|g\rangle \cdots |g\rangle$ is the initial atomic state in which all atomic electrons are in their (bound) ground states; $|R_i\rangle$ is the initial field state, $\hat{D}$ is the electric dipole operator proportional to the collective raising operator $\hat{J}_+$, and $|f\rangle_a$ and $|R_f\rangle$ are the final atomic and field states. Equation (6.19) can be factorized into the field and atomic coordinates to obtain an expression for the single photon count rate at the detector D_A, which is proportional to

$$R_A = \langle\psi_1|\langle\psi_2|\hat{E}_A^{(-)}\hat{E}_A^{(+)}|\psi_2\rangle|\psi_1\rangle \tag{6.20}$$

from which it can be shown that

$$R_A = |K|^2 \left[|\eta_1 \epsilon_{p_1} F_1|^2 + |\eta_2 \epsilon_{p_2} F_2|^2 \right] \tag{6.21}$$

This expression lacks an interference term, and this means that the signal photon count rate exhibits no interference, even though the state vector Eq. (6.15) or Eq. (6.16) carries the pump phase information.

Similarly, the Fermi's golden rule transition rate for the two-photon coincidence count rate is given by

$$W = \left(\frac{2\pi}{\hbar}\right)^2 \sum_{R_f} \sum_{f_1} \sum_{f_2} |\langle R_f|\,_a\langle f_1|\,_a\langle f_1|\hat{D}_B\hat{E}_B^{(+)}(t_2)\hat{D}_A\hat{E}_A^{(+)}|R_i\rangle\,|i_1\rangle_a\,|i_2\rangle_a|^2$$
$$\left(\frac{2\pi}{\hbar}\right)^2 |\langle d_A\rangle|^2 |\langle d_B\rangle|^2 \langle R_i|\hat{E}_A^{(-)}(t_1)\hat{E}_B^{(-)}(t_2)\hat{E}_B^{(+)}(t_2)\hat{E}_A^{(+)}(t_1)|R_i\rangle \tag{6.22}$$

where $|R'\rangle$ and $|i\rangle_a$ are the intermediate field and atomic states between the first and second photon absorption, and $\sum_{R_f} |R_f\rangle\langle R_f| = \hat{I}$, $\sum_{f_1} |f_1\rangle\langle f_1| = \hat{I}$ and

$\sum_{f_2} |f_2\rangle\langle f_2| = \hat{I}$ are used. Thus the two-photon coincidence count rate at the detectors D_A and D_B is proportional to the expression

$$R_{AB} = \langle\psi_1|\langle\psi_2|\hat{E}_A^{(-)}(t_1)\hat{E}_B^{(-)}(t_2)\hat{E}_B^{(+)}(t_2)\hat{E}_A^{(+)}(t_1)|\psi_2\rangle|\psi_1\rangle \tag{6.23}$$

from which it can be shown that

$$\begin{aligned} R_{AB} = |K|^4 \Big\{ |\eta_1\epsilon_{p_1}F_1|^2 + |\eta_2\epsilon_{p_2}F_2|^2 - 2|\eta_1\epsilon_{p_1}F_1||\eta_2\epsilon_{p_2}F_2| \cos[arg(\epsilon_{p_1}) \\ - arg(\epsilon_{p_1}) + \text{constant}] \Big\}. \end{aligned} \tag{6.24}$$

The last term in this expression is an interference term and therefore the two-photon coincidence count rate exhibits interference, even though the single photon count rate does not.

The standard explanation of why the coincidence rate R_{AB} exhibits interference, whereas the single photon rates R_A and R_B do not invokes the indistinguishability of the two photon paths. If it is impossible to determine whether the photons originate in NL1 or in NL2, then the corresponding probability amplitudes for the two paths have to be added in order to arrive at the detection probability, and then interference results. On the other hand, if there is a possibility, even in principle, of determining the source of the photons, then all interference is wiped out. In the two-photon coincidence measurements there is indeed no way to determine the source of each photon pair without introducing disturbances.

By looking only in the signal photons and removing the beam splitter BS_B, detections by D_A which are accompanied by detections by D_B occur only when both photons originate in NL1. By contrast, a photon detection by D_A which is not accompanied by a detection by D_B must be attributed to an s_2 photon emitted by NL2. It follows that the source of each detected signal photon can be identified in this way, and therefore all indistinguishability of the sources is lost, and so is all interference. It is not necessary, however, for the auxiliary measurement to be actually carried out. The possibility that it can be performed is sufficient to suppress the interference of the signal photons. In this sense, removing the beamsplitter BS_B is also auxiliary. As far as the single photon count rate is concerned, there is no interference effect. Whether the BS_B is in place or not, and the idler photon is detected or not, does not affect the experimental result. A similar argument shows that the idlers do not interfere either.

6.2. Parametric Processes

Parametric processes are light-scattering processes that can be observed with the help of lasers. There are a number of these processes and they have a variety of applications in non-linear optics. A brief description of the parametric scattering process is the following: A collimated beam of a monochromatic laser light is incident on a piece of non-linear optical crystal. Most of the light goes through the crystal, but under suitable conditions a very small fraction (typically of the order of 10^{-12}) will be scattered via the parametric scattering process. This will generally be in the forward direction within a cone of a few degrees solid angle. The colour of the scattered light is shifted towards the red end of the spectrum, and its wavelength can be tuned very accurately with the direction of the crystal. The intensity of the scattered light is proportional to the intensity of the incident beam, the length of the crystal, the square of the nonlinear susceptibility, and the fourth power of its frequency.

According to a semiclassical picture of the process, it comes about because of the fact that the dielectric constant of the crystal is not a constant, but depends on the incident light itself (and on the excitations in the crystal, such as molecular vibrations, phonons, etc., that give rise to different processes), and this produces a coupling of the incident wave with light of different frequencies (or with the crystal excitations). Specifically, the dielectric constant of the macroscopic susceptibility χ_{ij} of the crystal can be written as a constant $\chi_{ij}^{(1)}$ term corresponding to the usual linear susceptibility plus other terms depending on the electric field of the incident light wave E_k (or the molecular vibration coordinate Q_k, the acoustic wave strain S_{kl}, etc.):

$$\chi_{ij} = \chi_{ij}^{(1)} + \sum_k \chi_{ijk}^{(2)} E_k + \cdots \tag{6.25}$$

It is the second term which gives rise to the parametric process. Higher order terms in the macroscopic susceptibility lead to terms in the induced macroscopic polarisation of the medium that are not linearly proportional to the electric field of the light wave:

$$P_i = P_i^{\mathrm{L}} + P_i^{\mathrm{NL}} \tag{6.26}$$

where

$$P_i^{\mathrm{L}} = \sum_j \chi_{ij}^{(1)} E_j \tag{6.27}$$

$$P_i^{\mathrm{NL}} = \sum_{j,k} \chi_{ijk}^{(2)} E_j E_k + \cdots \tag{6.28}$$

Classically the parametric process can be seen as the mixing of an incident photon of frequency ω_p with the quantum-mechanical zero point fluctuation of the electromagnetic radiation at ω_i that leads to a polarisation that oscillates at

$$\omega_s = \omega_p - \omega_i \tag{6.29}$$

through the nonlinear term of (6.28). Seen quantum-mechanically, the parametric process is a process by which an incident photon spontaneously breaks down, due to the nonlinearities of the crystal, into a pair of photons. At the same time the process is not caused by the thermally excited molecular vibrations, or the blackbody radiation because the corresponding energies involved $\hbar\omega_i$ are larger than the corresponding thermal energy kT at room temperatures.

In addition to the frequency matching condition above, there is also a similar matching condition for the wave vectors that comes from Eq. (6.28). The justification is that had this not been the case there would be destructive interference if the contributions of the process is averaged over a scattering volume larger than the wavelengths involved. The phase matching condition for the wave vectors reads:

$$\mathbf{k}_s = \mathbf{k}_p - \mathbf{k}_i \tag{6.30}$$

This means that the incident photon breaks down into two photons with wave vectors $\mathbf{k}_s$ and $\mathbf{k}_i$ in the nonlinear medium when the frequency and the wave vector condition are simultaneously satisfied.

To describe the process the total Hamiltonian can be separated into two parts:

$$\mathcal{H}_{\text{tot}} = \mathcal{H}_0 + \mathcal{H}_1, \tag{6.31}$$

where $\mathcal{H}_0$ is independent of the nonlinearity of the medium and $\mathcal{H}_1$ describes the nonlinear interaction. The interaction free part is given by:

$$\mathcal{H}_0 = \frac{1}{8\pi}\int [\mathbf{D}\cdot\mathbf{E} + \mathbf{B}\cdot\mathbf{H}]\, d\mathbf{r} \tag{6.32}$$

Now, the electric field in the medium can be expanded in plane extraordinary and ordinary waves normalised in the continuous spectrum:

$$\begin{aligned} E(\mathbf{r},t) =& \frac{i}{2\pi}\int \frac{\sqrt{\hbar\omega_o}}{|n_o(\mathbf{k})|}\Big[a_o(\mathbf{k})\exp(i\mathbf{k}\cdot\mathbf{r} - i\omega_o t) \\ &\qquad - a_o^+(\mathbf{k})\exp(-i\mathbf{k}\cdot\mathbf{r} + i\omega_o t)\Big]\,\hat{o}(\hat{k})\, d\mathbf{k} \\ &+ \frac{i}{2\pi}\int \frac{\sqrt{\hbar\omega_o}}{|n_e(\mathbf{k})|}\Big[a_e(\mathbf{k})\exp(i\mathbf{k}\cdot\mathbf{r} - i\omega_e t) \\ &\qquad - a_e^+(\mathbf{k})\exp(-i\mathbf{k}\cdot\mathbf{r} + i\omega_e t)\Big]\,\hat{e}(\hat{k})\, d\mathbf{k} \end{aligned} \tag{6.33}$$

where $a_e^+(\mathbf{k})$ and $a_e(\mathbf{k})$ are the creation and annihilation operators for a quantum of extraordinary wave with wave vector $\mathbf{k}$ and polarisation $\hat{e}(\hat{k})$; and similarly $a_o^+(\mathbf{k})$, $a_o(\mathbf{k})$ and $\hat{o}(\hat{k})$ for the ordinary wave. Using the representation in Eq. (6.33), the free part of the interaction Hamiltonian becomes

$$\begin{aligned} \mathcal{H}_0 =& \frac{1}{2}\int \hbar\omega_o[a_o^+(\mathbf{k})a_o(\mathbf{k}) + a_o(\mathbf{k})a_o^+(\mathbf{k})]\, d\mathbf{k} \\ &+ \frac{1}{2}\int \hbar\omega_o[a_e^+(\mathbf{k})a_e(\mathbf{k}) + a_e(\mathbf{k})a_e^+(\mathbf{k})]\, d\mathbf{k} \end{aligned} \tag{6.34}$$

The interaction Hamiltonian

$$\mathcal{H}_1 = \int\int_0^{\mathbf{E}} \mathbf{P}^{\text{NL}}\cdot d\mathbf{E}\, d\mathbf{r} \tag{6.35}$$

should be consistent with Eq. (6.25), and since the induced macroscopic nonlinear polarisation of the medium is given by Eq. (6.28):

$$P_i^{\mathrm{NL}}(\mathbf{r},t) = \sum_{jk} \chi_{ijk}^{(2)} E_j(\mathbf{r},t) E_k(\mathbf{r},t)$$

the interaction Hamiltonian becomes

$$\mathcal{H}_1 = \sum_{ijk} \int \chi_{ijk}^{(2)} E_i(\mathbf{r},t) E_j(\mathbf{r},t) E_k(\mathbf{r},t)\, d\mathbf{r} \tag{6.36}$$

Substitution of $E(\mathbf{r},t)$ from Eq. (6.33) into the interaction Hamiltonian above leads to a number of terms involving triplets of creation and annihilation operators. These terms describe all possible three-photon processes that can take place in the environment described. But the parametric processes corresponding to either the annihilation of one photon together with the creation of two others, or the inverse one, have to satisfy specific momentum conditions. Therefore they can be separated out. These processes are proportional to

$$a_2(\mathbf{k})\, a_3(\mathbf{k})\, a_1^{+}(\mathbf{k})\, \delta(\mathbf{k}_1 - \mathbf{k}_2 - \mathbf{k}_3) \quad \text{and} \quad a_2^{+}(\mathbf{k})\, a_3^{+}(\mathbf{k})\, a_1(\mathbf{k})\, \delta(\mathbf{k}_2 + \mathbf{k}_3 - \mathbf{k}_1) \tag{6.37}$$

Considering only Type-II matching, i.e., the annihilation of one photon together with the creation of two others, this leads to the following part of the interaction Hamiltonian:

$$\begin{aligned} \mathcal{H}_1' = -i \iiint \sum_{ijk} & \frac{\chi_{ijk}^{(2)}\, e_i(\hat{k}_1)\, o_j(\hat{k}_2)\, o_k(\hat{k}_3)}{n_e(\mathbf{k}_1) n_o(\mathbf{k}_2) n_o(\mathbf{k}_3)} \sqrt{\hbar^3\, \omega_{e_1}\, \omega_{o_2}\, \omega_{o_3}} \\ & \times \Big\{ a_e^{+}(\mathbf{k}_1)\, a_o(\mathbf{k}_2)\, a_o(\mathbf{k}_3)\, \delta(\mathbf{k}_1 - \mathbf{k}_2 - \mathbf{k}_3) e^{i(\omega_{e_1} - \omega_{o_2} - \omega_{o_3})t} \\ & - a_e(\mathbf{k}_1)\, a_o^{+}(\mathbf{k}_2)\, a_o^{+}(\mathbf{k}_3)\, \delta(\mathbf{k}_2 + \mathbf{k}_3 - \mathbf{k}_1) e^{i(\omega_{o_2} + \omega_{o_3} - \omega_{e_1})t} \Big\}\, d\mathbf{k}_1\, d\mathbf{k}_2\, d\mathbf{k}_3 \end{aligned} \tag{6.38}$$

