

TOWARDS AN OBJECTIVE INTERPRETATION
OF QUANTUM MECHANICS

by

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TABLE OF CONTENTS

	<u>PAGE</u>
Foreword	
<u>PART 1</u>	
CHAPTER 1 : PHILOSOPHICAL CONSIDERATIONS	
1.1 Introduction	1
1.2 The Forms of Argument	2
1.3 The Present Status of Quantum Mechanics	5
1.4 The 'Loose' Application of Q.M.	6
1.5 Non-separability and Atomism	8
1.6 On Physical Formalisms and Regulative Principles	11
1.7 Q.M. as a Physical Formalism	15
1.8 In Defence of Realism	20
1.9 Summary and Conclusion	24
 CHAPTER 2 : COMPLETENESS, LOCALITY AND THE E.P.R. 'PARADOX'.	
2.1 Introduction	26
2.2 The Formal Argument	27
2.3 Toward a Wider Definition of Completeness	32
2.4 Completeness and Determinism	36
2.5 The Conceptual Argument	39
2.6 Some Recent Criticisms	41
 CHAPTER 3 : DETERMINISM	
3.1 Introduction	44
3.2 Determinism and Causality	47
3.3 Determinism and the Function	49
3.4 Dealing with Indeterminism	50
3.5 The 'Statistical' Interpretation of Q.M.	59
 CHAPTER 4 : HIDDEN VARIABLES THEORIES	
4.1 Introduction	66
4.2 The Logical Status of Hidden Variables Theories	68
4.3 Non-locality in Hidden Variables Theories	69
4.4 The Classification of Hidden Variables Theories	77
4.5 Hidden Variables Theories of the Second Kind	79

CHAPTER 5 : LOCALITY, COMPLETENESS AND DETERMINISM : GENERAL CONCLUSIONS

5.1	Introduction	82
5.2	Non-locality : The Problem of Definition	83
5.3	The Case Against Hidden Variables Theories	90
5.4	A Complete Quantum Mechanics	98
5.5	Non-determinism and Probability	100
5.6	Non-determinism and State Reduction	104
5.7	Conclusion	109

PART 2

CHAPTER 6 : MEASUREMENT

6.1	Introduction	113
6.2	Measurement Theory, Empiricism and Realism	116
6.3	The 'Problem of Knowledge' and Classical Measurement Theory	121
6.4	The Quantum Mechanical Description of Measurement	126
6.5	Some Criticisms	135

CHAPTER 7 : STATE REDUCTION

7.1	The Concept of State Reduction	140
7.2	Criticisms	143
7.3	Ballentine's Statistical Interpretation	146
7.4	Ensembles and Single Systems	147
7.5	The System After Measurement	155
7.6	Other Objections	159
7.7	The Many Universes Interpretation	165
7.8	Dualism in Quantum Mechanics	170

CHAPTER 8 : THE ROLE OF THE OBSERVER

8.1	Introduction	177
8.2	On the Possibility of an Experimental Test	180
8.3	The 'Interfering Schrödinger's Cat'	184
8.4	Criticisms	190
8.5	Performing the Experiment	197
8.6	Interpretation	199
8.7	Conclusions	204

	<u>PAGE</u>
CHAPTER 9 : CHANGING QUANTUM MECHANICS	
9.1 Restricting the Domain of Q.M.	206
9.2 Measurement-type Interactions	207
9.3 Restricting Q.M. to Microscopic Systems	210
9.4 Macroscopic Mechanics	215
9.5 Spontaneous State Reduction	221
9.6 Toward a Criterion for State Reduction	224
9.7 Uncertainty Considerations	228
9.8 The Criterion for Spontaneous State Reduction	233
9.9 Applications	237
9.10 Criticisms	247
9.11 Conclusion	249
CHAPTER 10 : CONCLUSION	
10.1 Summary	251
10.2 Prognosis	255
BIBLIOGRAPHY	258

Foreword

This thesis is motivated by two main considerations in response to which it is divided into two parts. The first purpose, which I attempt to fulfil in the first five chapters, is to provide a systematic analysis of the relationships between those criteria for the interpretation of physical theories which are contentious in the quantum mechanical context. It is surprising that such an analysis has, to my knowledge, not been attempted previously. It seems particularly important in this field which is riddled with controversy. In the second part, (actually from the end of the first part), I abandon my 'impartial' stance and, using conclusions drawn from the first part, I formulate an objective, realist interpretation of quantum mechanics.

While the analysis of the first part, especially where it is expressed in terms of conditional propositions, is relatively clear-cut, the arguments of the second part are considerably more complicated. This is indicated by a profusion of cross-referencing and repetition. This is partly a consequence of the fact that I found it very difficult to formulate arguments which are both compelling and intellectually honest, in full awareness of their necessarily subjective character. It is also a consequence of the extremely novel and complicated nature of the subject matter. Although there is a fairly straight-forward spine to my argument, I fear that this may be partially obscured by the numerous digressions I have felt necessary. Finally, I must acknowledge that some of the complications arise because of a lack of clarity in my own mind. It is for the reader to assess whether this is justifiable in the face of the demonstrably novel aspects of physical reality as it is comprehended by quantum mechanics.

The above remarks have bearing on the note of pessimism which occurs in the final chapter. While I believe I have formulated a *valid*

realist interpretation of quantum mechanics (i.e. it is *not incorrect*), I cannot accept it wholeheartedly as *the* interpretation of quantum mechanics. This is mainly because it fails to fulfil an important, but vague criterion for the interpretation of a physical formalism: that a fully acceptable interpretation should 'ring true'. I have experienced no such revelation with respect to my interpretation or any other.

Too many people have assisted me in this work for me to mention them individually. I must thank my friends and colleagues all over the world who, by their interest in my work and by their communications and papers, have provided a rich source of encouragement. Acknowledgements are due to the C.S.I.R. for a bursary and the University of Natal for employment, during the time taken in the preparation of this work. I must thank my ex-colleagues in the Physics Department of the University of Natal in Durban for the many stimulating (if unresolved) discussions and arguments we have had over the years. It is difficult to overestimate the role played by my typist, Fiona Fletcher, in bringing this work to completion. Lastly, I am indebted to my supervisor, friend and partner-in-crime, Don Bedford, for enabling me to transcend the traditional supervisor/student relationships and work together with him as an 'equal'.

Derek Wang,
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Chapter 1

PHILOSOPHICAL CONSIDERATIONS

1.1 Introduction

One of the most disturbing general features which characterizes the literature in the field of the foundations of quantum mechanics (Q.M), and more particularly the problem of measurement in Q.M., is the existence of several competing theories or interpretations¹. This seems at first sight, to imply that 'proper' scientific method is not being applied in the treatment of these problems, due to a popular fallacy that there are uniquely defined and unambiguous criteria that all physical theories, including their interpretations must satisfy. Further, it is assumed that if a theory satisfies these criteria, then no theory which is not consistent with it will also satisfy them. If this were the case, then any controversy of this type could be explained only in terms of the incomplete or erroneous nature of the competing theories and the insufficiency of the understanding of those working in this field.

That the situation is more complicated and less clearly defined than suggested above is illustrated by the reputation of some of the physicists involved in the controversy as well as the convincing nature of their arguments, some of which we shall review below.

The historical analysis by Kuhn (1962) of the development of science provides useful insight into the nature of this controversy.

1. For a comprehensive review of this literature see Nartonis (1970).

1.2 The Forms of Argument

Briefly, Kuhn contests the idea that science develops as a gradual accretion of knowledge, always subject to the same criteria and aims. He indicates, by means of historical examples, how changes in the aims of science have accompanied major developments in the theory. For instance, prior to the introduction of Newtonian Mechanics, a theory of the motions of the planets was considered sufficient only if reasons were given for the particular planetary motions that were observed. By distinguishing between 'laws of nature' and 'initial conditions', (a particularly fruitful distinction), and embodying only the laws of nature in the fabric of the theory, Newton dispensed with these aims and introduced the new aim of discovering and formulating general laws of nature. This is a significant and famous example, yet changes in the aims of science may also be more subtle and less dramatic.

Kuhn divides scientific research into two classes. Although this classification has since been questioned, it is most helpful for the understanding of this situation.

The first, 'normal scientific research', is the activity which is pursued under an invariant (or nearly invariant) set of criteria which may include a scientific theory (e.g. classical mechanics), which is termed a 'paradigm'. This activity consists of demonstrating the applicability of the paradigm to diverse situations, both experimentally and theoretically. The paradigm is used to explain or describe as many distinct situations and events as possible.

The second class of scientific activity, 'revolutionary science' or 'paradigm change' occurs when a sufficient number of situations which cannot be explained in terms of the paradigm, or which contradict the predictions of the paradigm, have been encountered for the 'validity' of the paradigm to be called into question. Kuhn shows that a single counterexample (such as the advance in the perihelion of Mercury with respect to non-relativistic classical mechanics) need not be sufficient for this to occur.

Nevertheless, when the scientific community concerned with a particular discipline or field becomes dissatisfied with the effectiveness of their existing paradigm, they start 'casting about' for a new paradigm which renders some or all of the difficulties in the old paradigm understandable, and which provides the basis for fruitful normal scientific exploration.

An example of this activity may be seen in the development of the theory of blackbody radiation leading to the quantization of the electromagnetic field (in a sense) by Planck in 1900. On a broader scale, we may consider the events and discoveries leading to the introduction of Q.M.

Kuhn characterizes 'revolutionary science' by, among other things, the emergence of competing theories or candidates for the new paradigm, as well as a concern with the fundamentals and philosophical background of science. This latter characteristic is noted also by Körner (1957) as follows:

"When the task in hand is not the solution of problems within some conceptual framework, but rather the construction of the framework, physicists tend to use philosophical arguments" (p.97).

We note that even a superficial survey of the literature on the problem of measurement in Q.M. will reveal that these characteristics are present. See in this regard Nartonis (1970).

In order to understand how controversy can arise under these conditions we must recall that, not only theories, but also the criteria for a satisfactory theory may change in a scientific 'revolution'. As an example of this we consider the following passage due to Wigner (1967).

"... They would have .. the absurd property that two situations which are completely equivalent would develop, in the course of time, into two distinguishable situations" p.23.

This is an expression of the necessity of determinism (in classical mechanics). Nevertheless, the same author does not subscribe to hidden variables interpretations of Q.M. thereby rejecting the notion of determinism in Q.M.¹.

If the criteria for a physical theory change as a result of a scientific 'revolution', the choice of new criteria is not a well-defined procedure. If, for instance, one or other of the criteria from the old paradigm must be modified or abandoned, they must be ranked in terms of importance, and the least important criteria discarded in favour of those considered to be more important. However, the importance of a particular criterion is subject to the particular outlook of each individual or group working in the field. As a result of this, several incompatible theories may be advanced, each subject to and satisfying different criteria. The arguments favouring any one theory or interpretation must be persuasive in nature, relating to the subjective ordering of importance of the criteria.

"When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defence. The resulting circularity does not, of course, make the arguments wrong or even ineffectual.... Yet, whatever the force, the status of the circular argument is only that of persuasion. It cannot be made logically or even probabilistically compelling for those who refuse to step into the circle." Kuhn (1962) p.94.

1. See Chapters 3 and 4.

This is not to say, however, that there can never be unambiguous distinctions between such competing paradigms. In some cases, there may be experimental tests which favour one paradigm over another. Where possible, we turn to these experimental tests to provide the answer to any problems of choice between paradigms. In situations where there is no experimental basis for preferring a particular paradigm, the procedure we follow is to analyse the different paradigms in terms of some of their criteria and construct persuasive arguments for the retention of some traditional criteria and the rejection or modification of others. In this way we may hope to formulate an acceptable interpretation of Q.M. subject to a consistent conceptual basis.

1.3 The Present Status of Quantum Mechanics

Since its introduction during the first three decades of this century, Q.M. has been successfully applied to a wide range of physical situations. The predictions of Q.M. in situations inconsistent with classical theories (e.g. the photoelectric effect) and those consistent with classical theories (e.g. the interference of light) have, in every case, proved consistent with experimental results.

Quantum mechanical arguments are used in research work in many diverse fields of physics (e.g. solid state physics, optics, atomic theory) as well as in chemistry. Further, the results of Q.M. are often used without specific reference to their derivation or their origin.

Finally, this theory is now the subject of many text books, and is presented to students at an undergraduate level.

These factors are the hallmark of a thoroughly accepted theory. The absence of alternative theories (other than those which are equivalent to quantum mechanics) at least so far as the general user of Q.M. is concerned, together with the above considerations indicate that this theory provides the current general paradigm for dealing with the behaviour of microsystems (at least). How is it possible, then, that at this stage, many of the signs of paradigm change or scientific revolution are to be found in the area of the *foundations* of Q.M.? How can a theory be established and accepted when, at the same time, the so-called '*foundations*' of that theory are being subject to paradigm change?

We shall consider two related answers to the above questions. The first concerns the 'loose' nature of the application of quantum mechanics in many explanations while the second is concerned with the fact that the aims of science can be divided into two classes. We show that one class is unambiguously satisfied by Q.M., while the other may not be. (See §1.8).

1.4 The 'loose' application of quantum mechanics

At their most formal, Q.M. arguments do not provide an explanation for the behaviour of microsystems at all. They are used only to provide a description of the development of the quantum states of the system and hence to predict or account for the results of specific experiments. As we shall see, as long as we do not consider the quantum states to refer to some actual microphysical situation, no difficulty can occur. Hence, by omitting this assumption, we can avoid the problems associated with the interpretation of Q.M., and so present Q.M. arguments without controversy. The reasons for not relating Q.M. to a microphysical reality vary from the pragmatic desire on the part of text book authors to avoid controversy to the insistence by positivistic scientists (e.g. Bohr) that the concept of '*microphysical reality*' is meaningless since it is not available to '*direct*' observation.

Where attempts are made at explanation or interpretation of the structure and results of Q.M. on the basis of microphysical reality (as opposed to the macroscopic level of preparation and measurement systems) they must necessarily be suspect, since the fundamental interpretive principles on which they are based are not universally agreed upon.

We agree with Bohr to the extent that the interpretive principles of classical physics are closely akin to those of 'common-sense'. Indeed, we live on a scale at which our environment can, very nearly, be adequately described and explained using the formalism and principles of classical physics. Also, the common-sense notion of reality and that which appears in classical physics can scarcely be reckoned to be independent; each has profoundly influenced the other.

For these reasons, it has seldom, if ever, been necessary for physicists to formulate explicitly the principles on which their explanations and interpretations have been based: they are agreed upon by an appeal to 'reason' or 'common-sense'. While such formulations may have been of interest to philosophers, philosophers of science in particular, they had no place in physics per se. The controversies and paradoxes that they lead to (see e.g. Ayer (1956)) were of no direct concern to physicists. In their more philosophical moments, classical physicists could be empiricists, naive realists or even solipsists without this having any radical bearing on the physics they taught and researched.

Many physicists believe that this should be the situation today, especially with respect to Q.M. The introduction of the theories of relativity led to some changes in the classical notions of reality (especially the relativity of simultaneity) but did not give rise to any lasting controversy on a large scale. (Some controversy does, however, still exist. See e.g. Kingsley (1975)). The notion of absolute simultaneity was recognised as a mistake, which, once corrected, allowed everything to go on as before.

When this program is applied to Q.M., it entails using classical concepts and interpreting the formalism in a classical manner until difficulties are encountered. At such points, a comment on the difference between classical and quantum notions is commonly made. Alternatively, we are assured that an explanation of microphysical processes is impossible! We hope that this thesis will provide a counterexample!

This unconsidered extrapolation of classical notions into the field of Q.M., we call the loose application of Q.M. In some cases these 'explanations' can be made to appear quite reasonable since they appeal to classical concepts which in turn, are embedded in our 'common-sense'. Nevertheless, as we show in the rest of the present work, these are insufficient or inconsistent, and they cannot withstand systematic analysis.

As an interesting, if trivial, example we consider below the relation between atomism, the notion that macroscopic systems are made up of interacting microsystems whose properties and interactions determine the properties of macroscopic systems, and the Q.M. formalism. The notion of atomism is fundamental to the development of the physics leading up to Q.M. and is widely believed. Nevertheless, there are indications that atomism in its present form is not supported by the Q.M. formalism. The (implicit) use of 'classical' atomism in Q.M. is an example of the loose application of Q.M. in explanations.

1.5 Non-separability and atomism

The problem of non-separability in quantum mechanics may be presented as follows, following d'Espagnat (1971);

Consider two physical systems, U and V, which are initially non-interacting, and described by quantum state vectors $|\psi_n\rangle$ and $|\phi_o\rangle$

in Hilbert spaces $\mathcal{H}^{(U)}$ and $\mathcal{H}^{(V)}$ respectively. Let the systems interact for a finite time whereafter they become spacially separated, and no longer interact. Now, if the initial state of the composite system $(U + V)$ is given by $|\psi_n\rangle|\phi_o\rangle$ in $\mathcal{H}^{(U)} \times \mathcal{H}^{(V)}$, where \times denotes 'outer product'. Suppose that the final states of the systems are $|\psi'_n\rangle$ and $|\phi_n\rangle$ respectively. Then the time development of the state of the composite system over the time interval during which the interaction occurs is

$$|\psi_n\rangle|\phi_o\rangle \rightarrow |\psi'_n\rangle|\phi_n\rangle \quad (1.1)$$

This time development may be described by the action of a unitary time development operator, U , as follows:

$$U(|\psi_n\rangle|\phi_o\rangle) = |\psi'_n\rangle|\phi_n\rangle \quad (1.2)$$

Now suppose that the initial state of the system U is given by $\sum_n a_n |\psi_n\rangle$ where the a_n are (complex) coefficients. This is a permissible state for system U by the superposition principle. (We ignore the possibility of superselection rules in this case). Then, if we suppose that equation (1.1) holds for each value of n , the interaction in this case is

$$\sum_n a_n |\psi_n\rangle|\phi_o\rangle \rightarrow \sum_n a_n |\psi'_n\rangle|\phi_n\rangle \quad (1.3)$$

This equation follows from equation (1.1) and the linearity of the time development operator U .

Now the left-hand side of equation (1.3) is the product of a vector in $\mathcal{H}^{(U)}$ with a vector in $\mathcal{H}^{(V)}$. This is not true, in general, of the right-hand side of the equation. Unless $|\psi'_i\rangle = |\psi'_j\rangle$ or $|\phi_i\rangle = |\phi_j\rangle$ for all i and j , it is not possible to express the right-hand side of equation (1.3) as a product of vectors $|\psi\rangle|\phi\rangle$ where $|\psi\rangle \in \mathcal{H}^{(U)}$ and $|\phi\rangle \in \mathcal{H}^{(V)}$.