Finally, the process is described by the transition from an initial state $|\psi_{\text{in}}\rangle$ where there is a number of photons per unit volume at the pump frequency, N_p, to a state $|\psi_{\text{fin}}\rangle$ where there are $N_p - 1$ pump photons and one signal and one idler photons. The initial state is given by the normalised state which is produced from hitting the grand state $|\psi_0\rangle$, where there are no photons in the crystal, with N_p creation operators $a_e^+(k_p)$:

$$|\psi_{\text{in}}\rangle = \frac{(2\pi)^{3/2}}{\sqrt{N_p!}}[a_e^+(k_p)]^{N_p}|0\rangle \tag{6.39}$$

This actually corresponds to an incoherent pump beam where all the photons have the same frequency. For a coherent pump beam the superposition of the photon states at the various frequencies is needed, but it turns out that the transition probability is independent of the coherence property of the beam. The interaction Hamiltonian, $\mathcal{H}_1'$, then leads to a scattering to the final state:

$$|\psi_{\text{fin}}\rangle = \frac{1}{\sqrt{N_p! - 1}}a_o^+(k_s)a_o^+(k_i)[a_e^+(k_p)]^{N_p-1}|0\rangle \tag{6.40}$$

And the transition probability per unit volume and time is given by

$$W = \lim_{T\to\infty}\frac{1}{VT}\left|\frac{1}{i\hbar}\int_0^T \langle\psi_{\text{fin}}|\mathcal{H}_1'|\psi_{\text{in}}\rangle\, dt\right|^2 \tag{6.41}$$

6.2.1. Creation of Photon Pairs in Parametric Down Conversion

In optical parametric down conversion a pump beam is incident on a nonlinear birefringent crystal, creating one or more photon pairs with a low conversion efficiency f. The state of the field can be written as Fock States in the following way,

$$|\psi\rangle = |0\rangle + f|1_s\rangle|1_i\rangle + f^2|2_s\rangle|2_i\rangle + \cdots \tag{6.42}$$

where the numbers n_s and n_i (i.e., 1_s, $1_i\ldots$ etc.) are the number of signal and idler photons in a given mode of the field. Given that the parametric process has a very low conversion efficiency f, the terms for the creation of two or more photon pairs can be neglected. For all practical purposes therefore only one pair consisting of signal and idler is of interest.

The photon pair fulfils the usual phase matching conditions [25] that are stemming from the conservation of energy and momentum:

$$\omega_s + \omega_i = \omega_p, \qquad \vec{k}_s + \vec{k}_i = \vec{k}_p. \tag{6.43}$$

This means that the sum of the frequencies of the created photon pair is well defined by the pump laser frequency ω_p. The individual frequency spectrum is relatively broadband because there are several ways to fulfil Eq.(2). The propagation directions of the two light quanta (collinear or different directions), which reflects the phase matching relations, are determined by the orientation of the crystal. Additionally the down-conversion can be categorized in terms of the polarization (parallel or orthogonal) of the photons as type I or type II. In practice, in order to create a photon pair that is entangled in momentum and phase, degenerate photons are selected by several apertures and interference filters. Furthermore several ways of producing polarization entangled photon pairs have been introduced in a number of experiments using type II downconversion.

Specifically, in parametric down-conversion pairs of photons are created out of a strong (coherent) pump optical field that is incident in a suitable second-order nonlinear crystal such as β-BaB_2O_4 (BBO). The crystal can be assumed as having length L and cross-section area A with all the relevant dimensions being much bigger than any optical wavelength involved in the process. To avoid complications by optical interfaces, it can also be assumed that the crystal is embedded in a material of the same linear susceptibility. The electric polarization in a nonlinear medium can be expanded in powers of the electric field. The i-th component is expressed as

$$p_i(\mathbf{r},t) = \sum_{j=1,2,3} \chi_{ij}^{(1)} E_j(\mathbf{r},t) + \sum_{j,k=1,2,3} \chi_{ijk}^{(2)} E_j(\mathbf{r},t) E_k(\mathbf{r},t) + \cdots \tag{6.44}$$

The interaction Hamiltonian between the field and the local polarization is

$$H_{\text{int}} = \epsilon_0 \int_V \mathbf{p} \cdot \mathbf{E}\, d^3r \tag{6.45}$$

It can be further assumed that only one component of the second order non-linear susceptibility is different from zero, which for type-II parametric down-conversion is the $\chi^{(2)}_{eeo}$ component, where e and o represent two orthogonal linear polarizations and the first index refers to the pump light. Typically in down-conversion the pump can be treated as a classical wave of amplitude E_p, whereas the down-conversion modes have to be treated in a quantized picture. A detailed calculation can then show that the non-linear part of the interaction Hamiltonian (the linear part is a part of the unperturbed system) simplifies to

$$\begin{aligned} H_{\mathrm{NL}}(t) = &- \epsilon_0 \chi^{(2)}_{eeo} E_p \int d^3k_e \, d^3k_o \, E_0(k_e) E_0(k_o) a^\dagger_e(k_e) a^\dagger_o(k_o) \times \\ &\delta(\pm k_p - k_e - k_o) \delta^2(\mathbf{k}^\perp_e + \mathbf{k}^\perp_o) e^{i(\pm\omega_p + \omega(k_e) + \omega(k_o))t} \end{aligned} \tag{6.46}$$

where

$$E_0(k) = \sqrt{\frac{\hbar\omega(k)}{(2\epsilon_0)}}$$

is the amplitude of the field due to one photon in a mode with wavenumber k.

The perturbation solution for the whole process tells us that the final state $|\psi(t)\rangle$ is given by

$$\begin{aligned} |\psi(t)\rangle &= \exp\left\{\frac{1}{ih}\int_0^t H_{\mathrm{I}}(t')\, dt'\right\} |\psi(0)\rangle \\ &\approx c_0|0\rangle_s|0\rangle_i + c_1|1\rangle_s|1\rangle_i + \cdots \end{aligned} \tag{6.47}$$

where $|\psi(0)\rangle = |0\rangle_s|0\rangle_i$ is the initial state and $c_1 \propto \epsilon_p \chi^{(2)} t$.

Chapter 7: The Kim and Shih Experiment

After years of being mostly ignored by the majority of the physicists, the Popper thought experiment did eventually acquire its own realisation. Popper had thought of his experiment as realisable, and would have wanted to see it find its way to the experimental laboratory. Unfortunately he did not live to see this happen, as the concretisation of the experiment only came some five years after his death. The fact that the experiment did find its way to the physicist's laboratory is yet another testimony, should it ever be needed, that despite his lack of technical expertise Popper did contribute to the subject of quantum mechanics, not least with this thought experiment.

Moreover, the authors of the report[362] of the experiment claim in their abstract that "The experimental data show $\Delta y \Delta p_y < h$ for photon 2",[363] and ask if this can be thought of as indicating a violation of the uncertainty principle.

This chapter gives a critical review of the experiment by Kim and Shih, and the next chapter gives a review some of the reactions in the physics literature.

7.1. The Report of the Kim and Shih Experiment

Kim and Shih start their report by noting that, while similar to the EPR *gedankenexperiment* of 1935 (that is, Popper intends his experiment to show that a particle can have both precise position and momentum), Popper's experiment did not attract the attention of the physics community. They then state that they wish to report on their realisation of Popper's experiment, and further that "it is astonishing to see that the experimental results agree with Popper's prediction".[364] They explain this by asserting that they use the property of entanglement

[362] Kim and Shih (1999)

[363] (*Ibid.*: 1849)

[364] (*Ibid.*: 1850)

to acquire knowledge of a photon's position, but at the same time they do not observe a corresponding increase in the uncertainty of its momentum as expected by the Copenhagen interpretation. So, they ask if this indicates a violation of the uncertainty principle. They further claim that this is not a surprising result, as it is similar to results from other EPR types of experiments, and lately the focus lies more on causality, locality, and reality rather than on the uncertainty principle.

Kim and Shih describe Popper's experiment as based on the entanglement of a two-particle state, which allows the determination of the position or momentum of particle 2 from the corresponding property of particle 1, "due to the momentum conservation of the quantum pair".[365] Then they describe the situation where both slits are very narrow and some extreme counters start firing, indicating "the greater Δp_y due to the smaller Δy",[366] and they add that this is a situation which is agreeable to both Popper and the Copenhagen school. But when one of the slits (say B) is widened, then, through the entanglement of the pair, knowledge of the precise position of the particle going through that slit is acquired without the particle going through a very narrow slit. So Popper poses the question of whether this knowledge brings about a higher uncertainty in momentum, as this is required by the uncertainty principle. If it does then for Popper we have some action-at-a-distance; if it does not, then there is a problem for the Copenhagen school.

[365] (*Ibid.*: 1850)

[366] (*Ibid.*: 1851)

7.1.1. Realisation of Popper's Experiment by Kim and Shih

Kim and Shih's realisation of the Popper experiment involves the use of entangled photon pairs. They achieve the production of such pairs by using the process of Spontaneous Parametric Down-Conversion (SPDC). That is, the pairs of photons are created out of a coherent pump optical field (i.e. a laser) that is incident in a second-order nonlinear crystal (in this case β-BaB_2O_4 (BBO)).

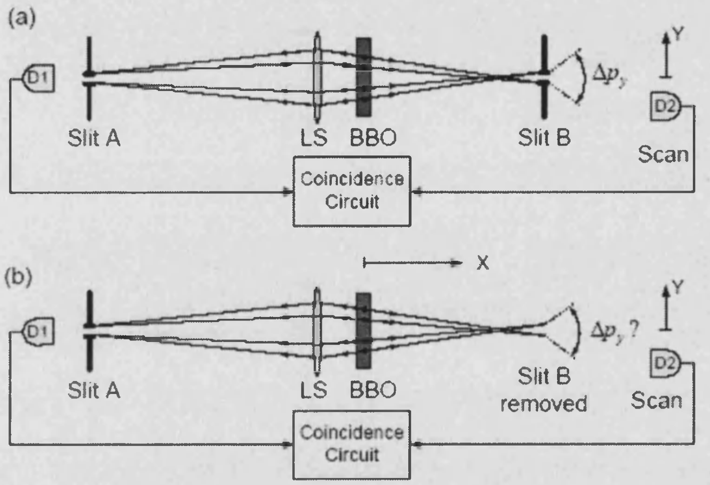

Figure 4. Kim and Shih's modified version of the Popper experiment.[367]

Moreover, they have slightly modified (Figure 4) the original design in order to deal with the difficulties arising from the non-point-like source: They move the source from the centre of the arrangement and they insert a lens there so that they can take advantage of the entanglement properties and create "a "ghost image" of slit A at "screen" B".[368] Effectively, in the arrangement of Figure 4, if there was a light source on the left of slit A and the BBO source was removed, the function of the lens would be to focus the light emerging from slit A so that a sharp image of the slit would be created where the screen of slit B lies. This is so, provided

[367] (*Ibid.*: 1852)

[368] (*Ibid.*: 1852–3)

that the locations of slit A, the lens and the "ghost image" are governed by the Gaussian thin lens equation:

$$\frac{1}{a} + \frac{1}{b} = \frac{1}{f},$$

where a is the distance between the lens and the slit A, b is the distance b_1 from the lens through the beam splitter to the source, plus the distance b_2 from the source through the beam splitter to the screen B, and f is the focal length of the lens.

In this way, they claim, one can achieve the central aim of Popper's experiment, which is to extract the information of the y-position of the B-particle from that of the A-particle, within an accuracy Δy equal to the width of slit A. That is, even when the slit B is wide open, slit A gives the information for the position of its ghost image at screen B. At the same time measurement of the diffraction pattern behind screen B allows a determination of Δp_y of photon 2. This is achieved by the recording of the coincidences between detectors D_1 and D_2, when detector D_2 is moved to various positions behind screen B. This allows the calculation of the product $\Delta y \, \Delta p_y$ for photon 2 and its comparison with h.

Kim and Shih next discuss the use of point source in Popper's original experiment. They say:

> notice that a "point source" is *not* a necessary requirement for Popper's experiment. What is required is the position entanglement of the two-particle system: if the position of particle 1 is precisely known, the position of particle 2 is also 100% determined. So one can learn the precise knowledge of a particle's position through quantum entanglement. Quantum mechanics does allow the position entanglement for an entangled system (EPR state) and there are certain practical mechanisms, such as the "ghost-image" effect shown in our experiment, that can be used for its realization.[369]

Here Kim and Shih have the wrong emphasis. The criticism that Popper's version faced was that if the source was indeed point-like, then coherence would be lost. If on the other hand, the source is not point-like, then the geometry of the

[369] (*Ibid.*: 1853–54)

experiment would not allow the determination of the y-position of the B-particle from the position of the A-particle. Kim and Shih claim that they overcame the problem of the point source by the use of a set-up that allows "the position entanglement of the two-particle system".[370] But this, by itself, is not enough to overcome the problem. Entanglement alone only tells us that the total momentum is conserved. But Popper's suggestion is that we need to know the position of the particle. What Kim and Shih did which ensures that the y-position of the B-particle can be inferred from the position of the A-particle, was to insert the lens and thus focus the photons in such a way that although the pair of photons is emerging through the crystal, it is as if there is one photon travelling from screen A through the lens and the source, towards the screen B where an image of the slit A is created. In a sense, Kim and Shih are wrong when they claim that "Quantum mechanics does allow the position entanglement for an entangled system",[371] since quantum mechanics allows for the momentum correlation, and then the lens ensures the position correlation.