Hence, after interaction, we cannot give the state of system U on its own, or the state of system V on its own. We say that the two systems are nonseparable.

Quantum mechanics does not give, independently, the states of systems that have interacted in the past. It only gives the state of the composite system.

Suppose now that a third system interacts with the composite system (U + V). After the interaction, the state of this system, too, is incorporated into the non-separable state of a composite system consisting of three interacting subsystems. By induction, then, any number of systems, all of which have interacted with one or more of the other systems in the past (so that there is an unbroken 'network' of past interaction linking all of the systems) can only be described by a single non-separable quantum state, prior to any detailed measurements on these systems.

Suppose, now, that we consider the structure of a macroscopic object. Subject to the usual ideas of atomism, any macroscopic object consists of a large number of interacting microsystems (atoms or molecules). However, according to the above argument, in the absence of any microscopic measurement on the macroscopic system, or any part of it, the states of the component atoms of the system (or each macroscopically distinguishable part of the system) must be non-separable. That is, we should only deal quantum mechanically with the *whole system*. This, in turn, implies that the notion of atomism is, in some sense, incompatible with quantum mechanics.

Nevertheless, this notion of atomism is (implicitly) employed in many of the examples used to illustrate the success of quantum mechanics.

For instance, to explain the absorption spectrum of a bottle of hydrogen gas in terms of the properties of a single hydrogen atom or,

alternatively, to draw conclusions about the properties of individual hydrogen atoms from experiments on a bottle of gas, postulates the validity of the concept of atomism.

Such deductions are, therefore, inconsistent with quantum mechanics. Of course, it is possible to show that the non-separable state describing the bottle of hydrogen gas would embody the same properties as regards absorption of electromagnetic radiation as that for an individual atom. However, it is precisely the fact that such considerations are not entered into in the course of normal quantum mechanical arguments that indicates how 'conceptually loose' the usual application of quantum mechanical concepts is:

Quantum Mechanics leans heavily upon classical theories for a conceptual backing, thereby avoiding the controversy associated with its own fundamental concepts.

1.6 On Physical Formalisms and Regulative Principles

Körner (1957) defines what he calls a *physical formalism* as follows:

"A physical formalism consists on the one hand of a mathematical part or calculus. It gives rules for the formation of formulae from given signs, and for turning well-formed formulae into new ones which are again well-formed; and it selects some well-formed formulae as postulates. On the other hand, it consists of an interpretation, i.e. rules of reference which relate the signs and formulae to possible observations, in such a manner that some of the interpreted formulae express empirical laws of nature. These latter are either causal or statistical correspondences between empirical predicates. Once the general structure of physical formalisms is exhibited, their function in the achievement of conceptual economy, in prediction, and in the technical control of events is easily seen."

Belinfante (1973) voices the opinion of many scientists, including most text-book authors, when he gives the following criteria for the acceptability of a scientific theory: "Physicists call a theory satisfactory if 1). it agrees with the experimental facts, 2). it is logically consistent, and 3). it is simple as compared to other explanations." The first two criteria are just those which Körner gives as criteria for a physical formalism. The third is given to provide the theorist with a weapon for ending controversies between the adherents of competing theories: the notorious and ambiguous 'Occam's Razor'.

Many physicists would be prepared to stop here in their requirements for a physical theory. Indeed, some would even insist on stopping here. i.e. They would say that a physical theory is sufficient and acceptable, provided it is an acceptable physical formalism. One reason for this insistence is the adoption of a positivist or empiricist viewpoint, which we consider in §1.7 → §1.9 below.

Traditionally, however, the construction of a satisfactory physical formalism does *not* complete the task of constructing a physical theory. It is possible to construct a theory which satisfies all three of Belinfante's criteria, and yet which is unacceptable. This could be for many diverse reasons, e.g. It may deal with phenomena which are not traditionally within the domain of physics. It may describe all the empirical data correctly and yet embody principles which are unacceptable to the majority of physicists.

In attempting to extend and sharpen the criteria for a physical theory, we may go the way of the falsificationists (e.g. Popper(1959)) and demand that the theory be falsifiable and unfalsified. (That the theory be unfalsified is simply a restatement of Belinfante's first criterion). While this principle certainly applies, it does not do so exclusively. Also the notion of falsifiability is itself not unambiguous.

In his disturbingly coherent and well-argued thesis, Feyerabend (1975) argues that the only method for creating physical theories is 'no method', or 'anything goes'. His argument is backed up chiefly by an analysis of the 'unorthodox' methods which were used so successfully and beneficially by Galileo to assist and give credence to the 'Copernican Revolution'. However, Feyerabend's stand 'against method' is deliberately overstated, as he himself acknowledges.

His formulation disguises the fact that there are indeed criteria which must be satisfied by physical theories. 'Anything goes' may be a correct description of the methodology of a science, but it only goes if it appears acceptable to the current scientific establishment. The fact that this 'establishment' is in almost universal agreement on Belinfante's three criteria gives these criteria their prescriptive nature. As may be expected, however, other criteria exist which lack the definitive prescriptive character of the criteria for a physical formalism.

The physics 'establishment', in common with other establishments, displays a strong tendency towards *conservatism*. This attitude may be written as a principle: with respect to physical theories, *no unnecessary changes* must be made. This principle is not usually enforced prescriptively. A theory which requires too radical a departure from existing views will simply not 'catch on' and be accepted.

This has as a desirable result that the development of physics follows a continuous or nearly continuous course. Changes do not usually happen very quickly. Thus, we ensure that new physical theories are *intelligible* to at least some of the physicists who understand pre-existent accepted theories.

Conservatism has sometimes also retarded the development of our science due to over-zealous or misplaced application, and a sometimes unnecessary suspicion of new ideas. Nevertheless, too little regard for it may result in the proliferation of theories.

The path to our goal of an acceptable physical theory lies, therefore, in establishing general principles which are important in making theories intelligible to the scientific community (physics establishment). Körner (1957) calls these criteria for intelligibility *regulative principles*. They usually consist of requirements on the nature of physical reality, and, as such, cannot be entirely prescriptive. In contrast to the explicit criteria for a physical formalism given above, the regulative principles embedded in acceptable physical theories and the beliefs of important physicists are seldom stated explicitly nor are they universal. They may be statements of belief about nature (e.g. that nature is deterministic) or about theories (that theories must be complete) or they may be induced from pre-existing physical theories. By embodying these regulative principles in a new theory, we can make appear 'reasonable'.

With the introduction of a new physical formalism (such as Q.M.) into physics, we are faced with the possibility that all of the previously accepted regulative principles may not be consistent with each other subject to the new physical formalism. If no physical formalism can be found which is consistent with *all* desirable regulative principles, we must conclude that the physical situations under consideration are such that they *cannot* be explained in terms of accepted regulative principles. For instance, we show in §2.4 that the regulative principles that nature is deterministic and that physical theories should be 'complete' are incompatible in the Q.M. context. We can conclude that one of them, at least, must be discarded. In our decisions whether or not to retain regulative principles, and which of two incompatible principles we should reject, we are forced to use the persuasive arguments mentioned in §1.2. These arguments are greatly facilitated by an explicit discussion of some of the more important regulative principles, and the logical relations between them, subject to the physical formalism of Q.M.

1.7 Quantum Mechanics as a Physical Formalism

To show that Q.M.¹ provides a satisfactory physical formalism, we must show that it is logically consistent, and that it agrees with the experimental facts. It follows immediately that it is simple as compared with other physical formalisms since no comparable physical formalisms exist.

That Q.M. is logically consistent, in the sense required for a physical formalism, follows from the fact that no contradictory assumptions are made in its mathematical exposition. The so-called paradoxes relating to Q.M. are *not* formal mathematical contradictions. They demonstrate a discrepancy between an interpretation of Q.M. and certain ideas we have about the real world. i.e. They show that certain regulative principles are incompatible. This is demonstrated by example below.

The Q.M. formalism certainly agrees with the experimental facts (restricted to its domain), in as far as it can be compared with them. This can be seen by considering the insurmountable difficulties experienced by the adherents of some interpretations in formulating or finding experiments that would contradict the predictions of Q.M. As we shall see (e.g. in our treatment of hidden variables theories in chapter 4) any experimental tests which have thus far been devised have either proved to be inconclusive or else they agree with the predictions of the Q.M. formalism.

Thus, Q.M. satisfies the requirements for a physical formalism. As a corollary, we may expect that the problems and paradoxes relating

1. An axiomatic form of the Q.M. formalism is given in many text-books as well as source works.
See e.g. d'Espagnat (1971) p. 29.

to Q.M. are not formal problems, but are subject to the adoption of certain regulative principles. We show that this is the case for two such problems: the Schrödinger cat paradox and the measurement problem in general. In chapter 2, we also show that it is the case for the Einstein-Podolsky-Rosen paradox which is subject to regulative principles involving locality and completeness.

The problem of 'Schrödinger's cat' is a specific statement of a problem associated with measurement that has been much discussed in the literature. The problem is well known, and we present the argument briefly. A single photon (or its equivalent) is fired at a half-silvered mirror. If it is transmitted, the photon is absorbed by a photomultiplier tube, resulting in a signal which is used to trigger a device which smashes a phial of hydrogen cyanide. If the photon is reflected, it is harmlessly absorbed. Now the quantum state for a photon which has encountered a half-silvered mirror can be written

$$\frac{1}{\sqrt{2}} (|R\rangle + |T\rangle)$$

where $|R\rangle$, $|T\rangle$ are eigenvectors corresponding to finding the photon in the 'reflected path', and the 'transmitted path', respectively. If the poisoning device is sealed in a 'black box' together with a cat, and if the photon is transmitted, the cat will be killed. If it is reflected, the cat will remain alive. The problem is then, if the photon is neither transmitted nor reflected, but proceeds in a 'superposition of these states', we may causally suppose that the cat is 'superposed' alive and dead, at the end of the experiment. On the other hand, all cats that we have ever seen are either alive or dead.

This problem is not a paradox relating to the physical formalism of Q.M. and this can be seen as follows: The time-development of the 'cat and trigger' system may be written as follows:

$$2^{-\frac{1}{2}}(|R\rangle + |T\rangle)|B\rangle|A\rangle \rightarrow 2^{-\frac{1}{2}}(|R\rangle|B\rangle + |T'\rangle|B'\rangle)|A\rangle$$

$$\rightarrow 2^{-\frac{1}{2}}(|R\rangle|B\rangle|A\rangle + |T'\rangle|B'\rangle|D\rangle)$$

where $|B\rangle$, $|B'\rangle$ are the 'unfired' and the 'fired' states of the trigger, and $|A\rangle$, $|D\rangle$ are the states 'alive' and 'dead' for the cat.

The final state must be interpreted: "On measurement, the cat will be found to be dead and the triggering system 'fired' with a probability of $\frac{1}{2}$ ". This is because the eigenstate corresponding to this situation has amplitude $2^{-\frac{1}{2}}$, and is orthogonal to the other term. (i.e. no interference terms are expected).

This is precisely what is found. That is, the empirical prediction of the Q.M. formalism in this case is correct. If the experiment is performed many times, in approximately half the cases the cat will be found to be dead, and in the other half of the cases, it will be found alive.

The difficulties alluded to by Schrödinger arise when we assume that, corresponding to a given quantum state, there is an actual physical situation, and that the principle of superposition applies to actual physical situations. Further, this only constitutes a paradox if we then assume that all cats are either dead or alive at all times (i.e. that the states 'dead' and 'alive' for a cat are in all ways mutually exclusive) or else that no non-local effects occur: the cat cannot change from being, in some sense, both dead and alive to being either one or the other when, say, a conscious observer opens the box. The discussion proceeding from this problem is the problem of measurement as it is manifest in a specific example. We curtail this present discussion, having shown that the problems relating to Schrödinger's cat are subject to the assumption of several regulative principles.

We move now to a more general discussion of the problem of measurement. The problem of measurement in Q.M. relates to the fact that the quantum mechanical description of the processes of measurement cannot, in general, yield a state which corresponds to a single, unambiguous measurement result (like 'alive' or 'dead' concerning a cat). This is in contrast to the fact that, at least in studied scientific experiments, we *always* observe unambiguous measurement results i.e. we never see cats which are 'both' dead and alive or particles which are, in any sense, in more than one place at one time. This problem is subject to the adoption of very many regulative principles. They are too numerous (and perhaps impossible) to name, since they include the assumptions about reality that occur in the 'common-sense' usage of language. However, we name some of them below.

- i). There is a 'physical reality' which exists independently of any observation (although its mode of existence may be changed by observations).
- ii). We can have knowledge of this physical reality.
- iii). Our perceptions occur as a result of physical (and chemical) interactions between world and our bodies (which, too, are parts of the real world).
- iv). All interactions in the real world, including those above, are describable by physical theories, and Q.M. in particular.

And so we may go on.

However, we note that none of the above principles (i) - (iv) is analytic. None is logically related to, or necessary for the construction of a satisfactory physical formalism. Any or all of them may be rejected as a means of avoiding the problems their acceptance poses for Q.M. Heisenberg has frequently been quoted in support of the notion that Q.M. is a theory which is concerned with describing our *knowledge* of physical systems and *not* the physical

systems themselves. Indeed, if we take the empiricist (or positivist) viewpoint (i.e. that the purpose of science is to order, categorize and correlate our sense impressions, where these are considered the prime epistemic objects, and the only referents of physical theories) all the above observations become either meaningless sentences or else false propositions. From this point of view any theory of measurement will be meaningless since it will be concerned with the relationship between measurement results (either as the actual situation pertaining to measuring devices, or as sense impressions) and the actual state of affairs pertaining to a real (object) system. A theory of measurement involves the explanation of how measurement results are obtained. In a logical development this must be done prior to any considerations involving the measurement results themselves. From the empiricist viewpoint, the measurement results are considered to be primal and hence any theory of measurement must be both unnecessary and meaningless.

If there is no necessity, indeed no possibility, of a theory of measurement, then the problems associated with measurement cannot occur. i.e. The assumption of an empiricist stance solves the problems of measurement in Q.M. (or any other theory) trivially. More generally, from the empiricist viewpoint, all the requirements of a physical theory, i.e. that it should categorize and correlate our experience, are satisfied by a physical formalism. As we have shown, Q.M. provides a satisfactory physical formalism. Hence, *from the empiricist point of view, Q.M. is an entirely satisfactory physical theory.* The problems of interpretation and of measurement in Q.M. are trivially solved, since they *do not exist* in that their formulation is in terms of concepts which are meaningless.

As a corollary to this result, it is inconsistent to use empiricist arguments in order to provide anything other than a trivial solution to any of the problems of interpretation of Q.M. For example, to provide a 'solution' to the Einstein Podolsky Rosen paradox by invoking empiricist attitudes is inconsistent unless the whole problem is assumed to be 'meaningless'.

If the problems of Q.M. can be rendered trivial in this manner, it becomes important and meaningful to ask why the problems exist at all. i.e. What is wrong with an empiricist or positivist philosophy in relation to physics? Indeed, many physicists and philosophers of science have argued, since even before the situation in Q.M. was discovered (or created), that the empiricist standpoint provides the only possibility for 'a physics without metaphysics'. In order to answer this question, we consider the account of the aims of science by d'Espagnat.

1.8 In defence of realism

The division of the criteria for a satisfactory physical theory into those prescriptive criteria for a physical formalism, and the inductive criteria for an 'interpretation', is seen by d'Espagnat (1971) as a division in the aims of science.

"If the reader provisionally tolerates oversimplification, he will presumably accept the assertion that science has two purposes. One is to organize our perceptions and thereby enhance our power. The other is to understand the world at large and our relation to that world" (Preface).

He notes further that the first aim mentioned above has been emphasised during this century. We might add that this would be likely during the initial stages of any period of major upheaval in a scientific discipline, since the first aim is limited to the provision or construction of a physical formalism only. The deeper problems of explanation and interpretation relating to the second purpose can be ignored while this primary task, difficult in itself, is carried out. Thereafter, it follows in an extremely natural fashion from the scientific tradition that we should use the correlations provided by the physical formalism to investigate and explain the nature of the physical systems under consideration.

Many scientists (e.g. von Neumann) do not distinguish between the two processes, and, having set up a physical formalism or part thereof, proceed immediately with an interpretation. If the suspension of the second purpose is embraced as a positive doctrine rather than seen as a temporary oversight in the face of the more immediate difficulty of constructing a formalism, we find ourselves running against the scientific tradition which requires that we not only describe and correlate our observations, but that we explain them as well. This explanation is most commonly formulated in terms of the properties of the 'real world' or 'physical reality' and involves adopting a *realist* position. A rejection of the second purpose of science is tantamount to assuming an empiricist philosophy which, as we have seen, and will show below, leads to certain physical (and philosophical) problems.

This is not the place to enter into a deep philosophical discourse on the consequences of an empiricist viewpoint. This has been discussed elsewhere by experts. Bunge (1973) has formulated convincing arguments as to why this standpoint is unacceptable as a philosophy of physics. We simply wish to make some remarks which may indicate to the reader that the adoption of empiricism has some unsavoury consequences for science.

Our first objections relate to the common usage of 'realist' language and concepts in science. For an empiricist, terms such as force, electric field, particle etc. are meaningless except as collective names for certain sets of experiences. However, we note that in by far the majority of literature on physics (i.e. virtually all the literature not concerned with problems relating to the philosophy of science) these terms are used in the same manner as the terms 'cup' and 'table' are used in everyday life. i.e. They are assumed to have meaning because they have referents which exist as part of 'physical reality' and not as the sort of experiential shorthand mentioned above. This shows that, independently of the conclusions they may arrive at whilst doing philosophy, most physicists are realists while they are doing physics. This is born out further

by the fact that the special theory of relativity is considered to be due to Einstein, and not Fitzgerald, Lorentz or Poincaré. The physical formalism of special relativity had been more or less completely developed by the latter scientists. All that Einstein, a thorough-going realist, did that was new was to provide an interpretation and an explanation for the Lorentz transformation. This demonstrates that at least during the early years of this century, the fulfilment of the second purpose of science was considered to be a necessary prerequisite for a physical theory.

As we have noted, to reject the second aim of science goes against the scientific tradition as well as the actual practice of physics by most physicists. It also goes against the ideas about physics held by many physicists and laymen alike. Explicit arguments in defence of realism are to be found in the work of many scientists, e.g. Born (1956), Landé (1965), d'Espagnat (1971). Moreover, certain views which are at variance with empiricist physics are held by many physicists independently of their philosophical persuasion. For example, most pure scientists such as physicists would deny hotly that there is, in principle, no distinction between the 'pure' sciences and the technological disciplines. However, if science is restricted to the first purpose by the adoption of empiricism, its only function is to organize our perceptions and thereby enhance our power. This, surely, is the precise aim of such technologies as engineering. The empiricist can have no reasonable motivation for investigating domains which seem unlikely to yield any manipulative advantage with respect to our lives. The realist, on the other hand, can be motivated in such a case by a desire to understand his/her environment. i.e. by the second purpose of science.

By recognizing that the problems of interpretation of Q.M. are indeed non-trivial, many physicists (implicitly) affirm their belief in the second purposes. Having done so, as we have pointed

out in §1.7, it is thereafter inconsistent to 'solve' specific problems by the temporary adoption of empiricist concepts. Thus, for Heisenberg who asserts that Q.M. deals with our knowledge of physical systems only, the problems of interpretation of Q.M. should not exist.

Consider also the fact that empiricism is closely related to the extremely subjectivistic doctrine of positivism, since it is asserted in both that propositions relating to anything other than immediate sense-experience are meaningless. A possible distinction lies in the fact that for the empiricist such propositions are only meaningless with respect to science whereas for the positivist they are more generally so. Scientifically at least, there is no direct observation of which the proposition "He/She is conscious" can be rendered meaningful. Thus, from the point of view of the positivistic scientist, the notion of the consciousness of others is meaningless. Despite strong assertions to the contrary from the adherents of such doctrines, this implies that scientific positivism is equivalent to solipsism. While it is true that physical science does not usually deal with consciousness explicitly, it would be disturbing, to say the least, if the notion of the consciousness of others were meaningless in terms of its underlying philosophy, particularly since physics is a social or group activity.

Finally, we note that in the past, the choice between empiricism and some kind of realism has always been possible, if unimportant. There have always been realistic interpretations of physical theories. It is interesting from the philosophical point of view, that the problems of interpretation of Q.M. have brought this choice to the fore. The problem of knowledge, which gave rise to the doctrines of positivism and empiricism in the first place, has traditionally been the sole preserve of philosophy, having no place in physics, per se. Now this problem and the related choice between empiricism and some kind of realism has become of vital importance regarding the interpretation of Q.M. and, as such, it falls into the domain of physics. (See §1.7).

In conclusion, it seems to some extent absurd that the second purpose of science and the realism concomitant with it needs to be defended at all. The adoption of empiricism serves only to provide a means of avoiding problems that occur relating to scientific concepts as they are *actually used*. This is done at the cost of asserting that scientific propositions, concepts and language do not mean what the majority of scientists think they mean. Nevertheless, the choice between empiricism and realism remains, logically, an arbitrary one. We can, if we wish, *prescribe* the empiricist conditions for a physical theory and leave it at that. In doing so, we would not only be changing the traditional purpose of science. We would also discard as meaningless some of the most interesting epistemological and ontological problems that have occurred in physics for many years; viz. those involved in answering the question: "What is the nature of microphysical reality, as revealed to us by the physical formalism of Q.M?"

1.9 Summary and Conclusion

To summarise, the existence of competing interpretations is not necessarily the result of a misapplication of 'proper' scientific method, but may occur during periods of paradigm change or scientific revolution. While Q.M. appears, at one level, to be a fully accepted theory, paradigm change can still be going on.

Firstly Q.M. is usually applied to problems in a conceptually 'loose' way, employing the concepts of classical physics and the formalism of Q.M. Secondly, while the Q.M. formalism provides a satisfactory 'physical formalism' it does not provide an interpretation subject to generally accepted regulative principles. In this sense, it does not satisfy the second aim of scientific theories. The problems of interpretation of Q.M. are trivially solved if we neglect this second aim, but this also involves adopting an empiricist standpoint, or at least rejecting realism.

The preceding analysis sheds some light on the way ahead: we have seen that the problems in the interpretation of Q.M. are 'physical' and not 'formal' in content. Hence, we cannot expect to solve these problems by a formal analysis. A physical or interpretive problem must be dealt with in a physical or conceptual way.

Although, as we have seen, the choice of regulative principles is subject to persuasive arguments only, we may establish conclusive results by finding out which regulative principles are consistent and which are inconsistent, subject to the Q.M. formalism. The main motivation for the first part of this thesis is that such an analysis is, as yet, lacking in the existing treatments of the problem. This has had the unfortunate result that in some cases, attempts have been made to satisfy simultaneously regulative principles which are inconsistent, given the Q.M. formalism. In other cases, the persuasive arguments in favour of a certain interpretation have been facilitated by neglecting to mention that some particularly cherished regulative principle must be dispensed with. (e.g. the principle that interactions must be local, in the interpretation due to Ballentine (1970) and to Landé (1965)).¹

It has been possible for this situation to come about because it is acceptable for the authors of scientific papers not to state explicitly their choice of regulative principles. Where these choices have been made explicitly, they are often simple statements of belief.

While we do not claim that the specification of regulative principles is necessary (after all, this is physics and not semantics) or even possible, it will be seen that, by considering a limited number of such principles when they appear to be important, we can achieve results which exclude some of the currently viable interpretations, as well as making it easier to choose from those which remain.

1. See §3.5.

Chapter 2

COMPLETENESS, LOCALITY AND THE E.P.R. 'PARADOX'¹2.1 Introduction

The first regulative principle which we consider in detail is that a physical theory should be 'complete'. This issue was raised initially in 1935 by Einstein, Podolsky and Rosen (E.P.R.), but is still receiving attention in recent contemporary literature.² The work of E.P.R. is well known, and in §2.2 we present only a brief summary of their argument, mainly to introduce a particular example first presented by Bohm (1951). The conclusion of the E.P.R. argument is modified from "Q.M. is not complete" to read "either Q.M. is not complete, or else non-local effects must exist". E.P.R. simply assume that non-local effects cannot exist under any circumstances whereas we shall see (in §5.2, 5.3) that this may not be the case.

In §2.3, by an analysis in terms of classical concepts, we extend the concept of completeness to give a *definition* of the term (as opposed to the *condition* given by E.P.R.). This will facilitate the explicit analysis of the relationships between the concepts of completeness, locality and determinism that follows (§2.4 et seq). We consider that our definition of completeness is in accordance with the intuitive ideas implicit in the work of E.P.R. and others.³ In any event we show in §2.5 that the same conclusion (i.e. that Q.M. is complete or else non-local effects occur) holds also for our definition. Moreover, in §2.4 we show that the regulative principles of completeness and determinism are inconsistent with respect to any stochastic theory (particularly Q.M.). This important result, although it is virtually self-evident, indicates a possible motivation why Einstein, a staunch determinist, should be concerned with completeness.

1. We take this opportunity to remind the reader that, as in the 'paradox' of Schrodinger's cat, the E.P.R. argument is not a paradox resulting from any logical inconsistency in the physical formalism of Q.M. alone.
2. See e.g. Mirman (1973), Capri (1975), Moldauer (1974) Faraci et al (1974). For more comprehensive treatments, see d'Espagnat (1971) and Scheibe (1973).
3. See in this regard, Scheibe (1973)

The notion of non-locality is, as we shall see, fundamental to a discussion involving the E.P.R. argument. However, it is also important in the analysis of hidden variables theories. For this reason, we apply the concept in much the same intuitive way as do most authors. We postpone an explicit discussion of the concept until §5.2.

In §2.6 we deal with some of the more recent criticisms of the E.P.R. argument.

2.2. The Formal Argument

E.P.R. consider that "every element of reality must have a counterpart in the physical theory" if that theory is to be complete. They note that this necessary condition may not be sufficient for completeness. An element of reality is outlined as follows:

"If without in any way disturbing a system, we can predict with certainty (i.e. with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this quantity."¹

They then show that if there is an element of physical reality corresponding to a quantity represented by Hermitian operator A, then there is no element of physical reality corresponding to the quantity associated with Hermitian operator B, if A and B do not commute (or else Q.M. is not complete).

1. Moldauer (1974) has pointed out that the notion of physical reality can be eliminated between these two conditions. However, this renders the meaning of the term 'completeness' even more obscure.

To illustrate this¹, consider a microsystem S, which is prepared in a quantum state described by the normalized state vector $|\psi\rangle$ in the Hilbert space \mathcal{H} associated with the system. Suppose further that $|\psi\rangle$ is an eigenvector of a Hermitian operator A.

$$\text{i.e.} \quad A|\psi\rangle = a|\psi\rangle \quad 2.1$$

where a is a real number, and A is associated with a property (observable) of the system S.

We interpret this as follows, according to the physical formalism of Q.M.: we can predict the result of a measurement of the property (observable) associated with A with certainty (i.e. the value of the property is given by a). Hence, there is an element of physical reality associated with the property associated with A (bearing in mind that the associations represent one-to-one correspondences).

Suppose now that B is a Hermitian operator in \mathcal{H} such that B does not commute with A. Now, by a well known theorem of Q.M., $|\psi\rangle$ is not an eigenvector of B. i.e. there is no real number b such that

$$B|\psi\rangle = b|\psi\rangle \quad 2.2$$

In general, $|\psi\rangle$ is a linear combination of (normalized) eigenvectors $|\phi_n\rangle$ of B in

$$\text{i.e.} \quad |\psi\rangle = \sum C_n |\phi_n\rangle \quad 2.3$$

1. E.P.R. provide illustrations of the formalism in terms of momentum and position operators in the argument leading to propositions (1) and (2) below. This example serves only to show that there are physically meaningful quantities associated with non-commuting operators.

where the $\{C_n\}$ are (complex) coefficients with $C_n \neq 0$ for at least two values of n and

$$B|\phi_n\rangle = b_n|\phi_n\rangle \quad 2.4$$

where b_n is a real number.

It follows from the immediate interpretive rules of the Q.M. formalism that the result of a measurement of the quantity associated with B will be b_n with probability $|C_n|^2$. Hence there is no element of physical reality corresponding to the property associated with B . From these considerations E.P.R. conclude that either

- "(1) the quantum mechanical description of reality given by the wave function is not complete or
- (2) when the operators corresponding to two physical quantities do not commute, the quantities cannot have simultaneous physical reality."

Since, in general, the information obtainable with certainty from a quantum state corresponds exactly to what can be predicted without changing the state of the system, i.e. when the system is prepared to have a suitable eigenstate, we might suppose that the quantum mechanical description of reality may be complete. However, following E.P.R. and Bohm (1951), we shall describe a counter-example, subject to the assumption that non-local effects do not exist.

We consider the specific case of a spin-zero particle which decays into two electrically neutral particles, each with spin $\frac{1}{2}$, by a spin-conserving decay. By a system of shutters, we select only those particles which travel in opposite senses along a certain direction. (If the experiment is performed in the center-of-mass rest frame of the initial particle, the decay products must move in opposite senses in order to conserve momentum).

Suppose that the two subsystems corresponding to the decay products are U and V, associated with Hilbert spaces $\mathcal{H}^{(U)}$ and $\mathcal{H}^{(V)}$ respectively. Suppose also that $|u_{z+}\rangle$, $|u_{z-}\rangle$ and $|v_{z+}\rangle$, $|v_{z-}\rangle$ are the eigenvectors of the (Pauli spin) operators associated with the components of spin in a direction transverse to the motion of the particles (the z direction) for systems U and V respectively.

The state vector for the system at any time after the decay (at least) and prior to any measurement on either system must be given by the anti-symmetric singlet state

$$|\psi\rangle = 2^{-1/2}(|u_{z+}\rangle|v_{z-}\rangle - |u_{z-}\rangle|v_{z+}\rangle) \quad 2.5$$

where $|\psi\rangle \in \mathcal{H}^{(U)} \otimes \mathcal{H}^{(V)}$, the outer product Hilbert space¹.

Now suppose that an ideal measurement of the spin component in this direction is made on system U, and the result "spin up", the 'eigenvalue' corresponding to the eigenvector $|u_{z+}\rangle$, is found. By the conservation of angular momentum (or else by using the 'Projection Postulate' of von Neumann (1932) as part of the physical formalism) we can predict with certainty that a measurement of the component of spin in the z direction on system V would yield "spin-down". According to the projection postulate, we assert that the state of system V is independently given as $|v_{z-}\rangle$.

Suppose that, instead, we measure the spin components in the x direction, which is normal to both the z direction and the direction of motion of the particles. Suppose that the eigenvectors of the operator associated with this measurement are $|u_{x+}\rangle$ and $|u_{x-}\rangle$.

1. Note that this state-vector is 'nonseparable' (d'Espagnat (1971)) or 'of the second kind'.

It is a well known result, formally embodied in the properties of the Pauli spin matrices that the operators corresponding to spin component measurements in distinct directions do not commute. The following relationships hold between the eigenvectors of these two to non-commuting operators.

$$\begin{aligned} |U_{z+}\rangle &= 2^{-1/2}(|U_{x+}\rangle + |U_{x-}\rangle) \\ |U_{z-}\rangle &= 2^{-1/2}(|U_{x+}\rangle - |U_{x-}\rangle) \\ |V_{z+}\rangle &= 2^{-1/2}(|V_{x+}\rangle + |V_{x-}\rangle) \\ |V_{z-}\rangle &= 2^{-1/2}(|V_{x+}\rangle - |V_{x-}\rangle) \end{aligned} \quad 2.6$$

Now, substituting from 2.6 into 2.5, we get

$$|\psi\rangle = 2^{-1/2} (|U_{x+}\rangle|V_{x-}\rangle - |U_{x-}\rangle|V_{x+}\rangle) \quad 2.7$$

If we measure spin in the x direction on U and obtain the result 'spin left' corresponding to the state $|U_{x+}\rangle$ we can predict with certainty that a measurement in the same direction on system V will give the result 'spin right'. (According to the projection postulate, in the case of ideal measurement, system V should be described by the state vector $|V_{x-}\rangle$).

Thus, if we perform a measurement (of spin component) in the x-direction on U, we can predict, with certainty, the outcome of such a measurement on V. Alternatively, if we perform a measurement on U in the z-direction, we can predict with certainty, the result of a measurement in the z-direction on V. Now, although measurements of spin component in the x and z directions cannot be performed simultaneously, E.P.R. argue that the actual physical situation pertaining to system and hence the results obtained by measurement on V, cannot be affected by the direction in which we choose to measure the spin component on system U. This is because these measurements (on systems U and V) can take place as far apart spacially, and as close together temporally as we like. Any such interaction (involving events separated by a space-like interval, in the terminology of special relativity) must be non-local, (see §5.2) and the possibility

of non-local interactions is denied by E.P.R.

Thus, the results of two measurements of observables corresponding to two non-commuting operators can be predicted with certainty on the same system in the same actual physical situation and so each of these quantities have simultaneous physical reality.¹ This is in contradiction with proposition (2) above. Hence proposition (1) must follow i.e. the Q.M. description of reality is not complete.

2.3 Towards a Wider Definition of Completeness

E.P.R. provide only a condition for the completeness of a physical theory. However well-suited this condition maybe to the particular argument presented by E.P.R., it does not situate the concept within a wider semantic context, except by intuitive implication. Now, as Hooker (1973) has pointed out, and as we showed in Chapter 1, the problems of interpretation of Q.M., specifically the E.P.R. paradox, are not *formal* but *physical*. That is, the difficulties raised by E.P.R. are not apparent as a contradiction in the formalism of Q.M. (i.e. as a paradox). They only occur subject to certain assumptions about the nature of physical reality (e.g. that physical interactions are local) and the nature of the description of that reality by a theory (i.e. that it must be complete). An argument couched in purely formal terms cannot be useful in this instance since there is no problem or paradox on a purely formal level. For this reason we attempt an analysis of the 'physical' concepts involved in the argument.

1. Some authors (e.g. Moldauer (1974)) have observed that the results of spin component measurements in different directions *cannot* be predicted simultaneously. See §2.6.

This is another example of the fact, referred to in §1.6, that problems associated with Q.M. only arise subject to some sort of realist point of view, and are non-existent or trivial from an empiricist (operationalist, idealist, positivist) viewpoint. If we accept the realist second purpose of science, the following questions become legitimate and meaningful.

What is the nature of the reality which Q.M. has been constructed to describe or explain? To what extent is Q.M. a successful description of this reality?

We note that the problems posed by these questions are by no means trivial, particularly in the case of microsystems, which are not accessible to simple observation and hence not part of our 'common-sense reality'. Even in the case of macrosystems, their description in terms of classical mechanics is not simply determined by our common-sense notion of their objective properties. There is, rather, a dialectical interdependence between the two concepts. Our common-sense notion of the reality of microsystems is even more dependent on the descriptive theory in the case of Q.M. 'Empirical data' can only be interpreted in terms of a theory of measurement. Hence, it becomes non-trivial matter to decide whether or not a theory provides a sufficient account of reality at the microscopic level. Nevertheless, this 'completeness' of a theory may be investigated with the assistance of some regulative principles concerning the nature of the relationship between a theory and the 'reality' which it describes.

Finally, before we begin our investigation, we draw attention to the fact that the choice of the adjective 'complete' to describe a satisfactory relationship between a theory and the reality with which it deals, is not particularly apt. Since the failure of the 19th Century scientific optimism, scientists have become justifiably wary of claiming that science is able to provide a complete (in an unrestricted, general context) description of physical reality.

Also, in the sense that Q.M. is likely to be superceded, at some future time, by a new theory which will treat, among other things, the same aspects of reality, it would be presumptuous to call it 'complete', no matter how suitable a description we may find it to be at present. The term 'complete' should be viewed in the limited sense of describing a theory in which the relationship between that theory and physical reality is satisfactory according to certain specific criteria, or regulative principles.

In attempting to discover whether or not Q.M. gives a sufficient account of the systems with which it deals, it is instructive to consider whether a state vector in a Hilbert space is sufficient to characterize a single actual physical situation or whether it necessarily describes any one of an ensemble of *essentially* different actual situations. In the former case we will say Q.M. is complete, and in the latter that it is not. More generally, we may define *completeness* as follows:

If a 'state' in a physical theory is sufficient to identify exactly one physical situation (or identical copies of it) as opposed to any one of an ensemble of *essentially* different situations, that physical theory is complete. Otherwise, it is not complete.

This definition of completeness seems to be in agreement with the implicit ideas of E.P.R., as well as the expressed view of Einstein who states, for example, that the aim of physical theories is "the complete description of any (individual) real situation (as it supposedly exists irrespective of any act of observation or substantiation)" (Einstein (1949) p667). Here the word 'individual' indicates that Einstein would support our formulation. We show, in any event, that the same conclusions as those of E.P.R. follow from our formulation. Scheibe (1973) notes (on p 174) that "incompleteness is to be demonstrated on the foundation of classical realism". We assume that, under optimum conditions (i.e. with respect to phenomena within its domain) classical mechanics provides a model for completeness.

By an analysis of classical mechanics with respect to our definition of completeness, we come to a precise formulation of what is meant by "essentially different" in our definition.