Next, Kim and Shih give the specifics of their experiment (Figure 5). The laser is a CW Argon ion laser with a frequency of $\lambda = 351.1$nm. It is incident on the BBO crystal which is of type II SPDC. This generates an orthogonally polarized signal-idler photon pair. By not focusing the laser beam the phase-matching condition,

$$\mathbf{k}_s + \mathbf{k}_i = \mathbf{k}_p$$

(where $\mathbf{k}_j$ ($j = s, i, p$) are the wavevectors of the signal (s), idler (i), and pump (p) respectively), is reinforced in the SPDC process.

The signal-idler beams are generated with a frequency which is twice that of

[370] (*Ibid.*: 1853)

[371] (*Ibid.*: 1853–4)

the pump laser: $\lambda_s = \lambda_i = 702.2\,\text{nm} = 2\lambda_p$. They are generated collinear with the pump and they are separated from it when they go through a fused quartz dispersion prism. They are consequently split through the use of a polarization beam splitter PBS, and they follow different paths. The signal beam runs a length of 1000 mm, i.e., twice the focal length ($= 2f$), after it has gone through the lens (with a diameter of 25mm), towards the A slit (that has a diameter of 16mm). The idler beam follows the path towards the slit B. Because the total distance from the lens back to the BBO crystal and forwards to the slit B is again twice the focal length, $2f = 1000$mm, Kim and Shih assert that this achieves "a "ghost image" of slit A (0.16 mm) at 1"screen" B".[372] Once the two beams pass through the slits they hit the photon counting detectors. In fact the signal beam passes again through a lens so that it can be focussed on detector D_1. Thus the detector D_1 is stationary. The other detector D_2 can run along the y-axis.

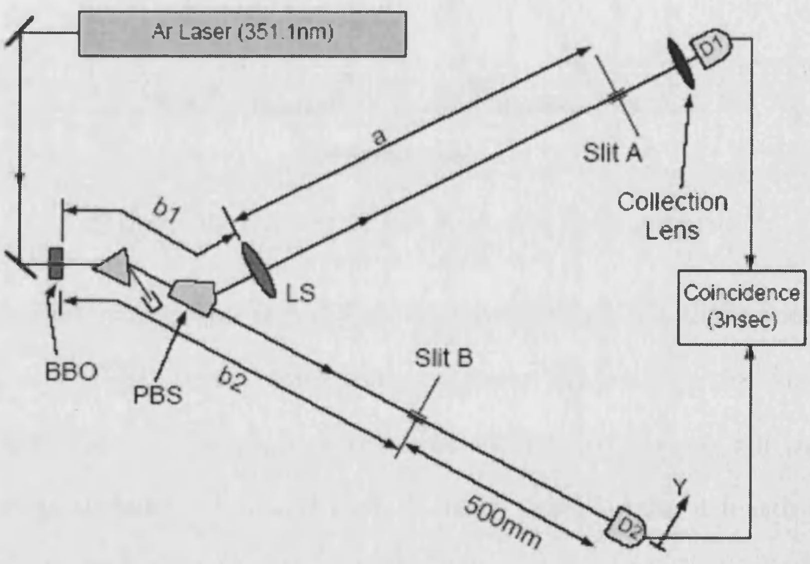

Figure 5. Experimental set-up of the Kim and Shih experiment.[373]

Both detectors are point-like: their diameter is 180μm. They produce pulses

[372] (*Ibid.*: 1854)

[373] (*Ibid.*: 1854)

that are fed to a coincidence circuit, and, moreover, the use of "10 nm band-pass spectral filters centered at 702 nm",[374] presumably ensures that only photons of the appropriate frequency are detected.

Next Kim and Shih report their measurements (Figure 6). They conduct two measurements, one with the slit B in place and one with the screen at B wide open. In both they measure the coincidence rates against the y coordinate on the position of detector D_2.

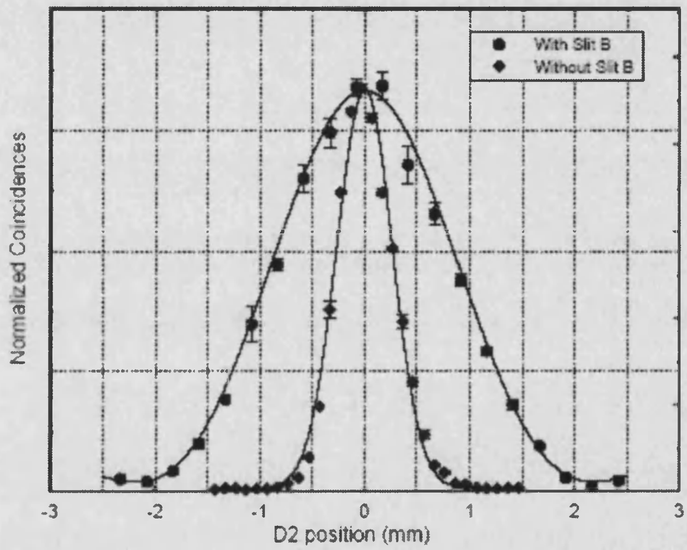

Figure 6. Results of the Kim and Shih experiment.[375]

The first measurement yielded "a typical single-slit diffraction pattern with $\Delta y \Delta p_y = h$".[376] In fact a closer examination of the pattern that represents coincidence with the slit B in place, shows that on the two edges of the pattern the data point seem to have a low and then to start rising again, a feature characteristic of the secondary peaks one would expect from a typical single-slit diffraction pattern. It is unfortunate that Kim and Shih did not include data from further than

[374] (*Ibid.*: 1855)

[375] (*Ibid.*: 1854)

[376] (*Ibid.*)

the 2.5mm on either side. The width of this pattern "represents the minimum uncertainty of Δp_y".[377] This means that this first measurement was conducted in order to establish the width of the pattern which represents the minimum uncertainty in the y-momentum of the photons that pass through a slit on the B screen that has a width of 0.16mm. Once this is done, the uncertainty Δp_y, can be compared with the uncertainty when the slit is wide open.

The second measurement was conducted with the screen at B wide open. It was done under the assumption that

> Because of entanglement of the signal-idler photon pair and the coincidence measurement, only those twins which have passed through slit A and the "ghost image" of slit A at "screen" B with an uncertainty of $\Delta y = 0.16\,\text{mm}$ ... would contribute to the coincidence counts through the simultaneous triggering of D_1 and D_2.[378]

This means that Kim and Shih assume that the experimental set-up is such that all the photons which passed through slit A have counterparts which pass through the "ghost image" of slit A on the B screen. For this second measurement Kim and Shih report that: "The measured width of the pattern is narrower than that of the diffraction pattern shown in measurement 1"; and further that: "The experimental data has provided a clear indication of $\Delta y \Delta p_y < h$ in the coincidence measurements"[379]

7.1.2. Kim and Shih's Analysis of Popper's Experiment

Next Kim and Shih give their analysis of their version of Popper's Experiment. They start by asking whether or not the indication of $\Delta y \Delta p_y < h$ in the coincidence measurements reported in their measurements constitutes a violation of the uncertainty principle. In order to answer this question they examine the quantum mechanical prediction, and ask "[i]f quantum mechanics does provide a

377 (*Ibid.*)

378 (*Ibid.*: 1856)

379 (*ibid.*)

solution with $\Delta y \Delta p_y < h$ for "photon 2." Indeed, we would be forced to face a paradox as EPR had pointed out in 1935".[380]

Kim and Shih start by asking whether quantum mechanics predicts that the counterparts of the photons which go through the A slit do indeed go through a "ghost slit" located at screen B, and thus provide precise knowledge of the position of photon 2 when it goes through the B screen when the slit is wide open. They give a positive answer and they provide two arguments for this.

The first argument for the positive answer is based on the entangled two-photon state of SPDC together with the Gaussian thin lens equation. They start by giving the quantum mechanical state of the pair of photons:

$$|\Psi\rangle = \sum_{s,i} \delta(\omega_s + \omega_i - \omega_p)\delta(\mathbf{k}_s + \mathbf{k}_i - \mathbf{k}_p) a_s^\dagger(\omega \mathbf{k}_s) a_i^\dagger(\omega \mathbf{k}_i)|0\rangle \tag{7.1}$$

where $a_s^\dagger$ and $a_i^\dagger$ are the respective creation operators for the signal and the idler. This equation simply gives the infinite ways in which a pair of signal and idler photons in the SPDC state can satisfy the conservation of energy and momentum conditions given by the delta functions, i.e., the phase-matching conditions:

$$\omega_s + \omega_i = \omega_p, \quad \text{and} \quad \mathbf{k}_s + \mathbf{k}_i = \mathbf{k}_p \tag{7.2}$$

The above state does not specify the energy or the momentum of either of the two photons on their own, but it determines that they are correlated by the above equations.

It should be noted here that the above state in (7.1) is not an EPR state. The wave function for a pair of particles that form an EPR state in 2-D is given by:

$$\Psi(x_1, x_2) = \int\int e^{ik_1 x_1} e^{ik_2 x_2}\, \delta(k_1 + k_2)\, \delta\left(\frac{k_1^2}{2m} + \frac{k_2^2}{2m} - E\right)\, dk_1\, dk_2$$

$$= \int e^{ik_1(x_1 - x_2)}\, \delta\left(\frac{k_1^2}{m} - E\right)\, dk_1$$

380 (*ibid.*)

Using

$$\delta(mx) = \frac{\delta(x)}{m} \quad \Rightarrow \quad m\delta(mx) = \delta(x)$$

and

$$\delta(x^2 - a^2) = \frac{1}{2a}\left[\delta(x-a) + \delta(x+a)\right] \quad .$$

we get

$$\Psi(x_1, x_2) = \int m\, e^{ik_1(x_1-x_2)}\, \delta(k_1^2 - mE)\, dk_1$$

$$= \int \frac{m}{\sqrt{mE}}\, e^{ik_1(x_1-x_2)} \left[\delta\Big(k_1 - \sqrt{mE}\Big) + \delta\Big(k_1 + \sqrt{mE}\Big)\right] dk_1$$

$$= \frac{1}{2}\sqrt{\frac{m}{E}} \left[\int e^{ik_1(x_1-x_2)}\, \delta\Big(k_1 - \sqrt{mE}\Big)\, dk_1 \right.$$
$$\left. + \int e^{ik_1(x_1-x_2)}\, \delta\Big(k_1 + \sqrt{mE}\Big)\, dk_1\right]$$

$$= \frac{1}{2}\sqrt{\frac{m}{E}} \left(e^{i\sqrt{mE}(x_1-x_2)} + e^{-i\sqrt{mE}(x_1-x_2)}\right)$$

$$= \frac{1}{2}\sqrt{\frac{m}{E}} \cos\Big(\sqrt{mE}(x_1 - x_2)\Big)$$

Furthermore, performing the same analysis in 3-D, the wave function for the two particles forming an EPR state is given by:

$$\Psi(\mathbf{r}_1, \mathbf{r}_2) = \int\int e^{i\mathbf{k}_1\cdot\mathbf{r}_1} e^{i\mathbf{k}_2\cdot\mathbf{r}_2}\, \delta\left(\mathbf{k}_1 + \mathbf{k}_2\right)\, \delta\!\left(\frac{k_1^2}{2m} + \frac{k_2^2}{m} - E\right)\, d^3\mathbf{k}_1\, d^3\mathbf{k}_2$$

$$= \int e^{i\mathbf{k}_1\cdot(\mathbf{r}_1-\mathbf{r}_2)}\, \delta\!\left(\frac{k_1^2}{2m} - E\right) d^3\mathbf{k}_1$$

We have

$$d^3\mathbf{k}_1 = k_1^2 \sin\theta\, dk_1\, d\theta\, d\phi \qquad \text{and} \qquad \mathbf{k}_1 \cdot (\mathbf{r}_1 - \mathbf{r}_2) = |\mathbf{r}_1 - \mathbf{r}_2|\, k_1 \cos\theta$$

We can also use $\mu = \cos\theta$, $d\mu = \sin\theta d\theta$:

$$\Psi(\mathbf{r}_1, \mathbf{r}_2) = \int_0^\infty dk_1 \int_0^{2\pi} d\phi \int_0^\pi d\theta \left[\exp\{i|\mathbf{r}_1 - \mathbf{r}_2|k_1 \cos\theta\} k_1^2 \sin\theta \; \delta\!\left(\frac{k_1^2}{m} - E\right)\right]$$

$$= \int_0^\infty dk_1 \int_0^{2\pi} d\phi \int_{-1}^1 d\mu \left[\exp\{i|\mathbf{r}_1 - \mathbf{r}_2|k_1\mu\} k_1^2 \; m\,\delta(k_1^2 - Em)\right]$$

$$= 2\pi m \int_0^\infty dk_1 \left[k_1^2 \;\; \delta(k_1^2 - Em) \int_{-1}^1 \exp\{i|\mathbf{r}_1 - \mathbf{r}_2|k_1\mu\}\, d\mu\right]$$

$$= 2\pi m \int_0^\infty dk_1 \left[k_1^2 \;\; \delta(k_1^2 - Em) \frac{e^{i|\mathbf{r}_1 - \mathbf{r}_2|k_1} - e^{-i|\mathbf{r}_1 - \mathbf{r}_2|k_1}}{i|\mathbf{r}_1 - \mathbf{r}_2|k_1}\right]$$

$$= \frac{2\pi m}{i|\mathbf{r}_1 - \mathbf{r}_2|} \int_0^\infty dk_1 \left\{k_1 \left(2\sqrt{mE}\right)^{-1} \left[\delta\!\left(k_1 - \sqrt{mE}\right) + \delta\!\left(k_1 + \sqrt{mE}\right)\right] \right.$$
$$\left. \left(e^{i|\mathbf{r}_1 - \mathbf{r}_2|k_1} - e^{-i|\mathbf{r}_1 - \mathbf{r}_2|k_1}\right) \right\}$$

$$= \frac{-i\pi\sqrt{m/E}}{|\mathbf{r}_1 - \mathbf{r}_2|} \left\{\left[\sqrt{mE}\left(e^{i|\mathbf{r}_1 - \mathbf{r}_2|\sqrt{mE}} - e^{-i|\mathbf{r}_1 - \mathbf{r}_2|\sqrt{mE}}\right)\right]\right.$$
$$\left. + \left[-\sqrt{mE}\left(e^{-i|\mathbf{r}_1 - \mathbf{r}_2|\sqrt{mE}} - e^{i|\mathbf{r}_1 - \mathbf{r}_2|\sqrt{mE}}\right)\right]\right\}$$

$$= -\frac{i2\pi m}{|\mathbf{r}_1 - \mathbf{r}_2|} \left[e^{i|\mathbf{r}_1 - \mathbf{r}_2|\sqrt{mE}} - e^{-i|\mathbf{r}_1 - \mathbf{r}_2|\sqrt{mE}}\right]$$

$$= -\frac{i2\pi m}{|\mathbf{r}_1 - \mathbf{r}_2|} \sin\left(|\mathbf{r}_1 - \mathbf{r}_2|\sqrt{mE}\right)$$

For normalisation we need that the total probability is equal to 1. We have:

$$1 = \int_{-\infty}^{\infty} \Psi(\mathbf{r}_1, \mathbf{r}_2)\Psi^*(\mathbf{r}_1, \mathbf{r}_2)$$

$$= \int_{-\infty}^{\infty} \frac{4\pi^2 m^2 N^2}{|\mathbf{r}_1 - \mathbf{r}_2|^2} \sin^2\left(|\mathbf{r}_1 - \mathbf{r}_2|\sqrt{mE}\right) d|\mathbf{r}_1 - \mathbf{r}_2|$$

$$= 4\pi^3 m^{5/2}\sqrt{E}N^2$$

where N is the normalisation constant:

$$N = \sqrt{\frac{1}{4\pi^3 m^{5/2}\sqrt{E}}} = \frac{\pi^{-3/2}m^{-5/4}E^{-1/4}}{2}$$

So the wavefunction becomes:

$$\Psi(\mathbf{r}_1, \mathbf{r}_2) = -\frac{i2\pi m}{|\mathbf{r}_1 - \mathbf{r}_2|}\frac{\pi^{-3/2}m^{-5/4}E^{-1/4}}{2} \sin\left(|\mathbf{r}_1 - \mathbf{r}_2|\sqrt{mE}\right)$$

$$= -\frac{i\pi^{-1/2}m^{-3/4}E^{-1/4}}{2\,|\mathbf{r}_1 - \mathbf{r}_2|} \sin\left(|\mathbf{r}_1 - \mathbf{r}_2|\sqrt{mE}\right)$$

Returning to Kim and Shih's analysis, they have given an "unfolded" figure of their experiment which is equivalent to assuming that one is working in a frame where $\mathbf{k}_s + \mathbf{k}_i = 0$. In this way they give

> ... the only possible optical paths of the signal-idler pairs that result in a "click-click" coincidence detection which are represented by straight lines in this unfolded version of the experimental schematic so that the "image" of slit A is well-produced in coincidences[381]

So in this unfolded version of their experiment it looks as if there is one photon travelling from the slit A through the lens and the source to the slit B, i.e., along the path of the two photons together. Given that, they justify their assumption that the image of the A slit is "clear" by the requirement that the locations of the two slits and the lens are the ones given by the Gaussian equation:

$$\frac{1}{a} + \frac{1}{b} = \frac{1}{f} \tag{7.3}$$

[381] (*Ibid.*: 1857)

By choosing, as mentioned above $a = b = 2f$ the two slits, slit A and its "ghost image" should have the same width. Kim and Shih add that "[t]he measured size of the "ghost image" agrees with theory".[382] They conclude that because of the entanglement of the two photons, their set-up does provide a precise knowledge of the position of the "ghost image" of the slit through which photon 2 has gone through. It should be noted here that, as we shall see later, Short challenged the conclusion that the set-up provides such precise knowledge.

The second argument for the positive answer to the question whether or not photon 2 does go through the "ghost slit" at screen B is based on conditional measurements. Given that the measurements are coincidence measurements, photon 2 is detected if and only if photon 1 has gone through slit A and been detected by D_1. Given also that Δp_y is measured by the width of the diffraction pattern of photon 2, Kim and Shih argue that:

> The two-photon paths, indicated by the straight lines, reach detector 2 which is located 500 mm behind "screen" B so that detector D_2 will receive "photon 2" in a much narrower width under the condition of the "click" of detector D_1 as shown in measurement 2, unless a real physical slit B is applied to "disturb" the straight lines.[383]

And so

> ...we have a paradox: quantum mechanics provides us with a solution which gives $\Delta y \Delta p_y < h$ in measurement 2 and the experimental measurements agree with the prediction of quantum mechanics.[384]

Now, this last sentence looks somewhat strange: Why would it be a paradox if the prediction of the theory and the experimental results agree? Obviously Kim and Shih think of this situation as paradoxical because they consider it as contradicting the tenet which says that for any system the product of the uncertainties cannot be less than h. If their arguments are valid this tenet cannot be taken as part

382 (*ibid.*)

383 (*ibid.*: 1858)
384 (*ibid.*)

of the formalism, because in that case the formalism contradicts itself. Therefore they obviously need to take this to be a tenet of the Copenhagen interpretation of the formalism, and, pushing the argument to its logical conclusion, they should consider this as a test against the Copenhagen interpretation.

One can of course take the view, as indeed Redhead did, that there is no paradox here: Given that the quantum formalism predicts that $\Delta y \Delta p_y < h$, and the Copenhagen interpretation is an interpretation of the formalism, and the test confirms the prediction, there is no problem for either the formalism or its interpretation. But this is not an avenue open to Kim and Shih since according to the above quote, they consider the prediction of the theory and its confirmation by their experiment as a paradox.

But do Kim and Shih consider this as a test against the Copenhagen interpretation and as vindicating Popper? Apparently not. And not for the reason given by Redhead for his rejection. They justify this in their conclusion by asserting that the mistake which Popper makes is to neglect the fact that this is a two particle system and not a one-particle system.

7.2. Kim and Shih's Conclusion

Kim and Shih start their conclusion by asserting that the paradox they refer to in the above quote "is the same paradox of EPR",[385] and that this could indeed be considered a "variant of the 1935 EPR gedankenexperiment in which the position-momentum uncertainty was questioned by Einstein-Podolsky-Rosen based on the discussion of a two-particle entangled state".[386] Furthermore, they state that "[a]ll reported historical experiments have shown good agreement with quantum mechanics as well as EPR's prediction (but not their interpretation)".[387] They therefore think that since their experimental results agree with both quantum mechanics and Popper's prediction, their discussion which follows applies both to the EPR and the Popper experiment. They say:

> Popper and EPR were correct in the prediction of the physical outcomes of their experiments. However, Popper and EPR made the same error by applying the results of two-particle physics to the explanation of the behavior of an individual particle. The two-particle entangled state is not the state of two individual particles. Our experimental result is emphatically NOT a violation of the uncertainty principle which governs the behavior of an individual quantum.[388]

So, according to Kim and Shih, both Popper and EPR made the mistake of applying the wrong ontology. The problem is metaphysical rather than physical. They go on to justify their claim:

> ... the measurements are "joint detection" between two detectors applied to entangled states. Quantum mechanically, an entangled two-particle state only provides *the precise knowledge of the correlations of the pair.* Neither of the subsystems is determined by the state. ... A quantum must obey the uncertainty principle but the "conditional behavior" of a quantum in an entangled two-particle system is different. The uncertainty principle is not for "conditional" behavior. We believe paradoxes are unavoidable if one insists the *conditional behaviour* of a particle is the *behaviour* of a particle. This is the central

[385] (*ibid.*)

[386] (*ibid.*)

[387] (*ibid.*)

[388] (*ibid.*: 1858–59)

problem of the rationale behind both Popper and EPR. $\Delta y \Delta p_y \geq h$ is not applicable to the conditional behavior of either "photon 1" or "photon 2" in the case of the Popper and EPR type of measurements.[389]

So, according to Kim and Shih, the paradoxes only appear because of Popper's insistence on applying the uncertainty principle to what they call "conditional behaviour". They go further by asserting that "Each of the *straight lines* in the above discussion corresponds to a two-photon amplitude",[390] and only the two-photon amplitudes appear in the formalism. The two photons are never considered individually in the formalism. This makes Popper's question "about the momentum uncertainty of photon 2 ... inappropriate".[391]

Here Kim and Shih do not explain why and how this question is inappropriate. They only say that "There is no reason to expect that the "conditionally localized photon 2" will follow the familiar interpretation of the uncertainty relation".[392] But they do not tell us what is it that what is the photon expected to do instead of 'following the familiar interpretation'. Is it instead suppose to follow a different interpretation of the uncertainty formula? This would be a natural interpretation of what they say. But they do not tell us what that interpretation might be. So we are left empty handed if that is what they meant. Or is it instead supposed to follow the familiar interpretation of a *different* uncertainty relation. If yes, then what is this different relation. Again they do not tell us, and in any case it is hard to reconcile what they say with this interpretation. What they do it to jump immediately to the following aphorism: they believe that "all the problems raised by the EPR and Popper type experiments can be duly resolved if the concept of

389 (*ibid.*: 1859) their emphasis

390 (*ibid.*)

391 (*ibid.*)

392 (*ibid.*)

biphoton is adopted in place of two individual photons".[393]

But how does that help? The idea of the bi-photon naturally suggests some type of holism or non-separability of states, discussed earlier in the thesis. This is an interesting idea but Kim and Shih do not develop it at all. We are just supposed to accept that its adoption will just resolve the issue at hand. If one tries to see how this could make sense, then one has to consider what is implied by holism and non-separability. The idea is that the properties, or the state of the whole are not determined by the those of the parts. But that only means that the properties of each individual photon does not determine the two-photon amplitude. This does not mean that each individual photon does not when any of its properties *upon* measurement. After all, if that was the case then what is it that their results represent?

It might be objected here that what does follow is that one cannot use the measurement of one particle to determine the properties of the other. That might well be indeed something that follows from a holistic approach. But it does not follow in this case. As noted earlier, Peres has shown that in this specific case it follows from the formalism that the localisation of the one photon, means that the other will be found in the symmetric position. So this does not help Kim and Shih.

In any case, holism and non-separability was an idea that applies to quantum mechanics exactly because the results of the experiments show non-classical correlations. It is difficult to see how it is suppose to help Kim and Shih to explain the fact that their results are, on face value, not exhibiting this non-classical characteristic.

Returning to Kim and Shih, their suggestion would seem absurd to Popper.

393 (*ibid.*)

Popper would see this as a no-win situation. If his prediction is *not* verified by the test then the Copenhagen interpretation is vindicated; if his prediction *is* verified by the test then the Copenhagen interpretation is vindicated again because he is using wrong metaphysics.

Earlier, while discussing Redhead's reaction to the Popper experiment, I gave an outline of what Popper's argument is. It is worth considering the revised version of this argument for this version of the experiment:

1. The formalism and its realist interpretation predict a decrease in scatter for the momentum p_y, of the photon 2 when the B-slit is removed.
2. The formalism and its realist interpretation suggest that we have the same precision in the knowledge of the y-position of photon 2 when the B-slit is removed, as when the slit was present.
3. According to the Copenhagen interpretation any such knowledge prohibits the decrease in scatter, and consequently the decrease of the uncertainty of the momentum p_y, of the photon 2 when the B-slit is removed, and therefore the scatter for p_y should not decrease.
4. *Therefore*, if the test does show this decrease in scatter for p_y, then the Copenhagen interpretation of the formalism (but not the formalism itself) is refuted and Popper's interpretation vindicated.

As in the discussion of Redhead's criticism, this is a valid argument. Kim and Shih need to spell out far more clearly what their suggestion is and how it is suppose to show that Popper's interpretation of the results would not apply.

Chapter 8: The Physicists' Reaction to the Kim and Shih Experiment

Once performed, and with a controversial outcome, the Popper thought experiment saw a number of papers being dedicated to it by the physics community. Here the papers by Unnikrishnan,[394] Plaga,[395] Short,[396] and in particular Rigolin[397] will be examined.