Classical mechanics, as it is used in the explanation of its problems, comprehends ensembles of actual physical systems which are not identical. For instance, in the description of planetary motion, an ensemble of solar systems is dealt with, since the detailed composition of the sun and the planets is not usually considered. The classical description of planetary motion applies to an ensemble of solar systems, each similar to our particular one, but differing with respect to the detailed composition (e.g. colour density, population, whether I am sitting or standing etc.). The elements of such an ensemble are considered essentially identical since these factors have no influence on the gross motions of the planets about the sun.

It may be argued that, in principle, it is possible to specify all of these properties in terms of classical mechanics, and although this would complicate the problem far beyond the limited purpose of explaining the gross motions of the planets, it would ensure that the description thus given would correspond to exactly one solar system or identical copies of it. However, we note that point particle classical mechanics deals, formally, only with mass points which have neither size nor internal structure. This can be generalized to a theory which deals with extrinsic properties (including size) and not intrinsic properties (the internal structure). Such a theory cannot be used to treat electromagnetic radiation, as indicated by the failure to detect an 'aether' in, for instance, the Michelson-Morley experiment. Nevertheless, in a universe without electromagnetic radiation, for an exhaustive description in terms of classical mechanics to be meaningful, we require that matter consist, ultimately, of point particles with no internal structure. It is doubtful whether such an assumption could be considered as reasonable, even if microsystems had been found to behave classically.

To postulate the existence of fundamental ontic atoms, which have no internal structure whether or not it is observable, is no less presumptuous than the postulate that atoms (in the sense of Dalton's atomic theory, say) should be indivisible.

Hence, in every application of classical mechanics, we assume that the internal structure of some of the constituents of the system under consideration has no effect on the properties in which we are interested. The classical description therefore treats an ensemble of systems, the elements of which differ by the internal structure of the ontic atoms, since the only requirement we place on that internal structure is that it be negligible.

If, however, the internal structure of the atoms is negligible, from the point of view of the description, the elements of the ensemble, though not identical 'in fact', are *essentially* identical.

2.4 Completeness and Determinism

The next question to be considered is how we are to test for the essential equivalence of the elements of an ensemble. In deterministic theories (such as classical mechanics) if the elements of an ensemble are initially equivalent subject to a certain description, they will remain so throughout any time interval under consideration. This follows immediately from the requirement that the properties which distinguish different equivalent elements of an ensemble have a negligible effect on those properties of the system which are under consideration. Therefore, if the properties of the system under consideration are found to be different for different elements of the ensemble during the time interval for which a description is required, we must conclude that these elements were not initially essentially equivalent, and hence that this description of the situation is not complete. We can remedy this by including more properties (relating to the 'internal structure' in the first, incomplete description) so as to specify an ensemble of

essentially identical systems. This process of rendering the description complete corresponds to the art of isolating the dependent variables in a given situation¹. The criterion that the ensemble of systems comprehended by a particular description in terms of classical mechanics consists of essentially identical elements (i.e. that the description is complete) is that the time development of each element of the ensemble be essentially identical. Hence, we may test for the completeness of a particular classical description by considering a (real) sequence of actual physical situations which have the same description and observing whether their behaviour remains the same (in as far as the properties under consideration are concerned) in each case.

We note that this procedure applies only to the description of systems the behaviour of which is inherently deterministic. In a system which is non-deterministic, *we expect* situations which are initially identical (or essentially identical) to develop into different situations (on different occasions). See §3.4.

Now, with respect to microsystems (in the domain of Q.M.), systems which are initially described the same quantum state develop, in general, into non-identical situations, at least after measurement has occurred. This is illustrated by the statistical distribution of (mutually exclusive) results obtained from similarly prepared microsystems, as embodied in the statistical predictions of the quantum mechanical formalism. Thus, if we assume that the behaviour of actual physical systems is necessarily deterministic, the description of this behaviour in terms of Q.M. must be incomplete, according to our definition of completeness. Clearly, this result can be generalised to apply to any stochastic theory:

1. In the context of the next chapter, this process corresponds also to 'splitting the cause'. See §3.4.

With respect to stochastic theories (e.g. Q.M., classical statistical mechanics) the regulative principles that a physical theory should be complete, and that actual physical systems (isolated if necessary) develop deterministically in time are incompatible.

This result is in accord with classical notions where classical statistical mechanics (a stochastic theory) is regarded as incomplete since, classically, reality is considered to be deterministic (as is shown, for example, in Newton's laws of motion).

This provides a possible reason why Einstein, whose basic prejudice in favour of determinism is well known¹, should concern himself with matters relating to completeness. viz If the behaviour of 'reality' is regarded as fundamentally deterministic, then Q.M., a stochastic theory, cannot be regarded as complete. Alternatively, if Q.M. is shown to be incomplete, then a deterministic theory of the behaviour of microsystems may be possible.

A study of the so-called 'hidden variables theories' (H.V. theories) will be particularly useful since it will reveal the conditions under which we may regard the behaviour of microsystems as being deterministic. Further, if we conclude that microsystems do indeed behave deterministically, then H.V. theories will also provide a means of distinguishing the non-equivalent elements of the ensemble described by an incomplete quantum mechanical state (i.e. by means of the 'hidden' variables themselves). By specifying the quantum state as well as the values of these 'hidden' variables, we would be able to specify an essentially unique actual situation, and the theory dealing with these new 'states' would be both complete and deterministic. In this regard see Chapters 4 and 5.

1. This is clear from many statements made by Einstein throughout his career. See especially Einstein (1949). His most famous comment in this regard was that he did not believe that God plays dice with the Universe!

2.5 The Conceptual Argument

To show that the concept of completeness which we have defined and discussed above is at least consistent with the (implicit) notions of E.P.R., we show that the same result, i.e. that Q.M. is not complete or non-local interactions exist, follows from our definition. Consider again the example of the decay of a 'spin-zero' particle into two 'spin-half' particles. The quantum state of this system is given by

$$|\psi\rangle = 2^{-1/2} (|U_{z+}\rangle |V_{z-}\rangle - |U_{z-}\rangle |V_{z+}\rangle) \quad 2.5$$

The eigenstates corresponding to the results 'up' and 'down' for systems U and V occur 'symmetrically' in the state $|\psi\rangle$. i.e. No information which singles out a result 'up' or 'down' for each particle occurs explicitly in the notation. In addition, $|\psi\rangle$ can also be expressed as follows:

$$|\psi\rangle = 2^{-1/2} (|U_{x+}\rangle |V_{x-}\rangle - |U_{x-}\rangle |V_{x+}\rangle) \quad 2.6$$

where x is any direction. i.e. The quantum state $|\psi\rangle$ is symmetrical with respect to any change in the direction in which spin components are measured.

Thus, no particular direction or sense for the spin component of either particle is singled out by this quantum state. Hence, if Q.M. is complete in the sense discussed above, the systems (particles?) described by this quantum state must 'have' unpolarized spins, in some sense, if they 'have' spins at all.

Suppose now, that, when the decay products are well separated spatially¹, a measurement is performed on one of them, and its spin component is found to be 'spin up' (down) in the z direction.

1. That this can occur has been contested. See §2.6.

Any spin component measurement performed hereafter on the other system will yield results compatible with the assumption that this particle 'has' 'spin down' (up) in the z direction. That is, after a measurement has been performed on the system, the symmetry with respect to sense and direction of spin component for the other particle is immediately broken. Since we have assumed that Q.M. is complete, information as to the spin polarization of the second particle is *not simply unknown* prior to any spin measurement, *it does not exist*. Hence, this information must be generated by the measurement on the first system. Since it is *immediately* apparent for the second particle, no matter what the separation, we may conclude that the interaction whereby this information is transmitted must be non-local¹.

This concludes our demonstration : if Q.M. is assumed to be complete, non-local interactions must exist.

By rejecting the possibility of non-local interactions, E.P.R. conclude that Q.M. is not complete. Before doing this, we should consider some of the further implications of such a conclusion. If we decide that Q.M. is not complete, then we should consider the means whereby this description could be made complete. i.e. We should specify more variables until the quantum state, together with such additional variables as may be necessary, is sufficient to characterize an essentially unique actual physical situation. We shall see in Chapter 4 that, if we require further that this theory be deterministic (which it may be since Q.M. is not complete) we run into non-localities of an even less acceptable variety.

The process of completing an incomplete Q.M. to give a deterministic theory is just the development of a H.V. theory, where the hidden variables are the additional variables suggested above. If Q.M. is complete, such a program would be useless since, with the Q.M. state corresponding to an essentially unique situation, each of the elements of the ensemble of actual situations pertaining to the system under discussion would be indistinguishable from any other.

1. For a detailed discussion of the concept of non-locality, see §5.2.

The hidden variables would either be superfluous or else they would have to have the same value for every element of the ensemble.

Before we move on to a discussion of H.V. theories, we consider, in detail, the notion of determinism, which plays an important (implicit) part in the E.P.R. argument, and a more explicit role in the development of H.V. theories.

2.6 Some Recent Criticisms

As we have mentioned (§2.1), the E.P.R. argument has been subject to criticism in some recent publications. Some of these criticisms have bearing on the argument presented so far, and others relate to our later development (§5.3, §5.4).

Hooker (1970) criticizes the conclusion by Jauch (1968) that the E.P.R. result represents a logical inconsistency in the Q.M. formalism by pointing out, as we have, that the argument depends on the assumption that no non-local interactions exist. Hooker also notes that Jauch's answer to the E.P.R. argument consists simply of reiterating the Q.M. features which E.P.R. find unacceptable from a classical standpoint, and stating that they are acceptable from his (Jauch's) point of view. This, argues Hooker, disguises the very source of the 'unhappiness' of E.P.R. with Q.M. by implicitly reinterpreting their notion of physical reality. In this matter, we agree with Hooker, but hasten to add that, in the light of arguments relating to the alternatives (§5.3) we may have to turn to a conclusion something like that of Jauch.

Moldauer (1974) makes much of the fact that the measurement of observables which correspond to non-commuting Hermitian operators cannot be made simultaneously; i.e. it is impossible, in a single situation, actually to predict with certainty the results of spin-component measurements in the x and z - directions on the second particle, since it is impossible to perform these measurements on the first particle without

the occurrence of uncontrolled disturbances. Hence, he distinguishes between 'predictability' (which is the case in the E.P.R. argument) and 'predictedness' (which would be the case if the results could actually be simultaneously predicted). He has certainly hit upon one of the weakest points in the E.P.R. argument. However, from the point of view of realism, his criticism amounts to asserting that the actual physical situation of the second system (or particle) depends on what measurement we actually perform on the first system. This implies some kind of non-local interaction between the two systems, and if we reject this, Moldauer's criticism falls away. 'Predictability' and 'predictedness' are clearly distinguishable with respect to the formalism of Q.M. and, indeed, this may prove to be an important distinction. However, if we further assume that i). Q.M. describes an independently existing microphysical reality and ii). no non-local forces exist, the distinction falls away.

The criticism due to Mirman (1973) seems to contain several strains, not all of which are compatible or simultaneously necessary. On the one hand he asserts that "no matter how far apart the two atoms are, they will interact, and this interaction will inform the second atom of the results of the measurement of the first atom". This is like the argument of Capri (1975) in some ways. The point made by E.P.R. is that such an interaction (relating events separated by a space-like interval) must be non-local. Mirman's additional explanation that "the first particle interacts with the first magnet which interacts with the second magnet which interacts with the second particle" does not avoid this difficulty. The non-locality is merely shifted to the interaction between the magnets. He points out that the magnets must be correlated before or after the experiment in order for the results relating to the E.P.R. argument to be obtained, and writes "Actually, of course, the interaction is produced by the experimenter aligning them". From a realist viewpoint, this means that the actual physical situation pertaining to the second system (or the microsystem as a whole) must depend on the measuring apparatus

before it has interacted with it. Mirman has identified a source of correlation between the two systems, but if a dynamical description of physical reality as it evolves with time is to be found, his explanation must be excluded on the grounds of non-locality.

On the other hand, Mirman explains the results of E.P.R. type correlations in the following paragraph of the same paper.

"In other words, the state is a superposition of states, and there is equal probability that the system is in either of these two states. What a measurement on the first particle does is determine *which of these states it is in.* Once having made this determination, the measurement on the second particle is superfluous; it merely gives back the same information". (My italics). This paragraph, together with the fact that Mirman goes on to give a classical analogue of the Bohm experiment indicates that he is begging the question by assuming that Q.M. is not complete: if the measurement on the first particle reveals more about the initial physical situation than is specified by the initial quantum state, Q.M. must be incomplete. We show in (§3.5) that in any case, this viewpoint is subject to the assumption that non-local interactions exist.

The most powerful criticism of the E.P.R. argument comes from Capri (1975) who argues that the condition of E.P.R. that the two systems cease interacting is not fulfilled. From the E.P.R. point of view, this is tantamount to assuming that non-local interactions exist. Capri argues that, since Q.M. is non-relativistic, information can travel at any speed and so interaction between the two systems at the time of measurement on the first system cannot be excluded. This argument is formally correct, but still physically somewhat uncomfortable: it indicates for instance that, on the basis of an E.P.R. type experiment, we may be able to show that the special theory of relativity is empirically invalid. However, in view of the difficulty in defining under what circumstances an interaction must be non-local (see §5.2) and other considerations (§5.3) we find that we can extend this argument to deal with the E.P.R. paradox in a fairly satisfying way (§5.4).

Chapter 3

DETERMINISM

3.1 Introduction

In the following chapter we shall deal with the attempts at 'completing' Q.M. or, alternatively, the attempts at providing a supertheory which contains Q.M. and provides a deterministic basis for it, which are known as the Hidden Variables (H.V.) interpretations of Q.M. In our development, we have indicated that the assumption Q.M. is not complete provides a motivation for the construction of a H.V. interpretation of Q.M. However, we consider the prime sociological motivation for these attempts to be a certain 'unhappiness' amongst physicists with the statistical or stochastic nature of the predictions of Q.M. i.e. In general, Q.M. does not predict the outcome of a measurement on a single particle with certainty, but gives a distribution of possible results with a definite probability for each outcome.

Now, in the past, whenever measurements on a system have yielded stochastic results, physicists have taken pains to provide a deterministic description of the behaviour of the system at another level. This occurred, for instance, with respect to Brownian motion. In fact, as long as the behaviour of the system is to be described classically, we *require* that a deterministic description be possible at some level; at least at the level at which we can apply the deterministic relations of point particle mechanics. We must assert that the seemingly stochastic behaviour of a classical system appears as a result of our lack of knowledge (i.e. empirical results) at the deterministic level. In the case of Q.M., however, the *basic* physical formalism involves a stochastic description. This is not altogether surprising since systems subject to Q.M. description behave stochastically. Measurements on these systems yield statistical distributions of randomly fluctuating results whenever the system is not such that it can be described by an eigenstate of the operator corresponding to the observable being measured. Hence, there is no *necessity*, as in the classical case, that

a deterministic description should be possible. We can only argue on the basis of past experience that such a description *should* be possible. That systems which are clearly non-classical should behave deterministically and therefore be subject to a deterministic description is an expression of *belief*, a regulative principle.

For instance Einstein (1949) has written the following. "If it is possible to move forward to a complete description, it is likely that the laws would represent relations among all the conceptual elements of this description which, per se, have nothing to do with 'statistics'".(p.673.)

Such a description would, of necessity, be deterministic. Indeed, we have shown (in §2.4) that the regulative principles i). that a physical theory should provide a complete description of physical reality, and ii). that processes in physical reality are deterministic are incompatible, given the stochastic nature of the Q.M. predictions. Hence, we suggested that Einstein's motivation for showing Q.M. to be incomplete stems from his prejudice in favour of deterministic theories.

Other authors (e.g. Bohm (1957)) have also motivated their researches into H.V. theories by a statement of their belief in the deterministic nature of the behaviour of real systems. Bohm is less definite than Einstein when he asserts that physical descriptions occur at different levels, alternately stochastic and deterministic.

Belinfante (1973) notes many diverse 'polemical' reasons as to why people should be prejudiced one way or the other with respect to determinism. We note here that *all* reasons relating to the acceptance or rejection of regulative principles must be polemical since, as we saw in §1.2, arguments relating to them cannot have a logical imperative. However, we note also that there may be deeper reasons than simple dogmatic prejudice or conservatism for scientists' wishing to retain

the notion of determinism. Deeply embedded in the world picture which first spawned, and later grew up with classical physics is the idea that physical systems (recognizable parts of physical reality) *have* properties or characteristics which change with time in a continuous or nearly continuous manner. Subject to such a world picture, it is difficult to imagine how any regularity or 'recognizability' could be maintained if the behaviour of reality were non-deterministic. If we were to set up *exactly* the same situation, with *all* of the properties or characteristics repeated, on two occasions, it is very difficult to imagine how these systems could evolve differently, and yet still behave subject to some easily recognizable and describable rules. The (unconscious) adherence to this classical world picture leads very naturally to the conclusion that fundamentally extant indeterminism in nature is absurd. However, to maintain such a world-picture is neither necessary nor, as we shall see, possible, no matter how comfortable it may seem. As we shall see in §3.5 and Chapter 4, the notion of continuously existing, continuously varying properties or characteristics, in the classical sense, is incompatible with the physical formalism of Q.M. together with some fundamental beliefs about causality.

In any event, we should recognise that determinism is an important regulative principle and, as such, should be retained if possible (by the 'Principle of Conservatism', see §1.6). We discuss the notion of determinism explicitly by distinguishing between it and causality (§3.2) and by treating the similarity between the deterministic relationship and the function in the fundamental algebraic sense (§3.3). By considering the various ways in which a ~~seemingly~~ non-deterministic situation can be subjected to a deterministic description, we are able, in §3.4, to reveal some of the conceptual devices employed in arriving at some of the interpretations of Q.M. In §3.5, we consider the most obvious of these (from the classical viewpoint) in order to indicate some of the difficulties which are likely to be encountered in any program intending to provide a deterministic description of microphysical reality. In this discussion of Ballentine's Statistical Interpretation, we show also

that the classical world picture described above is untenable with respect to Q.M. This theory is a special case of a H.V. theory and a discussion of these theories follows quite naturally in Chapter 4. We return to consideration explicitly involving determinism in §5.4.

3.2 Determinism and Causality

In the literature concerning these problems (e.g. Zeh (1975), von Neumann (1932)) the terms 'causality' and 'determinism' are frequently used as interchangeable and synonymous. We have a criticism of this usage in that the terms are indeed distinguishable in meaning. By stating that the behaviour of an isolated system¹ is deterministic, we mean that the situation of that system at any time is completely determined by knowledge of the situation of the system at some earlier time. A deterministic theory would be one in which the state of a system at some time and the relations governing the time development of the system determine, unambiguously, the state of the system at any later time. (In time reversal invariant theories, the state of the system at some time and the relations governing time development give the state of the system at *any* time).