8.1. Unnikrishnan on the Kim and Shih Experiment

One of the first reactions to the Kim and Shih experiment that appeared in the physics literature was given by C.S.Unnikrishnan. Unnikrishnan starts his paper by recognising that the Kim and Shih experiment is a realisation of the Popper experiment, and states that his intention is to show that the results of the experiment are consistent with the Copenhagen Interpretation, as well as the examination of signal locality and momentum conservation in the experiment.

8.1.1. Unnikrishnan's First Objection to Popper: The Correct Copenhagen Prediction Is $\Delta y_2 \Delta p_2 < \hbar$

Unnikrishnan states that Popper asks if fixing the location of photons on one branch of the experiment would affect the momentum spread of the photons on the other branch. He observes that by using entanglement together with a naïve application of the uncertainty principle to the second photon, Popper deduced that the Copenhagen prediction for the ensemble of particles going through the virtual slit is $\Delta y_2 \Delta p_2 \geq \hbar$. But Unnikrishnan objects that this is not the correct quantum mechanical prediction. His argument is as follows:

[394] Unnikrishnan (2000)

[395] Plaga (2000)

[396] Short (2001)

[397] Rigolin (2001) and (2002)

> For the first photon, after passing through the slit, the uncertainty relation demands $\Delta y_1 \Delta p_1 \geq \hbar$. In the production of the entangled pair, the two momenta are correlated and by choosing a subset of events in which one photon passes through a small aperture, the companion photon's momentum ray and hence its transverse position are well determined. This implies that $\Delta y_2 \Delta p_2 < \hbar$. The quantum mechanical prediction for the uncertainty product for the second photon is $\Delta y_2 \Delta p_2 < \hbar$, and this was confirmed by the experiment.[398]

This argument by Unnikrishnan constitutes yet another example of the mistakes that one can make by insisting on arguing from one's geometrical intuition concerning the flight of subatomic particles, like photons. His argument is that if the photon's momentum ray is well determined, then so is its transverse position. In effect this says that if the y-momentum is well determined then so is the y-position of the particle. But this is precisely the inference that the uncertainty relation precludes us from drawing. This does not mean that the Copenhagen prediction *has* to be $\Delta y_2 \Delta p_2 \geq \hbar$, in this situation. It only means that one cannot infer from the fact that the photon momentum is well determined that the photon's position is also well determined, to the extent that for the product of the uncertainties we have that $\Delta y_2 \Delta p_2 < \hbar$, and present this as the Copenhagen prediction.

Unnikrishnan then goes further to claim that the prediction $\Delta y_2 \Delta p_2 < \hbar$ is, contrary to what Popper thinks, in agreement with the views of Bohr and others concerning the EPR situation. According to him, this is because the system under consideration is a pair of particles, and the wave function applies to the pair. So the uncertainty principle applies to quantities that pertain to the full system, and not to the parts of the system. This has the consequence that the spread of momentum for the whole system is given by Δp_1 and the position spread is given by the slit width $\Delta y_1 \simeq \Delta y_2$, so that the uncertainty relation is satisfied, $\Delta y \Delta p_1 \geq \hbar$, without requiring that $\Delta y_2 \Delta p_2 \geq \hbar$, as Popper suggests. Now assuming that this is the case this means that for the second particle the

[398] (*Op. cit.*: 198)

relationship $\Delta y_2 \Delta p_2 < \hbar$ holds, as long as it belongs to an entangled pair and as long as we have detected its counterpart going through the first slit. Therefore, whether we would measure its position spread by placing a slit in its path or not its irrelevant for the position spread; it should always be $\Delta y_2 \simeq \Delta y$. And since the momentum spread is Δp_1 such that $\Delta y_1 \Delta p_1 \geq \hbar$, if Unnikrishnan's argument is correct, we should expect to find a reduced Δp_2 so that $\Delta y_2 \Delta p_2 < \hbar$, *irrespective* of the presence of the slit, since the quantities that enter the uncertainty relation are the ones for the whole pair, rather than the ones for the individual systems. Unfortunately for Unnikrishnan one of the findings of the experiment is that the spread in momentum *reduces* when the second slit is removed.

8.1.2. Unnikrishnan on Locality and Conservation of Momentum

Unnikrishnan also makes the argument that if we were to find in the Popper experiment that $\Delta y_2 \Delta p_2 \geq \hbar$, then signal locality would have to be violated. He goes on to say that in the Kim and Shih variant of the experiment there would be no violation of signal locality because, as was seen in the description of the experiment, the experiment is based on the use of coincidence counters, that have to use the signals of two spatially separated detectors and therefore obey signal locality. It should be mentioned here that this is precisely Popper's conclusion too. He admits that if $\Delta y_2 \Delta p_2 \geq \hbar$ were to be the outcome of the experiment, then he would have to admit that signal locality is violated; he says: "if the particles whose y-position has been indirectly measured at B show an increase in scatter... [t]his *could* be interpreted as indicative of an action at a distance..."[399] So this point of Unnikrishnan is not arguing against Popper in any way.

There is also a last point that Unnikrishnan makes concerning the conservation of momentum. He says "In Popper's experiment the measurement of the

[399] Popper (1982*b*: 29) emphasis in the original

transverse position and of the transverse momentum are independent and only classical momentum correlation is used to deduce the transverse momentum and position of the companion particle."[400] And he concludes that momentum conservation alone is enough to deduce the result of the experiment. This says exactly what Popper is arguing for. Popper is using the argument that because of momentum conservation that has to hold in the absence of an interference by a slit, he would be able to deduce the transverse position and by measuring the transverse momentum he would show that both properties obtain, whereas this would not be the case under the Copenhagen interpretation. But the fact that conservation of momentum implies $\Delta y_2 \Delta p_2 < \hbar$ and not $\Delta y_2 \Delta p_2 \geq \hbar$ does not mean therefore Popper is wrong in thinking that the Copenhagen interpretation suggests that $\Delta y_2 \Delta p_2 \geq \hbar$. That might or might not be the case, but Unnikrishnan cannot use classical conservation of momentum as a reason for deciding that the Copenhagen interpretation suggests either of the two signs in the inequality. This is the very issue under discussion and Unnikrishnan is using the result that is favourable for Popper to say to him that Copenhagen interpretation is right because of the result shows what Popper predicts.

8.1.3. Unnikrishnan's Conclusion

In his conclusion Unnikrishnan claims that the correct interpretation of the uncertainty principle for this experiment suggests that the Copenhagen interpretation is consistent with the result of the Kim and Shih experiment, namely that $\Delta y_2 \Delta p_2 < \hbar$. Irrespective of what is the correct interpretation for the experiment, Unnikrishnan's argument was shown to be false, because it is based on deducing the prediction of the Copenhagen interpretation by inferring the knowledge of one conjugate quantity from the knowledge of another, and this is precisely what this

400 (*Op. cit.*: 199)

interpretations precludes us from doing. Furthermore Unnikrishnan also points out his discussion of signal locality and conservation of momentum. His discussion of those two points in no way works against Popper in his conclusions for the results of the experiment.[401]

[401] Qureshi (2003) makes a similar argument to Unnikrishnan, but with a discrete model for the Popper experiment.

8.2. Plaga on the Kim and Shih Experiment

Yet another early reactions to the Kim and Shih experiment was given by R. Plaga.[402] For Plaga one of the central tenets of the Copenhagen Interpretation is that the state function represents our knowledge of a system. Popper's thought experiment, on the other hand was aimed at testing "the relation of a quantum-mechanical state to observed reality".[403] Plaga starts by identifying two flaws in Popper's original proposal and suggests an improvement which he calls "*Extension step* 1", and which, as he claims, "fully serves its original purpose".[404] His main purpose is therefore to suggest the execution of this experiment. He further claims that the so called "many-worlds" interpretation and the Copenhagen interpretation predict identical results for the experiment. He therefore suggests an "*Extension step* 2" which is supposed to distinguish qualitatively between the two. *Extension step* 2 is based on using an isolated ion as particle detector.

Plaga describes the original proposal by Popper and states Popper's question as asking what happens when the slit B is removed. According to him Popper's argument goes as follows: once one knows that the particle 1 has gone through the A slit, one also knows that particle 2 has gone through the "virtual" B slit, and therefore it is the localised state of particle 2 that has to be used for the prediction of the statistics of detection of particle 2. Detection of particle 1 here acts as a "preparation" for the state of particle 2, and "[m]ere knowledge can change the unitary evolution of photon 2".[405] Furthermore, Plaga notes that for reasons that are not of interest to him, Popper predicts that this is an effect which will not take place.

[402] Plaga (2000)

[403] (*Ibid.*: 462)

[404] (*Ibid.*: 461)

[405] (*Ibid.*: 466)

8.2.1. The First Flaw According to Plaga

The first flaw which Plaga points to is the one pointed out by Collett and Loudon, namely that since the particles at the source would have to obey the uncertainty principle, so that they are not localised at the source, particle 2 is not localised in slit B. Plaga then notes that Kim and Shih have performed the experiment in a way which solves this problem, with the use of a lens that ensures the localisation of photon 2 within the "virtual" slit B: "the phase matching conditions localise the quantum state of photon 2 within slit B of equal size. This is true even if it is not known from where in BBO crystal the photons were emitted."[406] Plaga then notes that Kim and Shih did not find that the momentum spread that corresponds to the width of the slit according to the Heisenberg's uncertainty relation. To clarify: Plaga accepts that the lens does indeed confine particle 2 within the width of slit B, provided that the twin particle has gone through the A slit.

8.2.2. The Second Flaw According to Plaga

Then Plaga goes on to identify the second conceptual flaw in Popper's original proposal. He claims that "the localisation via entanglement is not knowable to the observer performing the momentum-spread determination".[407] This he explains by claiming that when the observer behind the B-slit performs the momentum spread measurement for particle 2, "it is in principle still not knowable for him — for obvious causality reasons — whether particle 1 passed though slit A"[408] and therefore this observer will be using the "unlocalised" state function for his predictions. Plaga further states that "in spite of the fact that the observer...

[406] (*Ibid.*: 468)

[407] (*Ibid.*: 469)

[408] (*ibid.*)

near slit A already knows that the particle passed the slit - *one expects no increased momentum spread in the CI*".[409]

In order to fix this alleged (this will be discussed below) flaw, Plaga proposes what he calls "Extension step 1". His motivation is to make sure that virtual diffraction takes place because of the virtual slit, and he therefore looks for an extension of the experiment which will ensure the knowledge that the particle has gone through the virtual slit is acquired by the observer behind the B-slit. So he states that there are two conditions which need to be fulfilled by his extension: First, "[t]he click of detector 1 has to signal the localisation of particle 2 within the virtual slit B", and second, "[a]t time when particle 2 reaches detector D2 Bob has to be at a space-time point on the past light-cone of detector 1's click",[410] where, in the usual notation, "Bob" is the observer behind the B-slit. Given those two conditions, Bob can use as his state function the localised one, and only then, Plaga thinks, is the CI tested. His extension then consists of inserting a mirror in the path of photon 2 behind the lens, so that it is reflected and it follows a path that brings it to the detector D2 that now is near the detector D1. Furthermore, instead of slit A there is a mirror with the same size as the slit A. Plaga maintains, that this set-up ensures that his two conditions are fulfilled.

Before discussing this alleged second flaw, it is interesting to note that for Plaga, fulfilment of his two conditions implies that, first, the observer behind the B-slit, acquires knowledge of the passage of particle 2 through the B-slit by the clicking of Detector D1, and he therefore has to use the localised state function, and, second, that the "standard time evolution of quantum mechanics now predicts an enhanced momentum spread of this particle (i.e. the occurence

409 (*ibid.*) emphasis in the original

410 (*ibid.*)

of virtual diffraction)".[411] Furthermore in that case Popper's idea that the CI is tested by this set-up is indeed a viable one, and the absence of virtual diffraction would cast doubt on the relation between the "quantum-mechanical state" and "observed reality" as this is envisaged by the CI, i.e., as holding that the state function represents our knowledge of a system.

Plaga also thinks that the absence of virtual diffraction also renders his proposed "Extension step 2" irrelevant. The rest of his article assumes that virtual diffraction will be present in an experiment that fulfils his two conditions and it is therefore of no interest for the present discussion, since, as the following discussion argues, the Kim and Shih set-up does indeed fulfil the requirement for knowledge of the passage of particle 1 through the A-slit, and does not show virtual diffraction.

8.2.3. Discussion of the Second Flaw

The problem in what Plaga suggests is that the whole idea of "Extension step 1" seems to be misconceived. The Popper description, and the Kim and Shih set-up involve coincidence detectors. This means that, by its very nature, the measurement of the momentum spread, p_y, for the particles that have gone towards the B-screen, involves knowledge of the fact that their counterpart in their pair has gone through the A-slit. Otherwise they are not counted. Presumably there is a number of particles that reach the detector behind the B-slit, and whose counterparts do not pass through the A-slit. If these were to be counted for the statistical distribution of the p_y momentum, then the experiment would have nothing to say about the applicability of the uncertainty principle when the B-slit is removed. But the use of coincidence counters is supposed to ensure that only those particles whose counterparts have gone through the A-slit are counted.

[411] (*Ibid.*: 470)

But in that case the measurement is done on the condition that the passage of the sibling particle through the A-slit is known, and therefore the flaw that Plaga claims is simply absent. The "obvious causality reasons"[412] for which he claims that when the observer behind the B-slit measures particle 2's momentum, in principle he still doesn't know if the other particle has passed though slit A, are not obvious at all. The very nature of the coincidence measurements presupposes this knowledge: out of all the particles that reach the detector only the ones for which another one has reached the detector on the other side will be counted. The state function that the observer has to use then for these particles, is indeed the localised one.

[412] (*Ibid.*: 469)

8.3. Short on the Kim and Shih Experiment

One of the most important reactions to the Kim and Shih experiment was given by A. J. Short.[413] Short notes that the Kim and Shih experiment, while avoiding the flaws of the original proposal through the use of SPDC and an optical lens, captures the essence of Popper's proposal. Interestingly enough Short admits that the reported results of the experiment are in an apparent agreement with Popper's prediction. Furthermore, unlike Unnikrishnan whose paper was discussed earlier, Short finds that the application of the uncertainty principle to conditional measurements that are performed on sub-systems that are separated is valid. Short therefore does not accept that the Copenhagen interpretation predicts that for the second photon we have $\Delta y_2 \Delta p_2 < \hbar$, but he tries to explain the result of the apparent violation of the principle, that was found in the Kim and Shih experiment, by an optical analogy.

8.3.1. Short's description of the Experiment

In Section 2 of his paper Short gives a description of Kim and Shih's experiment, which follows closely the description given by them. He starts with the description of the positioning and distances of the SPDC source, the lens and the slits, and notes the use of the coincidence circuit that gives conditional measurements of the y-coordinate of the photon 2 in two situations: with the slit B present and absent.

Short then deals with the problem of the uncertainty in position and momenta with which the pairs are created. He says: "Although the photons are created with a large uncertainty in position and momentum there are strong correlations between each pair due to the phase matching conditions of SPDC."[414] For Short,

[413] Short (2001)

[414] (*Ibid.*: 276)

this comes from the almost perfect anti-correlation of the two photons, expressed by $\mathbf{k}_1 + \mathbf{k}_2 \simeq 0$, and this means that "the two-photon trajectories are well represented by straight lines and may be treated like optical rays."[415] This is supported by experimental findings.[416]

Short then notes that the experimental set-up is equivalent to one where the SPDC source is omitted and a lamp is placed behind slit A. In this set-up an optical image of slit A is created, due to the presence of the lens, on the plane of slit B that has the same size as slit A. The equality of the sizes of the slit and its image is a consequence of the Gaussian thin lens equation $1/a + 1/b = 1/f$, and the fact that both slits are at a distance of two focal lengths away from the lens. Because of this equality of sizes "one would not expect the behaviour of photon 2 to be affected if slit B is also narrowed to this width",[417] but, as the results of the experiment showed, "[i]t appears that the presence of a physical slit affects the results even though it does not change the spatial confinement of the photon."[418]

Short then goes on to report the difference between the two cases, as this was found in the experiment. He notes that in the first case, when slit B is narrowed to the same width as slit A (0.16mm), the distribution behind slit B has a width of 4.4mm, whereas in the second case, with no slit B, this is reduced to 1.6mm, i.e., the uncertainties in momenta have the following relation between them: "$\Delta_{(ii)}p_y \approx 0.36\Delta_{(i)}p_y$, where $\Delta_{(i,ii)}$ refers to the uncertainty in cases (i) and (ii) respectively."[419] Then, by accepting the argument that has the uncertainties in position to be equal in the two cases, we are led to a reduction in the product of

[415] (*ibid.*)

[416] Pittman et. al. (1995)

[417] (*op. cit.*: 277)

[418] (*ibid.*)

[419] (*Ibid.*: 278)

the two uncertainties and consequently to a violation of the uncertainty principle.

Short notes here a difficulty with defining exactly a violation of the uncertainty principle "because we are looking at peak widths rather than standard deviations." And because "[t]he $sinc^2$ function generated by diffraction actually has an infinite standard deviation, and alternative measures of uncertainty and uncertainty relations are therefore required".[420]

It should be noted here that peak widths are measures of standard deviations, irrespective of which function is generated from the theoretical description. In any case what is important here is the *prima facie* reduction in the uncertainty of momentum in accordance with the prediction made by Popper.

8.3.2. The Uncertainty Principle for Conditional Measurements

In Section 3 of his paper Short examines whether or not the uncertainty principle should be expected to hold for conditional measurements. We have already seen in the first section of this chapter how Unnikrishnan argued erroneously that the uncertainty principle should not be applied to this situation as Popper predicted because it applies to quantities that pertain to the full system, and not to the parts of the system. Contrary to that Short shows that it should be applied to any of the two subsystems in this situation.

Short starts by observing that the principle constrains any measurements of non-commuting observables and, following Isham,[421] he represents it by the general inequality

$$\Delta_\psi A \, \Delta_\psi B \geq \frac{1}{2} \left| \langle \psi | [\hat{A}, \hat{B}] | \psi \rangle \right|, \qquad (Sh.1)$$

for any two observables $\hat{A}$ and $\hat{B}$. It follows from this that if a system is comprised of many particles, then for the n'th particle we have the usual inequality for

[420] (*Ibid.*: 278*fn*)

[421] Isham (1995)

position and momentum:

$$\Delta x_n \Delta p_n \geq \frac{\hbar}{2} \qquad \forall n. \tag{Sh.2}$$

Then, in order to investigate whether or not this is valid for conditional measurements on entangled systems, Short considers a more general case: "'*A measurement M_2 of one of two non-commuting observables $\hat{A}$ or $\hat{B}$ given that a measurement M_1 of $\hat{O}$ obtains the result o'*."[422] Short considers two cases, the first when M_1 takes place before M_2, the second when M_2 takes place before M_1.

In the first case (M_1 preceding M_2), if $|\psi\rangle$ is the initial system, then since M_1 obtains the result o, then the system has became:

$$|\psi'\rangle = \frac{\hat{P}_o|\psi\rangle}{(\langle\psi|\hat{P}_o|\psi\rangle)^{1/2}} \tag{Sh.3}$$

with $\hat{P}_o$ being the operator projecting to the eigenstate(s) with eigenvalues o. The state then unitarily evolves to $|\psi''\rangle = \hat{U}|\psi'\rangle$ and then undergoes M_2. Here Short makes the following argument. Since the principle can be applied to measurements on any quantum state, it should also be applied to M_2 and so for the results of M_2 we have:

$$\Delta A \, \Delta B \geq \frac{1}{2}\left|\langle\psi''|[\hat{A},\hat{B}]|\psi''\rangle\right|. \tag{Sh.4}$$

For the second case (where M_2 precedes M_1) Short recognises that the situation is more complex, but gives an argument that shows that a measurement "M_1 which obtains the result o *after* measurement M_2 can therefore be replaced by a measurement M_1' obtaining o'... *before* measurement M_2."[423] By this he concludes that the ordering of the two measurements is irrelevant and the above result can be applied. And specifically for the Kim and Shih case, since the photons evolve independently from each other, for measurement M_2 being either "of [the] position

422 (*op. cit.*: 279) emphasis in the original.

423 (*Ibid.*: 280)

$\hat{A} = \hat{y}$ or [the] momentum $\hat{B} = \hat{p}_y$ of photon 2 in the plane of slit B given that a measurement M_1 on photon 1 detects it at D_1",[424] photon 2 should obey the relation $\Delta y \Delta p_y > \hbar/2$ "even though y and p_y refer to conditional quantities".[425]

So unlike Unnikrishnan, Short finds that the prediction of the Copenhagen interpretation is that $\Delta y_2 \Delta p_2 \geq \hbar$. At the same time the results of the experiment appear to be $\Delta y_2 \Delta p_2 < \hbar$, so in the next section Short attempts to explain this apparent violation of the uncertainty principle.

8.3.3. Short's Explanation of the Experimental Results

In Section 4 of his paper Short tries to resolve the paradox of the apparent violation of the principle. His answer is that we are mistaken in thinking that the image of slit A in the plane of slit B is perfect. He takes into account the fact that the SPDC source has a finite width, and this apparently results to an image that is "actually 2-3 times larger than the physical slit, precisely as predicted by the uncertainty relation."[426] For Short this also explains the increase of the uncertainty when slit B is narrowed since this selects only the centre of the image of slit A.

Before continuing with the Short's explanation it is worth recalling the equivalence that Short has noted between the experimental set-up and one where the SPDC source is omitted and a lamp is placed behind slit A. As noted earlier, in this set-up an optical image of slit A is created, due to the presence of the lens, on the plane of slit B that has the same size as slit A. An important feature of this equivalent set-up is that it shows clearly the reliance of the assumption of the perfect image on the straight lines that start from slit A, pass through the lens,

424 (*ibid.*)

425 (*ibid.*)

426 (*ibid.*)

and are focussed by the lens to the image on the plane of slit B.

Coming back to Short's explanation, he says that the image resolution is mainly limited by the width of the SPDC source, which should be identified with the laser-pumped region that is $\sim$ 3mm. This, Short claims, corresponds to an intuitive replacement of the SPDC source with a aperture of diameter 3mm, and leads to the image of slit A being blurred.

Then Short explains that the same effect can be understood in terms of imperfect phase matching in the SPDC process. Although we assume that the phases of the two photons are related by $\mathbf{k}_1 + \mathbf{k}_2 \simeq 0$, in fact the sum is not equal to zero, but to $\Delta(k_{1y} + k_{2y})$ which is given by

$$\begin{aligned} \Delta(k_{1y} + k_{2y})\Delta y_1 &\geq \frac{1}{2}, \\ \Delta(k_{1y} + k_{2y})\Delta y_2 &\geq \frac{1}{2}, \end{aligned} \tag{$Sh.5$}$$

Short notes that Δy_1 and Δy_2 are not larger than the source width, and therefore $\mathbf{k}_1 + \mathbf{k}_2$ is not exactly equal to zero. This means that the representation of the photons' trajectories as straight lines is an approximation, and their disturbance would result to a blurred image.

Then Short derived the same relation for a photon entering and leaving the source region, $\Delta k'_y \Delta y_s \geq 1/2$ and explains that this shows how the two approaches are equivalent, i.e, that "replacing the SPDC source with an appropriate slit in the single-photon system we are therefore simulating the effect of imperfect phase matching."[427]

Next Short derives his estimation of the width of the blurred image. On this he says:

> We can estimate the width of the blurred image by treating the SPDC source as a circular aperture in the single-photon system, with a diameter equal to that of the pump beam.

[427] (*Ibid.*: 281)

Using this simple model each point in the image is spread by convolution into an Airy function of diameter (between first minima)

$$\Delta y = \frac{2.44D\lambda}{s} \qquad (Sh.6)$$

where D is the distance from source to image (745mm), λ is the photon wavelength (702.2nm) and s is the source width (3mm). This gives a blurring of $\Delta y = 0.43$mm, which is almost 3 times larger than the expected width of the image (0.16mm) and is sufficient to ensure that the results do not violate the uncertainty principle.[428]

Furthermore there is a corresponding momentum spread that geometrically is given from

$$\frac{\Delta p_y}{p} \simeq \frac{s}{D} \qquad (Sh.7)$$

and in conjunction with (Sh.6) above and $p = h/\lambda$ we get $\Delta y \Delta p_y \approx 2.4h$.

This means that with the slit B

narrowed to the same width as slit A (0.16mm) then only those photons passing through the centre of the blurred image will be detected at D_2. This increased spatial confinement gives the photons a greater momentum spread and results in a broader pattern at D_2, as observed in the results for case (i).[429]

Short goes further to observe that a blurred image is a necessary condition for a diffraction pattern if D_1 detects all slit A photons. He considers the wavefunction of the two-photon entangled system when the two photons are passing through their respective slits, which would be, under the perfect image assumption, the entangled state:

$$|\psi\rangle \propto \int_{-s/2}^{s/2} |y\rangle_1 |-y\rangle_2 \, \mathrm{d}y. \qquad (Sh.8)$$

In this case, the state of photon 2 will be given by a reduced density matrix, namely $\hat{\rho}_2 = \mathrm{Tr}_1(|\psi\rangle\langle\psi|)$. This is an incoherent mixed state of the image points:

$$\hat{\rho}_2 \propto \int_{-s/2}^{s/2} |y\rangle_{2\,2}\langle y| \, \mathrm{d}y. \qquad (Sh.9)$$

This results to each of the image points acquiring an infinite transverse momentum spread and, instead of a diffraction pattern, D_2 would show a constant pattern.

428 (*ibid.*: 281)
429 (*ibid.*: 282)

Short concludes that "[i]t is only when the image points are blurred into coherent functions which spread over the width of slit B that a $sinc^2$ interference pattern will be obtained in the results."[430]

8.3.4. Short's Conclusion

Short concludes that although he agrees with Kim and Shih's claim that their findings do not show a violation of the uncertainty principle, he disagrees with the explanation of why this is so given the apparent contradiction with the first reading of the data. Kim and Shih accept that photon 2 is localised with the geometrical image of slit A, but this does not mean that the principle is violated as it should not be applied to conditional behaviour and its measurements. Short asserts that this is not only an unnecessary assertion for the explanation of the results, but he has also shown how the principle can be applied to conditional measurements, as long as the two systems are separated.

Short goes on to say that he has shown the blurring of the "image" of slit A is a "*necessary consequence* of the principle due to position-momentum uncertainty at the SPDC source... The effect is analogous to that which blurs point-source images (e.g. the image of a star) in any lens system with a finite aperture."[431] And furthermore "[t]hese conclusions are also supported by the results of the previous "ghost imaging" experiment of Pittman *et al.*, in which significant blurring is evident in the image."[432]

430 (*Ibid.*: 283)

431 (*ibid.*), emphasis in the original.