The notion of causality is not nearly as simple to define. However, we see the notion of causality as a statement of the belief that the actual physical situation of a system at some time is related to the actual situation of that system at an earlier time, and relates, also, to the future situations of the system. It is difficult to imagine a system that is not causal, in this general sense, and it is doubtful whether a scientific description and analysis of an acausal system

1. For convenience, we consider only isolated systems in this discussion. This condition could be relaxed to 'significant isolation'. See §5.2.

could be constructed. A further specification of causality relates to a time ordering that may be logically necessary and is certainly conceptually important. i.e. In all causal relationships *causes precede effects*.

Whether the behaviour of a system is stochastic or deterministic, it is still possible, and maybe necessary for a scientific analysis, that the situation of a physical system at a certain time be somehow related to its situation at earlier times. This can be seen in statistical theories where we would agree that a certain *distribution* of single results follows (causally) from a certain preparation procedure, or that the obtaining of some single result in the distribution is related to the situation pertaining to the system previously. In this sense, we could say that the spinning of a coin *causes* the result 'heads or tails'.

Hence, a theory by means of which the later states of an isolated system can be completely determined by considering its state at some former time will be termed causal and deterministic. If, however, it is only possible, by means of a theory, to make statistical predictions of the future behaviour of a system, this theory will be termed causal and non-deterministic.

Subject to this formulation, it is difficult to imagine an acausal theory. Unless it is one which, in some cases at least, effects precede causes, such a theory would have to describe an acausal system in which, for example, no perceptible relationship between the behaviour of the system at different times occurs.

3.3 Determinism and the Function

The defining characteristic of deterministic behaviour is that different mutually exclusive events (or sets of behaviour) follow from different causes¹. (Here 'cause' is used in the sense of 'complete cause' or possibly 'essentially complete cause'). Given the occurrence of the cause, it would not be possible to determine precisely which effect¹ to expect if two or more *mutually exclusive* effects could follow that cause. We can see that a deterministic relation between events (or situations) is asymmetrical as follows; although the same effect may proceed from different mutually exclusive causes, in a deterministic situation, different exclusive effects may not follow the same cause. This asymmetry we call the *single-valuedness* of a deterministic relationship.

In contrast to the assertion to the contrary by Simon (1965), the deterministic relationship and its properties are indeed embedded in pure mathematics. Precisely this property of *single-valuedness* is a requirement on a relation between two sets if that relation is to be a *function* or mapping, in the fundamental algebraic sense of the term. The analogy between functional relationships and deterministic relationships is therefore obvious.

Now, in classical theories², the equations of motion together with the state of the system at some time (the initial conditions) determine the state of the system at any other time. It follows conceptually (as well as from a suitable mathematical formulation) that the equations of motion provide a *function* mapping the set of possible initial conditions onto the set of possible states of a system at some later time. (Since the function is one-one, it will also map onto states at earlier times).

1. In this discussion 'cause' and 'effect' can be read as shorthand for 'state at an earlier time' and 'state at a later time'. In this case, the restriction to *complete* causes is equivalent to a restriction to *isolated* systems.
2. Arguments (due e.g. to Feynman (1965)) that classical theories are not deterministic in practice, due to experimental error or insufficient isolation of the system under consideration, cannot alter the fact that the mathematical formalism and concomitant interpretation as well as the underlying philosophy of classical physics is deterministic.

This is only possible because the relationship between successive states of an isolated system described by classical mechanics is deterministic. Indeed, any of the properties of a system can be determined using a function which maps the initial conditions onto the values of that property at a later (earlier) time. This is how the world picture discussed in §3.1 is embedded in the mathematical formalism: the behaviour of a classical property is described by a single-valued (well-behaved) function; the obvious interpretation being that each classical property *has* a value at every moment, and this value usually varies in a simple and well-defined way (i.e. we usually consider only continuous, or even smoothly continuous functions in classical physics.)

Conversely, if the behaviour of a physical system is inherently non-deterministic, it is impossible to describe the time development of that system completely in terms of any mathematical entity (e.g. a functional) which has the properties of a function (i.e. single-valuedness).

This result proves to be singularly important with respect to the interpretation of Q.M. See the last part of §3.4 and §5.6.

3.4 Dealing with Indeterminism

It is useful to consider the case of a highly schematized seemingly non-deterministic phenomenon in order to examine the possible ways of describing the situation, either by introducing determinism, or by other means. Consider the situation shown in Figure 3.1 where A represents the initial state of an isolated system, and B and C represent

two mutually exclusive¹ states of the system which may occur at some later time.

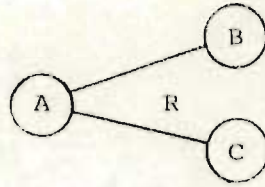


Figure 3.1

The relation R , relating the possible states at different times cannot be single-valued if it is to describe the time development of a system prepared with initial state A . Hence, on the face of it, it seems that the process under consideration is necessarily non-deterministic, and further, R cannot be a function. If this is the case, any theory describing such a phenomenon must be non-deterministic and functions cannot be used for the description of the complete time development of the system. However, situations like this occur often in the domain of classical physics, where they are rendered deterministic by a conceptual device which we call 'splitting the cause'. We will consider also a second method whereby this situation can be made to appear deterministic. This second method, which we call 'identifying the effects' does not occur in classical theories.

1). Splitting the cause

A situation much like that shown in Figure 3.1, though often more complicated in that more than two mutually exclusive situations result, is often to be found in classical statistical mechanics. For simplicity, we consider an experiment consisting of the flipping of a coin. This is somewhat artificial in that the state 'having been flipped' is not usually considered as a viable state for a coin. Nevertheless, it makes up for this in simplicity and in the fact that it can be generalized

1. If B occurs then C does not, and vice versa.

to more complicated situations as are to be found, for instance, in statistical thermal physics. Following on from the initial state 'having been flipped' (i.e. when the coin is still in the air) we have the two mutually exclusive results, 'heads' and 'tails'. (We neglect any other possible outcomes). If the state specification 'having been flipped' were to be regarded as complete (in the sense of Chapter 2), this would represent a non-deterministic phenomenon. However, the process is regarded as deterministic since the specification of the initial state is not considered to be complete, and is seen as a blanket specification covering several mutually exclusive initial states. i.e. The initial state 'having been flipped' corresponds to an ensemble of essentially different actual physical situations. The elements of this ensemble may be divided into two classes: those from which the result 'heads' follows deterministically, and those which lead necessarily to the result 'tails'. The use of the initial state 'having been flipped' is justified on the grounds that we do not know which element of the ensemble actually occurs in any single case, although we assert that, in any such case, exactly one of the elements of the ensemble actually occurs. If we knew which one it was (or which of the two classes it belonged to), either by means of measurement or else by using a more refined preparation system we would be able to predict the result (i.e. heads or tails) with certainty. In classical physics, we commonly assert that this is the case, even when such measurements or preparation procedures are impossible, as is the case in the statistical description of a box of gas, or Brownian motion, for example. This procedure for rendering the description of the phenomenon deterministic can be simply illustrated in terms of a modification to Figure 3.1.

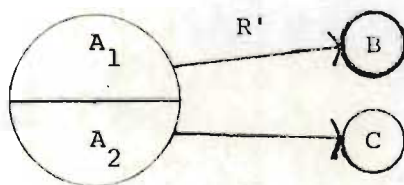


Figure 3.2

The 'cause' A, is considered to represent at least two mutually exclusive actual situations which can be divided into two classes, A_1 and A_2 . The 'effects', B and C, then follow deterministically from any actual situation which falls in A_1 and A_2 , respectively. It is clear here that the relation R' (as shown in Figure 3.2) can be used to define a function mapping $\{A_1, A_2\}$ onto $\{B, C\}$.

It is, at first, difficult to see how there could be any objection to applying the same procedure to situations described by Q.M. Consider, for instance, the case of a beam of spin- $\frac{1}{2}$ particles prepared with spin component orientated in a given direction, (spin 'left') which is incident on a Stern-Gerlach (spin component measuring) device orientated at right angles to the polarization direction. The beam is split into two parts, each one indicating opposite spin polarization, ('up' and 'down'), in the direction defined by the orientation of the measurement device. According to the procedure discussed above, we should assert that the beam of particles prepared with spin 'left' can be divided into two classes: those particles which will give the result 'up' on measurement, and those which will give the result 'down'.

In the simplest version of this procedure, we would say that each particle (prepared with spin 'left') *has* a spin component 'up' or 'down' prior to measurement, which simply separates the two classes. We show in §3.5 that this program, applied to Q.M. systems, implies the existence of non-local forces. A more sophisticated alternative would be to assert that each Q.M. state (spin 'left' in our example) corresponds to an ensemble of essentially different actual situations, half of which *lead* deterministically to the result 'up' and half of which *lead* to the result 'down', without asserting that the particles *have* spin 'up' or 'down' prior to measurement in the same way as classical systems *have* properties at all times.

The former interpretation is equivalent to the 'Statistical Interpretation' of Ballentine (1970) and the interpretation of Q.M. due to Landé (1955, 1965, 1975) and Duane (1923). The latter procedure is the basis of the H.V. interpretations of Q.M. in their more sophisticated forms. It is clear from this discussion that the interpretations of Ballentine and Landé are special cases of hidden variables theories in which the values which different properties *have* (in the classical sense) prior to measurement are the 'hidden' variables. We show in §4.4 that the same result as for Ballentine's and Landé's interpretations holds for all H.V. theories which have predictions consistent with Q.M.

(ii) Identifying the effects

The only alternative means by which the phenomenon depicted in Figure 3.1 can be made to appear deterministic is to assert that the two effects, B and C, are *not* mutually exclusive at all. They are seen as perfectly compatible effects of a common cause, A. In this case, good reasons must be given as to why, at first sight, the effects B and C appear to be mutually exclusive. There is no simple classical analogue of this procedure, but it gives rise to two different interpretations of Q.M. We may illustrate the procedure as follows:

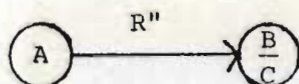


Figure 3.3

Note that R'' is a single-valued relation so that the time development of the system can be represented by a function.

This device is used in the 'Many Universes' interpretation of Q.M. (See § 7.7) in which it is asserted that the different possibilities

compatible with a statistical prediction from a given quantum state *all* actually occur, even when the system is a 'single system' consisting of a single particle. The seeming inconsistency of the results 'finding the particle at point (x_1, y_1) on the screen' and 'finding the particle at point (x_2, y_2) on the screen', where $(x_1, y_1) \neq (x_2, y_2)$, is explained by the assumption that they occur *in different universes* whereas we are only conscious of one universe¹. Thus, in one universe, exactly one result occurs for each single system. However, all the other possibilities *do* occur without being available for simultaneous observation. According to this interpretation, the evolution of such a system (any quantum system which is not in an eigenstate of the operator corresponding to the measured observable) for any one observer (or any one consciousness of an observer!) appears to be non-deterministic. However, the evolution of the universe (or universes!) as a whole is considered to be deterministic. The function R is provided in this case, by the Schrödinger equation or its equivalent.

A second interpretation in which Q.M. can be rendered deterministic in this way is achieved by asserting that the theory does not deal with 'single systems' or systems consisting of 'single particles'. It is restricted, according to this definition, to dealing with systems which consist of many particles. A supportive argument is constructed on the basis of the 'relative frequency' theory of probability i.e. It is assumed that the concept of probability, and hence the predictions of Q.M., is only meaningful when applied to large (actual) ensembles. In this case, the quantum state can be used to predict deterministically a single, unambiguous *distribution* of results. This is an example of the 'statistical determinism' of d'Espagnat (1971).

Thus according to this interpretation, Q.M. does not deal with measurement results like 'the particle was found at (x, y) on the screen.' It only deals with results obtained on whole (actual) ensembles of single systems, of the kind : 'the particles were found on the screen in such-and-such a distribution.' In this case too, the single-

1. Strictly, we would be reproduced in many universes, but each consciousness would be unaware of the others.

valued relation (function) relating the initial state (of a many-particle system) with the final outcome, a distribution of results with specified relative densities, is provided by the Schrödinger equation or its equivalent.

This interpretation occurs explicitly in the work of Belinfante (1975). It also occurs implicitly in some other interpretations (e.g. that due to Daneri, Loinger and Prosperi (1966)). We deal with it in more detail in § 7.4. However, we note briefly that this interpretation has some extremely unattractive features. It involves the assertion that there are experiments, performed on systems usually considered to be within the quantum mechanical domain, which cannot be described by Q.M. i.e. Those experiments in which few enough particles are involved for statistical fluctuations to be evident. This restriction is motivated, in the case of Belinfante, at least, by an adherence to a particular theory of probability as well as, probably, a desire to avoid some of the more puzzling problems in the interpretation of Q.M. Nevertheless, there are theories of probability which, no matter how shaky their philosophical basis, encompass the use of probability in the description of single-events.

The statement that a die has associated with it a probability of $\frac{1}{6}$ for each face landing upwards can be made on the basis of the measurement of the properties of that die and the throwing device, without even throwing the dice once, never mind hundreds of times.

Further, this adherence to the "relative frequency" interpretation of probability, and the concomitant assertion that Q.M. deals only with real ensembles, prevents us from using Q.M. to induce what 'microphysical reality' is like. As with all other regulative principles, whether we choose to make this assumption or not is arbitrary, from a logical point of view. However, for the reasons mentioned and by invoking the 'principle of conservatism' we shall consider that Q.M. applies to single systems as well as many-particle

systems. This involves the assumption of some alternative theory of probability (like, perhaps, the 'betting' theory, since we can bet accurately!). We deal with this question again in §5.5.

iii). Indeterminism

Should we find that the postulate that microphysical processes are inherently deterministic cannot be reasonably maintained, there are still at least two ways in which a phenomenon like that depicted in Figure 3.1 can be described. Firstly, we can throw up our hands and abandon all hope of achieving a *dynamical* description of the evolution of microphysical processes with time, and simply relate the initial situation, A, and the final results, B and C, by R as in Figure 3.1. In the case of Q.M., this is already achieved by the physical formalism. Here we assert that, if a system has quantum state $|\psi\rangle = \sum_n c_n |\psi_n\rangle$ at the time of measurement of the observable associated with a Hermitian operator with eigenstates $\{|\psi_n\rangle\}$, this means that the result will be as if the system were in the state $|\psi_n\rangle$ with probability $|c_n|^2$. The evolution of quantum states with time is simply regarded as an *algorithm* for calculating the possible measurement results, and is *not* interpreted as representing, in any way, the time development of any actual microphysical situation. From this point of view, we must either look to theories other than Q.M. in our attempts to discover the nature of microphysical reality, or else regard such attempts as impossible. The other theories required for the first alternative do not exist¹ and the second alternative involves the assumption of an empiricist or positivist stance which we discussed and rejected in §1.8.

1. It is difficult to see how the basic features of such an alternative could differ from Q.M., on the basis of the empirical evidence on microsystems which exists at present.

A second alternative is to assume that the time development of the quantum mechanical state, as occurs in the physical formalism of Q.M.¹, gives some information as to the actual dynamical development of the microphysical system which it represents. Here, we are faced with the difficulty that whereas, as we have shown, a non-deterministic time development cannot be represented by a function, in Q.M. time evolution is always described deterministically, either by means of the Schrödinger equation or by the action of a time development operator. i.e. Knowledge of the initial quantum state of a system allows us to determine the exact, unambiguous state of the system for all future times, by means of the physical formalism. There is one powerful condition on this procedure: it is possible *provided no measurements take place*. On the other hand, it is only in the interpretation of the quantum state on *measurement* that statistical considerations enter into Q.M. As we shall see in Chapters 6 and 7, it is only when we attempt to describe *all* the physical processes involved in measurement by means of Q.M. that difficulties occur. Thus we can consider the following schema for the non-deterministic evolution of a microphysical system: the system is prepared in a certain way (corresponding to a quantum state) and, thereafter, it develops deterministically until certain criteria are fulfilled. When these criteria are satisfied, a non-deterministic change in the actual situation pertaining to the microphysical system occurs. Thereafter the evolution may again be deterministic (as described by the Schrödinger equation). These criteria, which must be satisfied at some stage in all measurements, will be investigated further in Chapter 9. Schematically, this evolution may be represented as follows:

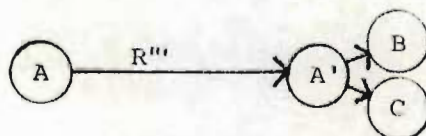


Figure 3.4

1. Here, we consider the 'Schrödinger Picture' as opposed to the mathematically equivalent 'Heisenberg Picture' in which the *operators* change with time.

Here A' represents a situation which satisfies the criteria for non-deterministic change. The single-valued relation R''' is as defined by, say, the Schrödinger equation. The non-deterministic change will be called '*state reduction*' and will be dealt with in §5.6. It will also be introduced after different considerations, in Chapter 7 and discussed in the following chapters.

3.5 The 'Statistical' Interpretation of Quantum Mechanics

Before we treat those interpretations which are usually considered to be H.V. theories, we will analyse the 'statistical' interpretation described by Ballentine (1970). Many different interpretations have gone under the name 'statistical', but the theory which we consider here is similar to that due to Landé (1955, 1965, 1975). We agree with many of Landé's criticisms of the conventional or 'orthodox',¹ interpretations of Q.M. as well as his aim to "explain the quantum principles themselves, that is to show them to be consequences of still more elementary principles known from pre-quantal physics" (Landé (1965) p2). Still we find that this interpretation violates a fundamental regulative principle: that no 'non-local' interactions exist. Since this interpretation is a type of H.V. theory, that it involves the admission of non-local interactions follows from the proof, presented in §4.4, that this result holds for all H.V. theories with the same predictions as the Q.M. formalism. Nevertheless, it is useful to deal specifically with this interpretation, firstly as an indication of some of the difficulties which arise when attacking the problems of interpretation of Q.M. from a naive classical position, and secondly, to provide an illustration of the general proof which may render it more reasonable and intelligible. Finally, the analysis of

1. The term 'orthodox' has been applied as ambiguously as the term 'statistical' in relation to interpretations of Q.M.

this interpretation gives rise to some restrictions on the nature of microphysical reality, and hence, the possible forms of a H.V. theory. Still, we stress that this interpretation *is* a H.V. theory, where systems are assumed to 'have' properties (such as spin, momentum, position, etc.) in the same way as do classical systems. The values that these properties 'have' in a particular case, information which is not generally given in the Q.M. state, would determine the values of the hidden variables in this interpretation. Q.M. is certainly seen as incomplete (in the sense used in Chapter 2) since the quantum state

"..... (pure or otherwise) represents an ensemble of similarly prepared systems. For example, the system may be a single electron. Then the ensemble will be the conceptual (infinite) set of all single electrons which have been subjected to some state preparation technique (to be specified for each state), generally by interaction with a suitable apparatus. Thus a momentum eigenstate (plane waves in configuration space) represents the ensemble whose members are single electrons each having the same momentum, but distributed uniformly over all positions. Physical systems which have been subjected to the same state preparation will be similar in some of their properties, but not all of them (similar in momentum but not in position in the example). Indeed the physical implication of the uncertainty principle is that no state preparation procedure is possible which would yield an ensemble of systems identical in all of their observable properties. Thus it is most natural to assert that a quantum state represents an ensemble of similarly prepared systems, but does not provide a complete description of an individual system. "Ballentine (1970).