432 (*ibid.*)

8.3.5. Discussion of Short's Findings

I shall start the discussion of the findings of Short's paper by pointing out that there is one objection that should be raised. This objection has little consequence for Short's argument but it should be mentioned nevertheless.

The objection is against his claim that he has shown the blurring of the image to be a necessary consequence of the uncertainty principle. What he has done is rather the inverse: Short uses the classical Airy function,

$$\Delta y = \frac{2.44 D\lambda}{s}, \qquad (Sh.6)$$

that gives the spread of a plane wave of diameter s (that he identifies with the source width) and the corresponding momentum spread that is given by the component of the momentum of a particle that travels along the line that confines the spread of the plane wave,

$$\frac{\Delta p_y}{p} \simeq \frac{s}{D}, \qquad (Sh.7)$$

to *derive* the uncertainty relation for photon 2. He has not used the product of uncertainties $\Delta k'_y \Delta y_s \geq 1/2$ together with the fact that Δy_s is equal to the source width, to derive $\Delta k'_y$ and from there the momentum uncertainty Δp_y.

This point might give rise to another objection, as the derivation relies on the Airy function that works for a plane wave. It gives the expansion Δy of a wave that started with a wave front s and ends with a wave front $s + \Delta y$. But in the one photon case that Short considers, because of the presence of the lens, the wave front expands from the width of slit A to a diameter that is larger than the source diameter, goes through the lens, then reduces to the source width, and then reduces even further to the image of the slit. So this is a questionable step in Short's derivation given that the wave front itself reduces and so the spread might be smaller than the one that Short calculates.

One can actually calculate the spherical aberration of the lens, given by the formula

$$\Delta x \simeq \frac{1}{6}\lambda\frac{D}{a} \qquad (Sh.10)$$

where a is the diameter of the lens. Given that $a = 25$mm the result of the order of μm rather than mm, so it becomes irrelevant in this case. So one can always question the use of the Airy function in this case. Short justifies this by the uncertainty principle but, as just mentioned, all he has done is to prove that the uncertainty relation holds for the photons that are emitted from the source. But whether the uncertainty in position they have is the one that Short calculates, this is doubtful.

In general, the best way to decide how much is the uncertainty is to look at the experimental data. Short has actually appealed to an experiment by Pittman *et al.* in which Y. Shih is one of the collaborators.[433] As this is an independent experiment from the Kim and Shih one that looks at the ghost image formation, it is ideal for this purpose.

Before looking into the experiment a relevant observation needs to be made. One of the problems that one encounters with the original proposal is the finite size of the source. In fact there were two problems in connection with that. One is the fact that a source of finite size does not provide an image of equal size for slit A. The second is the fact that if one is to reduce the size in order to avoid this problem, then one runs into the problem of the uncertainty principle because of the reduction in the uncertainty of the y-position and the consequent increase in the uncertainty of the y-momentum that results in non-perfect EPR correlations.

The separation of those two problems is quite important if we are to understand the results of the experiment. The first problem comes from the geometry

[433] Pittman *et al.* (1995)

of the set-up and is a completely classical one, having nothing to do with the problems associated with quantum uncertainties and the like. The second problem is a purely quantum mechanical one. As we have seen, Kim and Shih have recognised the problem of the finite size of the source and have suggested that the insertion of a lens deals with the problem. A similar assertion is made in the Pittman *et al.* paper.

In this paper they rely on the delta function in the definition of the initial state,

$$|\psi\rangle = \sum_{s,i} \delta(\omega_s + \omega_i - \omega_s)\delta(\mathbf{k}_s + \mathbf{k}_i - \mathbf{k}_s)|\mathbf{k}_s\rangle \otimes |\mathbf{k}_i\rangle \quad , \tag{8.11}$$

to deduce that the scattering angles of the signal and the idler photons, α_s and α_i, are related by

$$k_s \sin\alpha_s = k_i \sin\alpha_i. \tag{8.12}$$

Then using Snell's law they deduce that the exiting angles of the signal and the idler photons, β_s and β_i, are related by

$$\omega_s \sin\beta_s = \omega_i \sin\beta_i. \tag{8.13}$$

Therefore "near the degenerate frequency case the photons constituting one pair are emitted at roughly equal yet opposite angles."[434] And they go on to say that although there is large uncertainty in the angle in which either of the photons is emitted, "if one is emitted at a certain angle, its conjugate must have been emitted at an equal, yet opposite angle with unit probability."[435] They go on to say that because of the equal angles they can represent by straight lines the amplitudes of the photon pairs.

Where their argument goes wrong is at the link between Eq. (8.12) and Eq. (8.13). Eq. (8.12) gives the angle in which each photon is emitted but since

434 (*ibid.*: R431)

435 (*ibid.*)

each photon is confined within a region of the order of $s = 3$mm, each one might diverge within an angle of the order of $\phi = \lambda/s$.[436] And this divergence might be different for each one of the two photons. So although Eq. (8.12) holds for the angles at which the photons are emitted, this does not mean that Eq. (8.13) holds necessarily for the angles at which they exit the crystal.

So although the insertion of the lens takes care of the uncertainty into which angle the *pair* is emitted, by focusing the lines to the image through the extended source to the geometrical image of slit A, and thus takes care of the first problem associated with the size of the source, it does not take care of the second problem. The second problem, that of the uncertainty in y-momentum that results from the confinement in the source region, results in the angles of exit from the crystal not being exactly equal.

Of course given that the lens solves the problem of the source not being small enough one could always reduce the uncertainty in y-momentum by increasing the width of the laser. Assuming that the calculation by Short is correct, then increasing the width of the laser gun five-fold would result in an uncertainty of 0.085mm, i.e., half the size of slit A. In that case, the argument by Short loses its force, and the uncertainty in y-momentum should be the same in the two cases, i.e., with the slit present and absent. But this is only if Short's calculation is valid, and there are doubts about that. In any case the increased width should give a better accuracy so this should be an interesting experiment to perform.

The suggestion is made here that increasing the size of the width of the laser, and thereby reducing the uncertainty in y-momentum, the correlation in momentum of the two particles becomes almost perfect and therefore the accuracy of the knowledge of the limits of the area of slit A is increased, which should lead,

[436] Braginski and Khalili (1995: 26)

according to the uncertainty principle, to an increase in the y-momentum uncertainty. Even if, on repetition of the experiment, the increase in the y-momentum uncertainty is not such that the same spread is achieved as in the case with the slit B present, performing the experiment with the slit B being open and showing that an increase in the width of the source results in an increase in the uncertainty of momentum would be enough to show that there is no violation of the uncertainty principle, and the Popper prediction would be in that event refuted. Furthermore a simple measurement of the width of the image slit would also shed some light on the matters discussed here, as it would clarify whether or not the size is the same or larger than the size of the A slit. The set-up of the Pittman *et al.* paper suggests that this is possible.

Returning to the Pittman *et al.* paper, as mentioned, Short, at the end of his paper, claims that their findings support his claim because the ghost image they produce has significant blurring. In the experiment instead of a slit they have inserted an aperture that has the letters UMBC cut so that if light falls on the aperture , those letters are projected. Although the image they produce using entangled pairs is not a perfect image of the aperture, Short's claim is probably a bit exaggerated. First, there is a major difference between the two experiments. In the Kim and Shih experiment, the slit and its image are placed to such distances, so as to be unmagnified. In the Pittman *et al.* experiment the image is magnified by a factor of 2. This could have an effect on the accuracy with which the image is reproduced. Second the scanning step size for the counters behind the corresponding B-slit is of size 0.25mm, which is much larger than the corresponding one in the Kim and Shih experiment. The width of the image letters are two scanning steps, but the width of the aperture letters is not given, so given size of the scanning step, it is reasonable to suggest that the distortion of the image is due to the large size of the scanning step, rather than blurring.

Furthermore Short does not provide a calculation for the expected blurring in this case, so his claim can hardly be supported, by the findings of the Pittman *et al.* paper.

Short's analysis may be flawed But this does not mean that Popper is justified. Further analysis would be needed to show that the uncertainty relations are satisfied, as indeed they must be if Redhead's point is accepted. However Short is actually analysing the wrong experiment! Thus he claims that two photons "do not interact with each other after their initial creation and must evolve independently between measurements when they are space-like separated."[437] But in the two-photon experiment the exigencies of the spin statistics theorem are being ignored. Since the photons have integral spin they must have a symmetric wave function. The correct explanation in this case was offered by Rigolin. This is presented in the next section.

[437] Short (2001: 280)

8.4. Rigolin on the Kim and Shih Experiment.

Rigolin's paper[438] contains a first reaction to Short's paper. He also presents a "generalized uncertainty relation for an entangled pair of particles"[439] by imposing a "symmetrization rule for all operators that we should emply when doing any calculation using the entangled wave function of the pair."[440] The generalised relation suggests that here are "new lower bounds for the product of position and momentum dispersions",[441] although they reduce to the familiar ones when applied to non-entangled particles. Thus, Rigolin thinks that the result of the Kim and Shih experiment can be explained.

Rigolin first gives a brief presentation of the Kim and Shih version of the Popper experiment, together with their results. Specifically he mentions that "Kim and Shih's experiment suggest that $\Delta y_2 \Delta P_{y_2} < \hbar$ in an apparent violation of Heisenberg relation."[442]

Rigolin then looks into the Short paper.[443] According to Rigolin, Short claims that there is no violation of the uncertainty principle because the two photons:

> do not interact with each other after their initial creation and must evolve independently between measurements when they are space-like separated.[444]

Rigolin does not agree with this because an entangled pair of photons would not follow this independent evolution. Instead Rigolin gives his own explanation.

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438 Rigolin (2002)

439 (*Ibid.*: 293)

440 (*Ibid*)

441 (*Ibid*)

442 (*Ibid.*: 294)

443 Short (2001)

444 (*Op. cit.*: 294)

8.4.1. Rigolin's Generalized Uncertainty Relations

Rigolin thinks that when it comes to a correlated system (entangled system) one must use what is called *physical observables*, which have to obey symmetry requirements as well.

These *physical observables* must commute with all the permutation operators that appear in the system. He defines the following operator:

$$\mathcal{O}(1,2) = \sum_{i=1}^{n} A_i(1) \otimes B_i(2)$$

and gives as an example the total angular momentum of two particles:

$$J(1,2) = L(1) \otimes \mathcal{I}_2 + S(1) \otimes \mathcal{I}_2 + \mathcal{I}_1 \otimes L(2) + \mathcal{I}_1 \otimes S(2)$$

where $L(i)$, $S(i)$ and $\mathcal{I}_i$ are the orbital, spin angular momentum and the identity operator of particle i.

Further, the operator $\mathcal{O}(1,2)$ is called a *physical observable* if it satisfies the following commutation relation:

$$[\mathcal{O}(1,2), P_{21}] = 0,$$

where P_{21} is the permutation operator in the state space $\mathcal{E}(1,2)$, and P_{21} is hermitian and obeys the following relation:

$$P_{21}\mathcal{O}(1,2)P_{21}^{\dagger} = \mathcal{O}(2,1).$$

Now, Rigolin defines the extended position and momentum operators, in a given direction,

$$Q(1,2) = Q(1) \otimes \mathcal{I}_2 + \mathcal{I}_1 \otimes Q(2)$$

and

$$P(1,2) = P(1) \otimes \mathcal{I}_2 + \mathcal{I}_1 \otimes P(2)$$

where $\mathcal{I}_i$ is the identity operator in the state space of particle i. It follows that

$$[Q(1,2), P_{21}] = [P(1,2), P_{21}] = 0.$$

So $Q(1,2)$ and $P(1,2)$ are *physical observables.*

Experimentally, in a coincidence measurement, $Q(1,2)$ is the sum of the positions of both particles and $P(1,2)$ gives the total momentum of the system in a given direction.

Then Rigolin demands that only *physical observables* are used in the derivation of the uncertainty relation for a correlated pair:

$$(\Delta Q(1,2))^2(\Delta P(1,2))^2 \geq \frac{|\langle[Q(1,2), P(1,2)]\rangle|^2}{4}.$$

and that one should not use the traditional relations

$$(\Delta Q(i))^2(\Delta P(i))^2 \geq \frac{|\langle[Q(i), P(i)]\rangle|^2}{4}$$

because $Q(i)$ and $P(i)$, where $i = 1$ or $i = 2$, are not *physical observables* and they do not commute with the permutation operator. By writing $Q(i) = Q_i$ and $P(i) = P_i$:

$$[(\Delta Q_1)^2 + (\Delta Q_2)^2 + 2(\langle Q_1Q_2\rangle - \langle Q_1\rangle\langle Q_2\rangle)]\times$$
$$[(\Delta P_1)^2 + (\Delta P_2)^2 + 2(\langle P_1P_2\rangle - \langle P_1\rangle\langle P_2\rangle)] \geq \hbar^2.$$

Assuming, as done by Popper and implictly by Kim and Shih, that $\Delta Q_1 = \Delta Q_2$ we get:

$$\left[(\Delta Q_2)^2 + (\langle Q_1Q_2\rangle - \langle Q_1\rangle\langle Q_2\rangle)\right] \times$$
$$\left[\frac{(\Delta P_1)^2}{2} + \frac{(\Delta P_2)^2}{2} + (\langle P_1P_2\rangle - \langle P_1\rangle\langle P_2\rangle)\right] \geq \frac{\hbar^2}{4}.$$

Rigolin comments that

> This last expression should be the correct uncertainty relation when treating a correlated pair of particles and not the naive Heisenberg uncertainty[445]

[445] (*Op. cit.*: 297)

By claiming that for calculations regarding the symmetric wave function, i.e., for entangled systems of identical particles, the correct variables to use are those that commute with all the permutation operators, Rigolin has showed that in conditional measurements the quantum formalism predicts that "we can have states where $\Delta Q_1 \Delta P_1 < \hbar/2$".[446] This applies to the two-photon system of the Kim and Shih experiment, and shows that the fact that the results show that $\Delta y \Delta p_y < \hbar/2$ does not indicate a violation of the uncertainty principle.

Despite the fact that Popper's prediction for the results of the experiment is verified, that is $\Delta y \Delta p_y < \hbar/2$ indeed, this does not work for Popper for his general argument as the uncertainties nevertheless obey a different uncertainty relation, that is shown by Rigolin to follow from the formalism. Once again the system is unable after all to refute Redhead's claim.

[446] (*Ibid.*: 298)

Chapter 9: Conclusion

By the end of the nineteenth century classical physics was a highly developed mathematical structure that could be applied to all the known macroscopic phenomena. Moreover, it admitted a logical structure that could be interpreted realistically. But the application of the scheme to the microscopic world proved to be more problematic and eventually brought about a new physics with a different logical structure and with its own orthodox Copenhagen interpretation.

The Popper experiment can be seen as one in the class of correlation experiments that were devised in opposition to the orthodox Copenhagen interpretation. This opposition was mainly expressed by Einstein and Popper, as the new orthodoxy had denied that a realistic picture of the world is possible and even deemed the search for physical reality futile, and they never accepted this new state of affairs. They expressed this opposition by questioning the rationality of the new orthodoxy, and by suggesting those thought experiments.

9.1. Popper and the Reasons for his Proposal

Popper's opposition to the Copenhagen Interpretation dates back to its first proposal in the 1920's. He denied that Quantum Mechanics had any special epistemological consequences. His opposition stems from the fact that he is a realist of a metaphysical kind, and sees the belief in realism as the drive for the scientific endeavour: If there is no objective reality, then what are the natural scientists looking for? So any physical theory, quantum mechanics included, should be interpreted realistically. So Popper interprets the Heisenberg relations as scatter relations with no special significance for the theory of knowledge: They are about the scatter of physical particles and not about indeterminacy or uncertainty. Moreover, Popper opposes the view that particles and waves are "complementary" views of the same entities. He instead thinks that particles, which are carriers of energy, are always accompanied by waves, while waves are perhaps not always accompanied by particles. As for the infamous "collapse of the wave packet", Popper thinks that it derives from a misinterpretation of probability theory.

Popper opposes subjectivism and considers it an obstacle to scientific progress. When, for example, physicists talk of "observables" that we can talk about and "hidden variables" that have to excluded, they ignore the fact that the proposal of new theories and theoretical terms always involves "hidden variables". Popper also disagrees with Bohr's suggestion that we should not try to understand theories about the micro-world, and with Heisenberg who thought that quantum mechanics should have a special epistemological status that determines the limits to our knowledge. Popper attributes this view to a misunderstanding of probability theory. He moreover thinks that the indeterminacy relations are merely scatter relations, and quantum theory is a probabilistic or statistical theory.

As mentioned, Popper was not alone in opposing the new orthodoxy. Einstein was in the same camp and proposed an experiment to support his view, the so-called EPR experiment. Popper agrees with the objectives of the EPR paper which, he thinks, was designed to establish that a 'particle may possess at the same time position and momentum'. Moreover, to support his opposition to the Copenhagen Interpretation, Popper presents an experiment similar to the one given by EPR. He thinks that the outcome of the experiment would be predictable by quantum mechanical calculations, and would agree with his specific predictions for its outcome, and show that the Heisenberg uncertainty relations are not limitations to our knowledge.

As Redhead has pointed out, whether the experiment can actually achieve the goals set by Popper, depends on whether, in general, an experiment can indeed refute an interpretation of a theory while, at the same time, leaving the theory intact. Redhead considers how three different interpretations of quantum theory consider the value of an observable Q when the state of the system is not an eigenstate of Q. The Hidden Variables interpretation would say that "Q has a sharp but unknown value", The Propensities and Potentialities interpretations would say "Q has an unsharp or 'fuzzy' value", and Complementarity would say "The value of Q is undefined or meaningless". So Popper *could* have answered Redhead's challenge thus: 'yes because an experiment could be devised in such a way as to show that "Q has a sharp but unknown value", thereby refuting the answer given by the Complementarity view'. This is in fact Popper's prediction for the outcome of the experiment.

So there are two issues at stake here. One is what is the exact prediction of the formalism for the specific experiment and whether this is in agreement with the experimental outcome. The second is whether the outcome of the experiment means that, given that the system is not an eigenstate of an observable Q, we can say that "Q has a sharp but unknown value", or that "Q has an unsharp or 'fuzzy' value", or that "The value of Q is undefined or meaningless".

The experiment is an EPR-type correlation experiment. In the EPR proposal two non-commuting observables are measured: The position along the x-axis for one of the two particles in the pair of correlated particles and the momentum along the x-axis for the other. The non-commuting observables measured in the Popper experiment are the position along the y-axis and the momentum along the y-axis respectively. So to rephrase the question at the end of the previous paragraph, given that we measure the y-position of one of the two particles and therefore the other particle is not in an eigenstate of p_y, can we say that "p_y has a sharp but unknown value", or that "p_y has an unsharp or 'fuzzy' value", or that "The value of Q is undefined or meaningless"?

Before looking at those issues one needs to make sure that technically the experiment is able to explore those issues. Indeed Popper's proposal has a number of technical flaws. Some of these flaws have been raised by a number of authors and some have been raised in this thesis. They include the use of coincidence counters and whether they can be assumed to discriminate between particles that do belong to the same pair and particles that do not, the issue of the finite size of the source, and whether the uncertainty of p_y is in fact a monotonic, increasing function of the scatter in angles $\Delta\theta$. These issues are discussed in Chapter 3. On the issue of the use of coincidence counters it was pointed out that it doesn't arise in the case of photons, provided some care is taken for their sensitivity. It is assumed that this was the case for the Kim and Shih experiment. The issue of the finite size of the source is more problematic and although Kim and Shih seem to think that they overcome it by the insertion of a lens in the path of the second particle of the pair, the discussion by Short shows that this is not a straightforward and unproblematic solution. Finally, the statistical analysis provided in Section 3.5 shows that the uncertainty of p_y is indeed a monotonic, increasing function of the scatter in angles $\Delta\theta$.

9.2. Reactions to Popper's Proposal

A number of responses that predate the Kim and Shih experiment have been considered in Chapters 4 and 5. First, the response by Krips has a number of mistakes and some valid points that have been shown nevertheless to be answerable from Popper's point of view. Moreover, his prediction for the outcome of the experiment is what Popper considers to be the Copenhagen prediction and this prediction is not what is found in the Kim and Shih experiment.

The second response examined here, that of Sudbery, raised a number of issues for the experiment, some of which were also considered by Krips, that were found to either not affect Popper's argument, or to be not relevant for the Kim and Shih version of the experiment. Furthermore Sudbery's objection regarding testability can be answered by the observation that Popper is not proposing a theory but, rather a test for an interpretation of a theory.

Next, there was the response by Redhead. Some of his points concern the problem of the point source. Popper failed to see how this is fatal for his argument, but this is not a problem faced by the Kim and Shih version of the experiment. But at the same time Redhead also pointed out the issue of whether the experiment can distinguish between a theory and its interpretation, in general and in the specific case. As discussed in the preceding paragraphs, this is the central issue here.

Furthermore, the response by Collett and Loudon is again centred around the problem of the point source, whereas Peres takes for granted that there is nothing to an interpretation, over and above it being a rule that will correlate the objects of the mathematical formalism of a theory with physical quantities, and in doing so denies the possibility for a test in the first place.

9.3. The Kim and Shih Experiment

Eventually, some twenty years after its proposal, the Popper experiment was concretized by Kim and Shih. Suprisingly, the authors report that "The experimental data show $\Delta y \Delta p_y < h$ for photon 2",[447] and further that "it is astonishing to see that the experimental results agree with Popper's prediction".[448] The experiment is based on the entanglement of a two-particle state, which, because of the momentum conservation of the pair, allows the determination of the position or momentum of particle 2 from the position or momentum of particle 1 respectively. In agreement with Popper they find that when both slits are very narrow, some of the extreme counters start firing. This, as Popper thought, indicates an increase in Δp_y when Δy becomes smaller.

This apparent violation of the uncertainty principle gave rise to a string of responses that attempted to explain this within the framework of the Copenhagen interpretation. That is, they tried to interpret this as not refuting the Copenhagen interpretation. In fact the first attempt to do so is by the authors themselves. In their analysis they use the following quantum mechanical state for the pair of photons:

$$|\Psi\rangle = \sum_{s,i} \delta(\omega_s + \omega_i - \omega_p)\delta(\mathbf{k}_s + \mathbf{k}_i - \mathbf{k}_p) a_s^\dagger(\omega \mathbf{k}_s) a_i^\dagger(\omega \mathbf{k}_i)|0\rangle \tag{9.1}$$

This though is not an EPR state. It has been shown here that the wave function for a pair of particles that form an EPR state in 3-D is given by:

$$\Psi(\mathbf{r}_1, \mathbf{r}_2) = \int\int e^{i\mathbf{k}_1\cdot\mathbf{r}_1} e^{i\mathbf{k}_2\cdot\mathbf{r}_2}\, \delta\left(\mathbf{k}_1 + \mathbf{k}_2\right)\, \delta\!\left(\frac{k_1^2}{2m} + \frac{k_2^2}{m} - E\right)\, d^3\mathbf{k}_1\, d^3\mathbf{k}_2$$

$$= \int e^{i\mathbf{k}_1\cdot(\mathbf{r}_1 - \mathbf{r}_2)}\, \delta\!\left(\frac{k_1^2}{2m} - E\right)\, d^3\mathbf{k}_1$$

447 Kim and Shih (1999: 1849)

448 (*Ibid.*: 1850)

and this gives

$$\Psi(\mathbf{r}_1, \mathbf{r}_2) = -\frac{i2\pi m}{|\mathbf{r}_1 - \mathbf{r}_2|} \sin\left(|\mathbf{r}_1 - \mathbf{r}_2|\sqrt{mE}\right) \tag{9.2}$$

Kim and Shih nevertheless continue their analysis and find that the quantum mechanical prediction for their experiment gives $\Delta y \Delta p_y < h$ and that this is in agreement with their results. Redhead would interpret this as an indication that there is no problem here neither for the formalism nor for its interpretation, given that the interpretation has to interpret the predictions of the formalism. Kim and Shih claim that this is a two-particle system and not a one-particle system, and that therefore Popper should not insist on applying the uncertainty relation on what they call "conditional behaviour" that one encounters in a two-particle system. They introduce the notion of *biphoton* and suggest that if it was adopted in place of two individual photons then there would be no problem, i.e., they ask for a new metaphysics in order to avoid the problem raised by the experiment.

9.4. Reaction to the Kim and Shih Experiment

There was a number of reactions to the Kim and Shih Experiment that suggested some interpretations of the results. The most important of those were considered here previously.

Kim and Shih suggest that there is no violation, simply because the principle should not be applied to conditional measurements. They support this with the metaphysical assertion that we do not have two separate systems, i.e., two photons, but one "two photon", or "biphoton" as they call it. Since there is only one system, the uncertainties for the sub-parts have to be combined, and when they do, there is no violation of the principle. The problem is that they provide no details of how to proceed with the calculations for the bi-photon cases. It seems that their suggestion is that there should be a different interpretation of the uncertainty principle, but hety do not provide one.

Instead, not a different interpretation but a different uncertainty relation is suggested by Rigolin. Short's analysis may be correct so far as it goes, but does not deal with the Kim and Shih experiment! With Rigolin we arrive at the appropriate conclusion, that shows that the empirical predictions are in accordance with the uncertainty principle, and that an interpretation can never undermine its own minimalist assumptions.

With regard to the Kim and Shih experiment, Popper *would* want to claim that *no* lower limit applies, for his argument to work. So although his prediction is verified, and indeed in the experiment the results indicate that $\Delta y \Delta p_y < \hbar/2$, this does not work for his argument given that there is a limit, albeit a lower one than the one suggested by the usual uncertainty relations. So Popper misses his target because he misunderstands the uncertainty relation that applies in this particular case. The results do show a violation but of the wrong uncertainty relation.

Returning to the three different types of interpretations that Redhead has distinguished, all three are actually compatible with the results of the Kim and Shih experiment. If, according to a hidden variable theory the particles do have precise position and momenta, then there should be a reduction to the uncertainties when the slit is removed, exactly as the results suggest. If on the other hand the particles have fuzzy values then again they should obey some type of uncertainty relation, as they actually do. And the same applies for the Copenhagen interpretation. As it was shown by Rigolin, the interpretation only needs to say that the results should obey they uncertainty relation that follows from the formalism, not the nave uncertainty relation that Popper thinks.

So, in summary, we have concluded that Popper's analysis is at fault, but the many studies that have been made show how tantalising is the exact source of the error. We have tried to tease out the true nature of his assumptions from many conflicting accounts that have been given. Rigolin's paper was in fact the first to set the experiment in proper detail. The paper got sadly neglected, but actually solved the problems of interpretation involved.

Chapter 10: References

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