Let us consider these 'most natural' assertions in the light of two experiments; two-slit interference between electrons, and the spin-component measurements on decay products of a spin-zero particle considered in §2.2.

Suppose a mono-energetic electron beam of low intensity (one electron at a time) from a narrow source passes through two slits of suitable width and spacing in an opaque screen to fall on a detecting system (we could equally well consider the source to be a monochromatic light source). If the detector is placed close to the slits, we find that each electron is either detected behind one slit or the other. i.e. In an actual experiment (such as that performed by Janossy and Naray (1969)) no significant number of coincidence counts occur. Hence, according to this interpretation, the ensemble corresponding to the quantum state defined by the preparation procedure can be divided into two classes: those electrons which pass through one slit and those which pass through the other. i.e. Electrons *either* pass through one slit *or* the other¹. Now, when the detector is moved further away from the slits, we find that the detection events are distributed in a characteristic manner, the 'two slit interference pattern'.

Suppose now than an 'electron absorber' or slit cover is placed behind one of the slits as in Figure 3.5. If non-local forces do not



Figure 3.5

1. This result follows directly from the naive classical standpoint implied by this interpretation.

exist, those electrons which pass through the other slit will not interact with the slit cover, and they will be detected in the positions in which they would have been found had no slit cover been present. The electrons which pass through the covered slit are absorbed by the slit cover and so do not affect the detection apparatus. A similar situation holds if the slit cover is moved to open the slit previously covered and close the other. By adjusting the detection time in each case, we should, according to this interpretation, be able to recreate the same ensemble of detection events as if no slit cover had been present: those electrons which are detected will not have interacted with it at all, in the absence of non-local forces. Hence, in this case we expect to find the same distribution of detection events (the 'two slit interference pattern') as in the case where the slit cover is absent. This is, of course, not the case. Instead a distribution characteristic of two overlapping 'single slit interference patterns' is observed.

Thus, our assumptions are at fault. i.e. Either the electrons do not pass through one slit or the other, or else there must be some non-local interaction between the electrons passing through the uncovered slit and the slit cover. The first alternative is inconsistent with the postulates of the interpretation under consideration. Thus, if the 'statistical' interpretation of Q.M. is to be logically consistent and empirically correct, it must involve non-local interactions.

We note that the method proposed by Ballentine and Landé to account for such interference phenomena (i.e. Duane's rules) does not avoid the difficulties described above. Duane (1923) devised an algorithm to account for interference phenomena without involving waves. Interference effects can be calculated by assuming that there are characteristic transfers of momentum associated with any periodic microscopic system¹.

1. This requires that the macroscopic body be rigid or else in 'instant communication' with all of its parts. This in itself implies the existence of non-local forces.

If we place the slit cover *behind* the slits, as in Figure 3.5, then each electron must interact with the slits *prior* to interacting with the slit cover¹.

In the absence of non-local forces, the electrons receive their characteristic momenta upon interaction with the double slit. *Thereafter*, those which pass through the closed slit will be absorbed, whereas those which arrive at the screen will have received increments of momentum suitable for the formation of a *double slit* pattern. By superimposing the detection events obtained from each slit as before, we reproduce the same result as when no slit cover is present. i.e. We still expect a double-slit pattern, in the absence of non-local forces, either between the slit cover and the electrons which pass through the open slit or else between the slit cover and the screen forming the slits, which would change the predictions of Duane's rules for the slits.

The unacceptability of this interpretation can be further illustrated by considering measurements of spin components for two spin-half decay products of a spin-zero particle. The details of this experiment are presented in §2.2 above. Recall that the problem was to account for the fact that the spin components of the two decay products are correlated (in opposite senses) despite the fact that the measurements on the particles are performed at spacially separated positions. In this interpretation, the problem appears, at first sight, to be trivial: After the decay, each particle 'has' a spin which is oriented in a given (if unknown) direction. These spins are in opposite senses immediately after the decay, and measurement simply reveals this fact. Let us suppose, therefore, that each particle 'has' spin component 'up' and

1. In an extreme case, the slit cover could be inserted only *after* each electron had interacted with the slits. In this experiment (which would take so long to perform as to be impossible) we *may* expect different results!

'down' respectively.

We know that a beam of particles prepared with spin 'up' (or 'down'), gives the results 'left' and 'right' with probability $\frac{1}{2}$ when the spin component is measured in a horizontal direction. In terms of the statistical interpretation, we conclude that, of a beam of particles prepared with spin 'up' one half of the particles has spin left and the other half has spin right.

Thus, in our two-particle experiment, the particle with spin 'up' may have spin 'left' or 'right' with probability $\frac{1}{2}$, while the same is true for a particle with spin 'down'. Hence, if we measure the spin-components of these particles in a horizontal direction, we expect, according to the laws for combining probabilities, to find with probability $\frac{1}{2}$ that the particles have spin components in opposite senses (left/right or right/left) or, with probability $\frac{1}{2}$ that they have spin components in the same sense (left/left or right/right). This is in contradiction with Q.M. which predicts that the spin components will be anti-correlated, irrespective of the direction in which spin components are measured.

Another general difficulty with this interpretation, closely related to that just considered, is that it is difficult to see how it is possible to distinguish between a beam of particles, all of which are in a superposition of two quantum states, and one in which each particle is in *one* of the two quantum states. i.e. It is difficult to distinguish between a 'superposition' and a 'mixture'. Consider, for instance, the differences between a beam of spin $\frac{1}{2}$ particles described by the superposition of spin states.

$$|\psi\rangle = 2^{-\frac{1}{2}}(|L\rangle + |R\rangle) \quad 3.1$$

and a beam of particles each of which is described by $|L\rangle$ or $|R\rangle$ with probability $\frac{1}{2}$ in each case. Here $|L\rangle$ and $|R\rangle$ are eigenstates corresponding to spin components in opposite senses, in the same horizontal direction.

If we measure the spin components of first beam in this horizontal direction, we find that half of the beam behaves as though it had spin 'left' while the other half gives the result 'spin right'. Thus, according to the statistical interpretation, the first beam consists of particles one half of which 'have' spin 'left' and the other half of which 'have' spin 'right'. This does not distinguish it from the second beam. However, if these beams are passed through a Stern-Gerlach machine oriented in the 'up-down' or vertical direction, the first beam will be deflected in one direction only, whereas the second will be split into two parts. The statistical interpretation cannot account for this difference between a superposition and a 'mixture' of two quantum states, although it has observable effects.

The unacceptability of this statistical interpretation indicates a certain limitation on the nature of H.V. theories: the hidden variables cannot indicate the values that properties of the system '*have*', in the sense of classical theories. They can, at most, indicate what values *will be found* upon measurement. Here, already, we have a hint that something interesting must occur during measurement on microphysical systems: properties change from propensities, in some general sense, to the actualities of classical properties. We will take up this hint in Chapter 5. First, we consider some of the more sophisticated and complicated attempts at 'splitting the cause' to achieve a deterministic description of microphysical reality.

We note finally, that, in rejecting this statistical interpretation, we are also rejecting the classical world picture in which all properties of a system '*have*' continuously, or nearly continuously varying values, which we mentioned in §3.1. Possible alternatives will be considered in Chapter 5.

Chapter 4

HIDDEN VARIABLES THEORIES

4.1 Introduction

The procedure of 'splitting the cause' outlined in §3.4 is well known and much used in classical physics. For this reason, physicists who find the fundamental statistical nature of the predictions of Q.M. unsettling, have attempted to apply this procedure to the apparently non-deterministic behaviour to be found in most experiments¹ involving microsystems. As we have indicated, this gives rise to the so called 'Hidden Variables' interpretations of Q.M. which may be defined as follows:

Hidden Variables Interpretations of Q.M. are those in which it is assumed that, by augmenting the specification of a microphysical system provided by the quantum state with additional (hidden) variables or parameters, we can determine the result of any measurement on the system with certainty.

From this point of view, Q.M. is clearly 'incomplete' in that the ensemble of actual situations specified by the quantum state must contain elements which are not essentially identical i.e. those which give rise to different measurement results. This assertion follows from the assumption that the behaviour of microphysical systems is inherently deterministic.

Indeed, the preservation of the notion that the behaviour of physical systems is deterministic provides the main motivation for attempting to construct H.V. theories. This stemmed, initially, from a desire to situate microphysical systems and their treatment

1. Those in which the system is not described by an eigenstate of the operator corresponding to the measured observable.

within the 'classical world picture' described in §3.1, with its concomitant determinism. As we have shown in §3.5 by considering the interpretations due to Ballentine (1970) and Landé (1955, 1965, 1975) this latter aim cannot be realised if we are to exclude non-local interactions.

One of the interesting features of the development of H.V. theories is that they did not appear in any coherent form until some thirty years after the formulation of the Q.M. formalism. Belinfante (1973) lays the blame for this tardy development squarely at the feet of von Neumann and his 'proof' of the impossibility of H.V. theories (von Neumann (1932)). Bell (1966) showed that this 'proof' rests on postulates which are not necessary for a H.V. theory. Nevertheless, we believe that there are more complicated reasons for neglecting the possibility of H.V. theories. The realization (implicit or explicit) that such theories cannot be used to interpret Q.M. in the context of a classical world picture removes the most powerful motivation for their construction. Also, according to Wigner¹, one of Von Neumann's strongest criticisms of H.V. theories was that they must be enormously complicated. This follows from the fact that the results of even an ideal measurement² cannot, in general be reproduced in a sequence of measurements: If operators A and B do not commute, then two measurements of the observable corresponding to A, separated by a measurement of the observable corresponding to B will not necessarily give the same result. Finally, there is (as yet) no experimental evidence favouring a H.V. approach over ordinary Q.M. We deal with this point in the next section.

1. See Wigner (1970), Footnote 1.

2. For more detail concerning ideal or 'non-disturbing' measurements see § 7.5.

4.2 The Logical Status of H.V. Theories

Bell's (1966) analysis of von Neumann's 'impossibility proof' shows that, as we would expect, measurements on system prepared in a given quantum state together with specified hidden variables would not reproduce the predictions of Q.M. The additional specification of the hidden variables would ensure that the same determined result would occur every time the experiment was performed, whereas the results of measurement on a system prepared with a given quantum state only are, in general, randomly distributed over different mutually exclusive possibilities. Hence, in situations where the results agree with the predictions of Q.M., we must conclude that the values of the hidden variables must be distributed randomly. Indeed, nobody has so far been able to prepare microphysical systems in such a way that the hidden variables have anything other than a random distribution. Papaliolios (1967) attempted to detect deviations from Q.M. predictions which would indicate some disturbance in the distribution of the hidden variables. He assumed that an ideal measurement would select a non-random distribution of the hidden variables, and performed a second measurement immediately after an ideal measurement. In each case, the predictions of Q.M. were fulfilled. Thus, either the 'relaxation time' for the hidden variables to return to a random distribution was shorter than the time between Papaliolios' measurements (10^{-13} s) or else experimental confirmation of H.V. theories cannot be obtained in this way. In any event, no direct experimental evidence for hidden variables theories exists. By invoking the principle of economy of postulates (i.e. Occam's Razor) it is therefore possible to exclude H.V. theories without further ado. If the hidden variables are doomed to remain 'hidden' in this way, we could, logically, get on just as well without them. Nevertheless, in our quest to understand the nature of microphysical reality, it is important to know the conditions under which a deterministic description of the behaviour of microsystems is possible. We shall see (in §4.3 et seq) that there are much stronger reasons for abandoning H.V. theories.

4.3 Non-locality in Hidden Variables Theories

We do not consider in detail any of the ingenious attempts (e.g. by Bohm (1951), Bohm and Vigier (1954), Jauch and Piron (1963), Jauch (1968), Pearle (1976)¹) at constructing a H.V. theory which is compatible with the predictions of Q.M.². Instead, we show that all such theories involve non-local effects which are at least 'artificial and unpleasant'. Belinfante (1973) traces the proof upon which this result depends back to the work of Gleason (1957). A similar proof appears in the work of Kochen and Specker (1967). (Belinfante (1973), provides an alternative proof of their result) where they show that the outcome of any particular measurement cannot be determined by specifying the quantum state and the value(s) of the hidden variable(s) only; the spectrum of possible results (i.e. the eigenvalues or, equivalently, the eigenvectors of the operator corresponding to the observable to be measured) must also be specified if the result is to be determined with certainty.

Bell (1964) was the first to relate this sort of result to non-locality, in his proof and discussion of 'Bell's inequality'. Subsequent reformulations of Bell's proof have been made (e.g. by Belinfante (1973) and Wigner (1970)). Wigner's proof has the advantage of being couched in conceptual, as opposed to formal, terms as well as being relatively simple and intelligible. It is therefore well-matched with the methods of analysis employed

1. Pearle himself does not consider his theory to be a H.V. theory in that 'no new variables are introduced into quantum theory'. We regard it as such since the values of some variables (the Q.M. phases) determine the outcome of any measurement with certainty and the result shown in §4.3 certainly applies to it.
2. The interested reader should consult Belinfante (1973) for a comprehensive review of H.V. theories.

elsewhere in the present work. Further, it is clearly not subject to the criticism of Bell's proof by Lochak (1976) where it is claimed that Bell's argument applies only to a special kind of H.V. theory i.e. that Bell assumes properties which are not necessary for a H.V. theory.

Following Wigner (1970) we consider the experiment which we used in §2.2 and §2.5 to illustrate the E.P.R. argument; viz that involving the decay of a spin-zero particle into two oppositely directed spin-half particles (fermions). We suppose that the spin component of each particle is to be measured in one of three possible directions, ω_1 , ω_2 and ω_3 . For simplicity, and to make the discussion more realistic¹, we consider only those directions which lie in a plane normal to the direction of propagation of the particles.

Now let us suppose that a hidden variables theory exists. It follows that Q.M. is not complete. Thus the singlet state (Equation 2.5) specifies an ensemble of actual situations which are not essentially identical. Then, by a further specification of hidden variables, we suppose that we can determine with certainty, the results of each of nine possible measurements, should it be performed. (The nine measurements are of spin component of particle 1 in the ω_1 - direction and of particle 2 in the ω_1 - direction; that of particle 1 in the ω_1 - direction and of particle 2 in the ω_2 - direction; that of particle 1 in the ω_1 - direction and of particle 2 in the ω_3 - direction; etc.)

1. The Stern-Gerlach machine, employing an inhomogeneous magnetic field, is a measurement device which measures spin-component 'directly'. It can only be used to measure spin components in a direction normal to the direction of propagation of the beam.

Now, for each measurement, there are four possible results. Denoting the result "spin component of particle 1 in the $+\omega_i$ - direction and that of particle 2 in the $-\omega_j$ - direction" by $(+, -)$, these are $(+, +)$, $(+, -)$, $(-, +)$ and $(-, -)$. This means that there are 4^9 possible results for the nine measurements. Hence, we may divide the ensemble specified by the quantum state into 4^9 subensembles (which are not necessarily disjoint) such that the result of each measurement can be determined by specifying (or finding out) in which of the 4^9 domains the actual situation happens to be in each case.

We can simplify matters considerably by making the assumption that no non-local interactions exist. More specifically, we assume that the result of a measurement on particle 2 cannot be affected by the orientation of the measurement apparatus at the site of the measurement on particle 1. This is equivalent to excluding a non-local interaction since, if it were not so, the events 'orientating measurement apparatus 1' and 'registering a measurement result for particle 2' would be causally related, despite the fact that they may occur as far apart spacially, and as close together in time as we like. (For more detail see §5.2, §5.3). Bell (1964) calls this the 'locality assumption'. It requires, in effect, that the result of a measurement on one of the particles be independent of the direction in which the measurement on the other particle is performed. With this assumption, we can reduce the number of subensembles which we must consider from 4^9 to 2^6 . Each of these subensembles can be characterized by the symbol

$$(\sigma_1, \sigma_2, \sigma_3; \tau_1, \tau_2, \tau_3) \text{ where } \sigma_i = \pm 1, \tau_j = \pm 1 \quad 4.1$$

where, if the actual situation occurs in this subensemble, we would expect the results $\sigma_i \frac{h}{2}$ and $\tau_j \frac{h}{2}$ for a measurement of the spin component of particle 1 in the ω_i direction and particle 2 in the ω_j direction respectively, should these measurements be performed.

For example, we expect the result $+\frac{h}{2}$ for a measurement of the spin component of particle 1 in the ω_1 and ω_2 directions and particle 2 in the ω_2 and ω_3 directions for all systems which occur in the subensemble $(+1, +1, -1, -1, +1, +1)$. There are clearly 2^6 such subensembles which must be disjoint since they are mutually exclusive.

Further, we can set up a one-to-one correspondence between the symbol $(\sigma_1, \sigma_2, \sigma_3; \tau_1, \tau_2, \tau_3)$ which characterizes a subensemble, and the *probability* that the actual situation will be in that subensemble, for a given quantum state¹. Henceforth, the symbol will stand for this probability.

In order to compare the predictions of local hidden variables theories with those of the Q.M. formalism, we must calculate the probabilities of finding certain results in one measurement, irrespective of what the results are in other directions. For example, the probability that a measurement in particle 1 in the ω_1 direction will give a result $+\frac{h}{2}$ and one on particle 2 in direction ω_3 will give a result $-\frac{h}{2}$, is given by

$$\sum_{\sigma_2, \sigma_3, \tau_1, \tau_2} (+1, \sigma_2, \sigma_3; \tau_1, \tau_2, -1) = : (+1, \cdot, \cdot; \cdot, \cdot, -1) \quad 4.2$$

where we have simply added the separate, independent probabilities. Here we introduce a new notation where the symbols σ_i, τ_j that are unspecified (and summed over) are replaced by dots.

1. d'Espagnat, (1971), in his presentation of Wigner's proof, assumes implicitly that each element of the ensemble of actual situations corresponding to a given quantum state occurs with equal a priori probability. This assumption is not necessary, and it weakens the proof which, in the form presented here, is valid for any probability distribution.

So far, we have constructed a general framework to represent a local hidden variables theory, without requiring that it reproduce the results of Q.M. By introducing this requirement, we have that

$$(\pm 1, \cdot, \cdot; \pm 1, \cdot, \cdot) = (\cdot, \pm 1, \cdot; \cdot, \pm 1, \cdot) = (\cdot, \cdot, \pm 1; \cdot, \cdot, \pm 1) = 0 \quad 4.3$$

This follows from the quantum mechanical result that the spin components of the two particles in any one direction are anti-correlated: i.e. if particle 1 has spin component in the $+\omega_i$ - direction, the probability for finding particle 2 with spin component also in the $+\omega_i$ direction is zero.

Now, in particular,

$$(+1, \cdot, \cdot; +1, \cdot, \cdot) = 0$$

$$\text{i.e.} \quad \sum_{\sigma_2, \sigma_3, \tau_2, \tau_3} (+1, \sigma_2, \sigma_3; +1, \tau_2, \tau_3) = 0 \quad 4.4$$

All the terms in the sum on the r.h.s. of 4.4 are positive or zero, being probabilities. Therefore, 4.4 implies that they are all zero. By applying a similar argument for $\sigma_1 = \tau_1 = -1$ and $i = 2, 3$, we obtain the result that

$$(\sigma_1, \sigma_2, \sigma_3, \tau_1, \tau_2, \tau_3) = 0 \text{ if } \sigma_i = \tau_i, i = 1, 2, 3 \quad 4.5$$

Now, if the angles between directions ω_1 and ω_2 , ω_1 and ω_3 and ω_2 and ω_3 are given by θ_{12} , θ_{23} and θ_{13} ($0 \leq \theta_{ij} < \pi$) it follows from the physical formalism of Q.M. that

$$\begin{aligned}
 (+1, \cdot, \cdot; \cdot, +1, \cdot) &= \frac{1}{2} \sin^2 \frac{\theta_{12}}{2} \\
 (+1, \cdot, \cdot; \cdot, \cdot, +1) &= \frac{1}{2} \sin^2 \frac{\theta_{13}}{2} \\
 (\cdot, \cdot, +1; \cdot, +1, \cdot) &= \frac{1}{2} \sin^2 \frac{\theta_{23}}{2}
 \end{aligned}
 \tag{4.6}$$

Now, using 4.5 to eliminate terms equal to zero,

$$\begin{aligned}
 (+1, \cdot, \cdot; \cdot, +1, \cdot) &= (+1, -1, \cdot; -1, +1, \cdot) \\
 &= (+1, -1, -1; -1, +1, +1) \\
 &\quad + (+1, -1, +1; -1, +1, -1)
 \end{aligned}
 \tag{4.7}$$

However, the first term in this expansion occurs also in the expansion of $(+1, \cdot, \cdot; \cdot, \cdot, +1)$ and, since all the terms are greater than or equal to zero,

$$(+1, -1, -1; -1, +1, +1) \leq (+1, \cdot, \cdot; \cdot, \cdot, +1)
 \tag{4.8}$$

Similarly, the second term in the r.h.s. of 4.7 occurs also in the expansion of $(\cdot, \cdot, +1; \cdot, +1, \cdot)$ and so

$$(+1, -1, +1; -1, +1, -1) \leq (\cdot, \cdot, +1; \cdot, +1, \cdot)
 \tag{4.9}$$

Substituting from 4.8 and 4.9 into 4.7 we get

$$\begin{aligned}
 (+1, \cdot, \cdot; \cdot, +1, \cdot) &\leq (+1, \cdot, \cdot; \cdot, \cdot, +1) \\
 &\quad + (\cdot, \cdot, +1; \cdot, \cdot, +1)
 \end{aligned}
 \tag{4.10}$$

Substituting from 4.6 into this inequality we get

$$\sin^2 \frac{\theta_{12}}{2} \leq \sin^2 \frac{\theta_{13}}{2} + \sin^2 \frac{\theta_{23}}{2}
 \tag{4.11}$$

1. It has been pointed out by Shimony (See Wigner (1970)) that 'Bell's inequality' follows easily from this result.

Now, we can choose the labels ω_1, ω_2 and ω_3 and their positive senses such that, for any three coplanear directions, $\theta_{12} = \theta_{13} + \theta_{23}$

$$\begin{aligned}
 \text{In this case, } \sin^2 \left(\frac{\theta_{12}}{2} \right) &= \sin^2 \left(\frac{\theta_{13} + \theta_{23}}{2} \right) \\
 &= \left(\sin \frac{\theta_{13}}{2} \cos \frac{\theta_{23}}{2} + \sin \frac{\theta_{23}}{2} \cos \frac{\theta_{13}}{2} \right)^2 \\
 &= \sin^2 \frac{\theta_{13}}{2} \cos^2 \frac{\theta_{23}}{2} + \sin^2 \frac{\theta_{23}}{2} \cos^2 \frac{\theta_{13}}{2} \\
 &\quad + 2 \sin \frac{\theta_{13}}{2} \sin \frac{\theta_{23}}{2} \cos \frac{\theta_{13}}{2} \cos \frac{\theta_{23}}{2} \\
 &= \sin^2 \frac{\theta_{13}}{2} + \sin^2 \frac{\theta_{23}}{2} + 2 \sin \frac{\theta_{13}}{2} \sin \frac{\theta_{23}}{2} \cos \frac{\theta_{12}}{2}
 \end{aligned}$$

4.12

Hence, 4.11 becomes

$$2 \sin \frac{\theta_{13}}{2} \sin \frac{\theta_{23}}{2} \cos \frac{\theta_{12}}{2} \leq 0 \quad 4.13$$

Now for distinct directions ω_j , $0 < \theta_{ij} < \pi$ hence $0 < \frac{\theta_{ij}}{2} < \frac{\pi}{2}$

and so each of the terms of the l.h.s. of 4.13 must be positive, and 4.13 must be false i.e. the condition 4.11 cannot hold for any choice of coplanear $\omega_1, \omega_2, \omega_3$.

Thus, we have shown that a hidden variables theory, as we have outlined it, cannot reproduce the results of Q.M. in this case. The result also holds in general, of course, since a single counter-example is sufficient. It may be that the predictions of a hidden variables theory are correct, and those of Q.M. incorrect in this case. However unlikely this possibility may seem, in the face of the general successes of the Q.M. algorithm, this is a matter that should be decided by experiment (see §4.5).

Alternatively, we may *require* that the hidden variables theory reproduce the predictions of Q.M. In this case, we must relax at least one of the assumptions made at the outset of the above argument. We cannot relax the assumption that the theory is a hidden variables theory in the sense of our definition in §4.1. without changing the subject of our discussion¹. This means that the only basis on which we can construct a H.V. theory which reproduces the results of Q.M. when the hidden variables are randomly distributed, is to relax the 'locality assumption'. i.e. We must assume that the result of a spin-component measurement in a given direction on particle 2 *is* affected by the direction in which the spin-component of particle 1 *is measured* in each case. i.e. If we suppose that the quantum state, as well as all relevant hidden variables are specified for a particular system like that considered above, we would still be unable to determine the result of a measurement on particle 2 by means of such a theory without knowing the direction in which the measurement on particle 1 was to be made, no matter how far away the latter may be.

This is equivalent to the result of Kochen and Specker (1967) who showed that the result in a particular measurement cannot be a function of the quantum state and the hidden variables only: it is also necessary to specify the spectrum of possible results. In our case this implies that the orientation of *both* measurement systems must also be specified. As we have indicated, this implies the existence of non-local interactions. In §5.2, 5.3 we consider in detail the concept of non-locality in general, and how it applies

1. We anticipate that some readers will object to our condition for a H.V. theory. However, in view of the generality of this condition, and the fact that H.V. theories thus far presented (as well as some other interpretations not usually called H.V. theories) fulfill it, it is difficult to imagine an interpretation which could be called a H.V. theory on intuitive grounds, and which does not fulfill our condition.

in this case, in particular.

4.4 The Classification of H.V. theories

Belinfante (1973) divides H.V. theories into three kinds. Those of the 'zeroth kind' are those which are self-inconsistent. Belinfante notes that self-inconsistent theories can always be formulated, and cites as an example the theories which von Neumann refers to in his 'proof' of the impossibility of H.V. theories.

H.V. theories of the first kind are those which are not self-inconsistent, and which reproduce the predictions of Q.M. exactly, when the hidden variables are randomly distributed. The results of our proof above indicate that, if we take the exclusion of non-local interactions as a necessary axiom for physical theories, this class of H.V. theories is empty. We take this opportunity to note once more that the requirements on a physical theory are not purely logical. Certainly, where deductive arguments occur, they must satisfy the requirements of logic. However, these requirements do not apply to our choice of axioms or regulative principles. From this point of view, Belinfante's classification of H.V. theories as of the zeroth kind is a ploy to make refutations of H.V. theories seem less acceptable. Any refutation of a H.V. theory of the first kind cannot be on empirical grounds without simultaneously being a refutation of Q.M., and a H.V. theory which is refuted on the grounds of logical inconsistency must be of the zeroth kind! The failure of von Neumann's 'proof' is not that it deals with H.V. theories of the 1st kind; it lies in the fact that his postulates for a H.V. theory are unnecessarily restrictive.

Finally, H.V. theories which are classified as of the second kind by Belinfante have predictions which are different from those of Q.M., particularly in cases like the experiment considered in §2.2 in the treatment of the E.P.R. paradox and in §4.3 above. If such theories are

feasable, Q.M. must be empirically incorrect. This means that experimental evidence favouring either Q.M. or H.V. theories of the second kind should, in principle, be obtainable. We consider attempts at obtaining this evidence in §4.5 below. We can write the results of our proof in §4.3 in terms of this classification in two different ways, depending on whether or not we consider the exclusion of non-local effects to be a necessary axiom for a physical theory.

If we accept this exclusion, our result is equivalent to the statement that no H.V. theory of the first kind exists. H.V. theories must either be of the zeroth kind (and therefore unacceptable) or of the second kind, in which case they are empirically testable.

Alternatively, we can say that all H.V. Theories of the first kind must include non-local effects.

In a recent paper Lochak (1976) has criticised Bell's (1964) proof of our results on the grounds that the H.V. theories which he considers are of the first kind i.e. that Bell's concept of a H.V. theory is unnecessarily restrictive. Whether or not this criticism applies to Bell's proof is beside the point, since it clearly does not apply to Wigner's derivation of the same result as we present it here. Lochak also mentions a criticism due to de Broglie of the use of the 'singlet state' in describing the experiment considered here and in §2.2. In using this formulation, however, we have simply followed the dictates of the physical formalism of Q.M. Further, the usage of this formalism can be independently confirmed by the experimental test of H.V. theories of the 2nd kind (§4.5).

4.5 Hidden Variables Theories of the 2nd kind

The proof presented in §4.3 that H.V. theories must be non-local rests, partly, on the assumption that the predictions of such theories must agree with those of the physical formalism of Q.M. This is not unreasonable in the face of the general success of Q.M., but the correct requirement on a physical theory is that it agree with the 'facts' i.e. the experimental results. Thus, in order to avoid the consequences of the above proof, it has been postulated that Q.M. is empirically incorrect in cases like the experiment considered in §4.3. This postulate is, of course, experimentally testable, although such tests prove to be much more difficult than is suggested by the simplicity of the 'gedanken-experiment' which we considered.

The first experiments attempted involved the measurement of correlations between the polarizations of spatially separated photon pairs, and the comparison of the results obtained with those predicted by ordinary Q.M. and those necessary for a local H.V. theory. We do not propose to treat these experiments in detail, since, for our purposes, the results will suffice. Details can be found in the original papers as well as in reviews such as those presented by Belinfante (1973) and Shimony (1971).

The first to attempt this kind of experiment were Kocher and Commins (1967). Their experiment proved to be inconclusive, due to technical difficulties, but an improvement suggested independently by Clauser, and Horne and Shimony (See Clauser et al (1969)) and carried out by Freedman and Clauser (1972) gave conclusive results in favour of Q.M. However, a similar experiment performed by Holt (see Belinfante (1973)) yielded tentative results in favour of a local H.V. theory (L.H.V.) Kasday, together with Wu and Ullmann, devised another photon-correlation experiment (Kasday (1971)) which gave results in favour of Q.M. However, Bell devised a counterexample of a L.H.V. theory which would agree with their results. Kasday notes that his results are in agreement with those of Wu and Shaknov (1950) Bertolini et al (1955) and Langhoff (1960).

In 1976, a conference was organised by J.S. Bell and B. d'Espagnat ("Workshop on Experimental Q.M., 19 - 23 April, 1976 at Erice, Sicily"¹) to bring together physicists working in this field and to try and settle the question for once and for all. E.S. Fry (1976), presented results of an experiment similar to that of Clauser and Freedman, and the same as that of Holt. (Using the 1 1 0 transitions in mercury) which unequivocally favour Q.M. Clauser and Horne presented their result which favours Q.M. being ~ 6 standard deviations away from the predictions of a L.H.V. theory. F.M. Pipkin et al performed a similar experiment which favoured a L.H.V. theory. However, he pointed out that every source of error moved the result away from the Q.M. predictions and towards those of a L.H.V. theory. He had twelve such sources of error!

J. Ullmann measured correlations on γ -radiation using Compton scattering to determine polarizations, and obtained results supporting Q.M. S. Notarrigo performed a similar experiment which gave poor agreement with a L.H.V. theory.

R. Ringo proposed an experiment involving low-energy proton-proton scattering, which yields a 98% singlet-state. This experiment is nearest to the gedanken-experiment presented in §4.3 and so provides a possible 'direct' test of the use of Q.M. in this case. A similar experiment has been performed by M. Lamehi-Rachti who obtained results in agreement with Q.M., two standard-deviations away from the predictions of a L.H.V. theory.

Thus, we can see that, although there is some disagreement and this result cannot be taken as totally conclusive, available research indicates that the predictions of Q.M. in this case are correct, and that a L.H.V. theory is not possible. This was

1. Details from D. Bedford, private communication.

the general feeling of those attending the conference (especially Bell). In order to come to conclusive results and to check all possibilities exhaustively, it was agreed that experiments should be performed in which polarization correlations in *random* directions are measured. A. Aspect suggested a method using Kerr-cells whereby this could be done¹. D. Bedford expressed his belief that this had, in essence, already been done in the experiment due to Ullmann.

Because our ultimate aim is not that of d'Espagnat (1971) (ours is to understand microphysical processes, his was to interpret non-relativistic Q.M.) we cannot use his ploy and disregard the possibility of a L.H.V. theory as outside the subject under discussion. Therefore, we eagerly await a decisive result, one way or the other. However, it certainly seems as though the majority of experiments indicate that Q.M. is correct and that any L.H.V. must be incorrect. We make this assumption in the rest of the present work, but re-emphasize the fact that, if a L.H.V. theory is found to be correct on empirical grounds, Q.M. and many of our interpretations on the basis of Q.M. will be empirically false.

1. This method has already been suggested by Clauser. See Shimony (1971).

Chapter 5

LOCALITY, COMPLETENESS AND DETERMINISM : GENERAL CONCLUSIONS

5.1 Introduction

It seems, on the face of it, that instead of clarifying the possibilities for a realist interpretation of Q.M., the analysis of the last three chapters has led us to a dilemma. The result of Chapter 2 is that if we assume that Q.M. provides a complete description of (microphysical) reality (which must therefore be non-deterministic), non-local effects must exist. In order to escape the 'artificial and unpleasant' features of such interactions, we might suppose that Q.M. is not complete. If we then attempt to 'complete' the description provided by Q.M. by constructing a H.V. theory (on the assumption that microphysical processes are deterministic) we find once again that we are forced to assume the existence of non-local effects! If we maintain strictly the regulative principle that non-local effects do not exist, we are left with only one possibility: Q.M. must be incomplete (to avoid the non-locality implied by the arguments of Chapter 2) *and* the behaviour of microphysical systems must be non-deterministic (to avoid the non-locality implied by the existence of a H.V. theory). This unfortunate conclusion, while it *may* avoid non-local interactions, certainly complicates our aim of using Q.M. to induce and explain the properties of microphysical reality. Even if this reality were not completely described by Q.M., we could still find out something about it by assuming its behaviour to be deterministic¹. If this latter assumption is also excluded, it is difficult to see how we could go about constructing a complete theory on the basis of Q.M. It is not even certain that such a theory would not also include non-local interactions. In view of our unfamiliarity in dealing with non-deterministic situations, too, it would seem advisable to reconsider such a drastic step.

1. We could augment Q.M. to form a (complete) H.V. theory, and then investigate microphysical reality using this theory.

For these reasons, we should re-examine our insistence that non-local interactions are unacceptable. It is no longer possible to delay a detailed analysis of the concept of non-locality, which plays such an important part in this context. It is surprising that an analysis such as that which is presented in §5.2 below has not appeared in the literature concerning the E.P.R. paradox, H.V. theories, and related topics. In §5.3, we compare the non-local effects as they occur in a complete Q.M. and in H.V. theories. As a result of this comparison, we decide in favour of the assumption that Q.M. is complete and non-deterministic. In the rest of this chapter, we deal with the consequences of this decision.

5.2 Non-Locality: The problem of definition

As we have seen, the notion that non-local interactions are unacceptable has played a crucial role in our analysis so far. This regulative principle is widely accepted, and under certain circumstances, may even be a criterion for the 'analyzability' of physical systems: if non-local interactions are present, it may not be possible, even in principle, to consider any physical system (other than the entire universe) to be isolated. Nevertheless, non-local or seemingly non-local interactions have been employed in certain physical theories. e.g. The interaction between the distant stars and local matter in Mach's hypothesis concerning inertial frames (these forces could be local, since they are represented by an essentially 'static' potential) and in those theories of electromagnetism (due to Wheeler and Feynman (1945, 1949)) employing advanced and retarded potentials. Still, we agree with d'Espagnat (1971) who finds that these interactions are 'at least artificial and unpleasant'.

Despite the general intuitive understanding of the concepts of locality and non-locality, it is, as is characteristic of regulative principles, difficult to describe or define these concepts unambiguously. Consider the following frequently quoted statement on the subject by Einstein (1949): "On one supposition we should, in my opinion, absolutely hold fast : the real factual situation of the system S_2 is independent of what is done with the system S_1 , which is spacially separated from the former". (p.85).

Now, in the absence of any further specification of how we are to interpret the term 'system' and in particular, its spacial extent, it is reasonable to assume, from a common-sense point-of-view, that the sun and the earth are spacially separated systems. However, any theory in which it was asserted that the real factual situation on the earth is independent of what occurs at the sun would be plainly unacceptable and absurd. The interaction between the earth and the sun, although they are spacially separated, need not be 'non-local' at all!

The following definition of the locality principle is due to d'Espagnat (1971):

"If a physical system remains, during a certain time, mechanically (including electromagnetically, etc) isolated from other systems, then the evolution in time of its properties during the whole time interval cannot be influenced by operations carried out on other systems" (p.114).

Here, the onus of the definition is placed on the concept of 'mechanical isolation'. If this concept is made explicit, it should mean the absence of any of the four known types of interaction (i.e. electromagnetic, gravitational, strong nuclear and weak nuclear). This definition is restrictive in that it excludes the possibility that some new *local* interaction may yet be discovered: any new interaction type would be non-local by definition. This presumptuous restriction is more in keeping with the confidence and faith of

nineteenth century physics than with the present scientific climate.

A second objection arises from the fact that electromagnetic and gravitational interactions have an infinite range. The class of isolated systems thus defined would be empty, and there would be no possibility of checking whether or not a particular interaction was local. This latter problem can be avoided by relaxing the condition of 'mechanical isolation' to 'significant mechanical isolation': the possible effects due to the four known interaction types can be calculated, in principle, and compared with any alteration in the behaviour of the system of interest. If the effect of these interactions is found to be negligible compared with the effects to be accounted for, the system could be termed 'significantly isolated,' and the change in behaviour ascribed to a non-local interaction. This leaves us with the former difficulty: that new local interactions are excluded. If we strengthen the condition on isolation to apply to any interaction, whether of the four known types or not, the whole statement reduces to a tautology: when a system does not interact (in any way) with other systems, then its behaviour is not affected by what is done to other systems. This does not exclude the possibility of non-local interactions since, if they occurred, the system would not be isolated under this definition of mechanical isolation.

We stress that these problems of definition are non-trivial, even though they may appear to be purely pedantic. They do not relate merely to a poor choice of terms by the authors considered here, but represent real problems in finding a statement which corresponds to our intuitive idea of what constitutes a non-local interaction. In some ways, a definition like that of d'Espagnat, in terms of 'significant mechanical isolation' comes closest to our aim of specifying (even *a Contrario*) a non-local interaction.

However, we should not be so presumptuous as to exclude the possibility of the existence of *local* interaction types which are, as yet, undiscovered.

It is significant that, in both passages quoted above, consideration is given to the intrusion of an 'operator', who carries out operations on or does things to the environment of the system under consideration. Both formulations can be made without explicit mention of or intervention by an operator, as we have indicated by our treatment. On the face of it, it is preferable to exclude any conscious intervention in the behaviour of physical systems since physical theories do not usually deal with conscious 'systems' or operators¹. Nevertheless, we feel that these authors have been influenced to include this 'non-physical' entity (i.e. a system that has 'intent') by the fact that our notions of interaction, non-local interaction in particular, are dialectically dependent on our notions of causes and causality². This can be illustrated as follows: if system A interacts with system B, then, in some sense, either events in A *cause* events in B to occur, or vice-versa. Similarly, if events in A cause events in B to occur, in any direct or physical fashion, we may conclude that systems A and B interact.

Now, although this may be open to criticism, we contend that the most certain (and perhaps only) way of ascertaining whether or not one event α , *causes* another, β , is to bring about α in many different environments, and see whether or not β subsequently occurs. If we can demonstrate the 'transmission of intent' (i.e. ensure that α occurs, intending that β should occur), under diverse conditions, we can be sure that the relation between α and β is causal, and not one of

1. A notable exception is the theory of measurement due to Wigner (1967) and London and Bauer (1939). See, in this regard, Chapter 8.
2. We distinguish between causality and determinism. See §3.2.

constant conjunction only (as, say, the common effects of a single cause). Likewise, if α occurs in system A and β in system B, we can be sure, in this case, that A and B interact. By including an operator (i.e. an experimenter) the authors quoted have indicated how we can know that an interaction occurs, and simultaneously, if unwittingly, provided the key to achieving an unambiguous definition of locality.

The transmission of intent is one type of information transfer i.e. the information that α has happened, by design, and hence, that β must occur. It is a result of the special theory of relativity that information transfer cannot proceed at a speed greater than that of light *in vacuo* i.e. events that are separated by a space-like interval (in the sense of special relativity) cannot be causally related. This conclusion follows from that fact that, due to the relativity of simultaneity, the time ordering of two events which are separated by a spacelike interval is not absolute, but depends on the state of motion of the observer. i.e. Two events, α and β , which are separated by a space-like interval will appear to occur in the order α first and then β from some rest frames, whereas from others, β will appear to occur first. If we assume that α and β are causally related in that α causes β , say, then when α is seen to precede β , all is well. On the other hand, if β precedes α , one of the fundamental conditions on causal relationships, that causes precede effects, is violated. Since we can always view α and β from a rest frame from which β is seen to precede α , this represents a serious difficulty. In the special theory, this difficulty is dealt with by assuming that events separated by a space-like interval cannot be causally related.

The prohibition on effects preceding causes stems, in turn, from several sources. Firstly, there is the empirical consideration that, no matter what procedures have been tried, nobody has been known to influence events that have already occurred! (It is difficult to conceive of how a claim to have done this could be checked).

Secondly, if effects could precede causes, a logical impasse of the kind where a hard-hearted (and foolish) logician kills his mother prior to his birth would be possible!¹ Finally, we would be able to see a sequence of events of the following kind: a bomb explodes, and thereafter a man says "I think I will detonate the bomb, after all" and then presses the detonator. Such a chain of events casts serious doubts on our subjective belief in free-will; once the effect has occurred, the cause *must* occur, whether the man (the transmitter of intent) has made up his mind at that moment (in the observer's rest frame) or not!

All these considerations give rise to a most stringent prohibition on the occurrence of effects prior to their causes. Note, however, that this does not, in itself, imply that no interactions over a space-like interval can occur. The difficulties mentioned above are only to be found in the case where interactions by means of which intent can be transmitted occur over a space-like interval. If an interaction were of such a kind that it was impossible to transfer information (in the sense of a message) there would be no reason, *a priori*, for excluding it. This is the case, for example, in theories involving advanced and retarded electromagnetic potentials. These act in such a way that any attempts at the transmission of intent are doomed to failure. For details see e.g. Davies (1974) Chapter 5.

We note that the cases of non-local interaction which we have encountered thus far share the property that they relate events which are as far apart spacially, and as close in time as we like i.e. they are separated by a space-like interval. A causal relation between such events is prohibited in special relativity. Using these concepts, we can construct a definition of non-locality which is both precise and unambiguous.

1. This would take at least two non-local steps since the events are on the same world line.

If events α and β are separated by a space-like interval (in the sense of special relativity) then any interaction relating them must be non-local.

The problem with this definition is, as was raised by Capri (1975) (see §2.6), that no acceptable relativistic generalization of Q.M. exists. (Our discussion is specifically restricted to non-relativistic Q.M.). However, in view of the difficulties associated with alternative definitions, this definition can be useful for the following reasons:

i). If an interaction between systems described by Q.M. *did* occur over a space-like interval, we could 'amplify' these effects to a macroscopic scale by a process such as measurement. Indeed, if Q.M. is to provide a universal description of microscopic phenomena, and the basic tenets of the theory of atomism are to remain valid, all macroscopic events should be related to each other, in some way, by interactions in the domain of Q.M. We could therefore expect to find macroscopic events which are causally related, but separated by a space-like interval. This would represent an empirical falsification of the special theory of relativity, a theory which has been found to apply universally to macroscopic events, and is believed by many physicists to provide an adequate description of reality at this level. Nevertheless, we cannot escape the fact that it is formally inconsistent to use the theory of special relativity in discussions relating to non-relativistic Q.M.

ii). The non-local interactions described in Chapter 2 and 4 are indeed between systems separated by a space-like interval¹ when viewed from the perspective of special relativity. We believe

1. Technically, we should write ".... between events separated by a space-like interval occurring in each system" but we feel that our usage is clear.

that all events thought, on intuitive grounds, to be related by non-local interactions are separated by space-like intervals. Hence, without considering the special theory of relativity to apply formally to the situations we consider, which are subject to Q.M., we can use the concepts of this theory as an *indicator*, to point out situations which are non-local, (and hence unacceptable) from an *intuitive* point of view.

iii). Finally, we note that, in a choice between alternative interpretations, each subject to regulative principles which are mutually inconsistent, we are restricted to the use of persuasive arguments only. If one interpretation satisfies this requirement of special relativity whereas an alternative does not, this simply adds weight to the case against the second interpretation. Since, as we mentioned in §1.2, we cannot hope for a logically rigorous distinction between two such interpretations, this informal usage of the concepts of special relativity cannot destroy the logical rigour of the argument. As we have repeatedly pointed out, the problems of interpretation of a physical formalism are *physical* and not *formal*. This application of special relativity can be seen as a *formal indicator* of a *physical* objection (the existence of (intuitively) non-local interactions).

5.3 The Case Against Hidden Variables Theories

In order to avoid the conclusion that Q.M. is an *incomplete* description of a *non-deterministic* reality, we must reconsider the exclusion of non-local interactions. If we accept such interactions unreservedly, we cannot conclude from the E.P.R. argument (§2.2 and 2.4) that Q.M. is not complete. Neither can we use the results of §4.3 to exclude the possibility that hidden variables theories exist. However, there is an essential difference between the non-localities involved in either case that, together with other considerations, allows us to exclude the possibility of H.V. theories and assume that

Q.M. provides a complete description of microphysical reality (which behaves non-deterministically).

We showed in §4.3 that, in the experiment considered there, the *result* of a measurement of spin component on one particle depends on the direction in which the spin component of the other particle *is measured*, and concluded that the interaction whereby the direction of the measurement on the first particle is transmitted to the site of the measurement on the second must be non-local. From our analysis in §5.2 above, it follows that, provided the 'transmission of intent' can take place via this interaction, we should be able to show that some extremely unacceptable phenomena, including a violation of causality and our notions of 'free-will' can occur. It remains to be demonstrated that this interaction can be used to transmit intent.

This can most easily be done by considering a 'gedanken' experiment. Consider, as in the previous arguments, the case of a spin-zero particle which decays into two oppositely-directed spin- $\frac{1}{2}$ particles via a spin-conserving decay. The state of the combined system (consisting of both decay products) after the decay is the singlet state

$$|\psi\rangle = 2^{-\frac{1}{2}}(|u_{z+}\rangle|v_{z-}\rangle - |u_{z-}\rangle|v_{z+}\rangle) \quad 5.1$$

where z is any direction in the plane normal to the direction of propagation, and all symbols have the same meaning as in Equation 2.5. Suppose, further, that Q.M. is incomplete, and that the actual physical situation is further specified by the value(s) of hidden variable(s) in each case. It is impossible (at the present time) to measure the value(s) of the hidden variable(s) in a particular case, or to prepare a system repeatedly with the same value(s) for the hidden variable(s)¹. If an acceptable H.V. theory exists, however, there is

1. This would give rise to results in conflict with those of Q.M. (i.e. dispersion-free states).

no reason, in principle, why one of these procedures should not become possible in the future, due to refinements in preparation and measurement techniques. i.e. The problem with specifying the values of the hidden variables in each case is a technical one. If this were not so, and we found ourselves unable, in principle¹ to specify the values of hidden variables as part of state preparation, H.V. theories would become even less acceptable from the point-of-view of the principle of economy of postulates (Occam's razor) and completely unacceptable to the fasificationists.²

Thus, we assume that, for the limited purpose of a 'gedanken' experiment (which deals only with matters of *principle*), the value(s) of the hidden variable(s) can be known or specified at the outset of the experiment. Now the specification of the quantum state and the hidden variables cannot be sufficient to determine the outcome of any spin component measurements on the second particle. We must also specify the direction in which the spin component of the first particle is measured. This, in turn, depends on the orientation of the magnets of the Stern-Gerlach (S.G.) apparatus (spin component measuring device) at the site of the measurement on the first particle. Hence, by influencing the orientation of these magnets (by means of a lever, say) an experimenter can influence the outcome of a measurement on the second particle even if the two events (aligning the S.G. magnets at the site of measurement on particle one, and performing the measurement on the second particle) are separated by a space-like interval (i.e. the time between them is less than the distance between them divided by the speed of light in vacuo). Hence, if H.V. theories which are consistent with Q.M. exist, then we can 'transmit intent' over a space-like interval.

1. As we mentioned in §4.2, this is already impossible in practice.
2. This argument indicates our rejection of any H.V. theory involving a 'conspiracy' to produce the particular results obtained in a given measurement.

In order to make explicit the unacceptable consequences of this conclusion, and in honour of Schrödinger, we consider a situation where the orientation of the S.G. magnets used in measurement on the second particle is fixed in, say, the "up/down" direction. If the particle is detected in the upper path (i.e. with spin 'up'), a sequence of events takes place which results in the death of a cat (in much the same manner as in Schrödinger's inhumane experiment). If it is detected in the lower path, the cat will remain alive. The S.G. apparatus used in measurement on the first particle is mounted so that it can be turned about an axis along the 'path' of the particle. For given values of the hidden variables, the possible orientations of this apparatus can be divided into two: those which give rise to the result 'up' and those which give rise to the result 'down' in the measurement on the second particle. By aligning the S.G. magnets in one or other of these directions an experimenter can kill or not kill the cat.

Suppose, initially, that the measurement apparatus are equidistant from the site of decay¹. Then the experiment could proceed as follows: an assistant at the site of the decay informs the experimenter that a decay has occurred (and, if necessary, the values of the hidden variables) by means of some signal which travels faster than do the particles (e.g. radio). Having calculated, previously, the maximum time at his disposal before the first particle reaches his apparatus, the experimenter waits as long as possible and then makes up his mind and aligns the S.G. magnets intending, say, that the cat shall die. If the magnets are sufficiently far apart, his decision and the cat's death (which will only be certain if H.V. theories are correct) will be separated by a space-like interval. Thus, an

1. For simplicity, we assume that both decay products travel at the same velocity.

observer moving by at a suitable velocity will see the cat die before the experimenter aligns the magnets to cause its death, and even before the experimenter had made up his mind! It is not even necessary to view the experiment from a different rest frame to observe these absurd phenomena. If we suppose that the S.G. apparatus and the cat are much closer to the site of decay on one side than is the experimenter and his S.G. apparatus on the other, the measurement on the second (cat's) particle will take place *before* the measurement on the first particle in the laboratory reference frame. In this case, there is no reason to suppose the result of measurement on the second particle suddenly becomes independent of the direction of the measurement of the first. In our proof (§4.3) we did not consider the time-ordering of the measurements at all. In this case, from the laboratory frame of reference, we would see the following sequence of events: the decay occurs and the experimenter is informed; the cat dies (survives); the experimenter decides to kill (reprieve) the cat; he aligns his S.G. accordingly. Sequences like this, if they are not logically unacceptable, at least cast serious doubts on our belief in our own capability of making decisions (free-will); once the cat has died, the experimenter *must* decide to kill it! If the cat survives, he *cannot* thereafter decide to kill it.

If the time interval between the two events 'cat dies/survives' and 'experimenter decides' can be made long enough (by ensuring that the decay products have low velocities) there will be time to inform the experimenter of his choice *before* he had made it! What would happen, then, if he decided to be contrary and choose the other alternative? If the experimenter and his assistant had decided to do this (contrary choice) beforehand, then either the H.V. theory would be shown to be empirically incorrect (if the experimenter succeeded) or else the assistant would see his colleague consistently breaking their decision and conclude that the latter had gone insane (or unscientific, to say the least)!

Clearly these results are absurd, and must be avoided. i.e. We cannot accept the type of non-locality concomitant with H.V. theories as an actual physical phenomenon. In contrast, we consider the non-locality implied, in terms of the E.P.R. argument, by assuming that Q.M. is complete. Here, it becomes possible, on the basis of a measurement of the spin component of the first particle in a given direction, to predict the result of a spin component measurement on the second particle in the same direction. Since the measurements on the two particles can occur over a space-like interval, the interaction whereby this prediction for the second particle becomes possible must be non-local. However, we note that, in this case, there are no operations which an experimenter at the site of measurement on the first particle can perform which can affect the result of measurement on the second particle directly. The occurrence of non-local effects can only be shown by comparing results after the experiment. For example, if the assistant finds a result 'down' for particle two, he can make no inference about any activity of the experimenter at the site of measurement on the first particle. He cannot tell whether or not measurement on the first particle has occurred. Neither can he infer in which direction such a measurement, if any, was made. The only criterion we have as to which measurement (i.e. on particle one or two) *caused* the spin of the other particle to be predictable with certainty (in a given direction) is the time-ordering of the measurements. If, on subsequent comparison of results, it is found that the measurement on the first particle occurred *prior* to that on the second, then we say that the measurement on the first particle *caused* the result of a measurement on particle two in a given direction to be predictable with certainty. If the measurement on the second particle occurred first (in the laboratory reference frame) then we say that it is the cause of the polarization of the spin of the first particle. Since no 'transmission of intent' is possible via this interaction, there is no other means of making the choice between 'cause' and 'effect'.

Suppose that, in a given instance, the following sequence of events occurs (in the laboratory reference frame): the decay occurs; a measurement is made on particle one and the result 'down' is obtained; a measurement is made on particle two and the result 'up' is obtained. The last two events may be sufficiently far apart, spacially, and close together in time to be separated by a space-like interval. In this case, an observer moving by at a suitable velocity could, if we accept the postulates of special relativity, see the order of these events as reversed. i.e. He would see the following sequence: the decay occurs; a measurement is made on particle two and the result 'up' is obtained; a measurement is made on particle one and the result 'down' is obtained.

In both of these cases, the observer sees a perfectly acceptable sequence of events (i.e. no observer sees a cat die and then someone saying "I suppose I will kill the cat, after all," and taking the requisite steps). The observer in the first case will say that the measurement on particle one caused the result for measurement on particle two to be determined whereas, in the second case (i.e. from a moving reference frame) the measurement on the second particle will be the 'cause' and the result of measurement on the first particle the 'effect'. The two observers will disagree in exactly the same way as they disagree about the time ordering of the two measurements. This must be so since this time ordering is the only criterion for applying the names 'cause' and 'effect'. We call the causality in this case *'relative causality'* to distinguish it from the causality involved in the transmission of intent, where cause and effect are distinguishable independently of time-ordering; the cause is that event which occurs via 'direct contact' with the system which possesses intent (the experimenter) and the 'effect' is the event that is intended. We also call the information transferred by means of relative causality *'virtual information'*. This distinguishes it from the 'real information' which carries a 'message' or which transmits intent.

As we saw in §5.2, there is no a priori reason to exclude the transmission of virtual information over a space-like interval, since causality and logic are not violated.

If any further evidence against H.V. theories is required, in the face of the impossible consequences outlined above, we need only recall that they not only need to be enormously complicated, but they are also empirically untestable. Thus, the only advantage of this complication is the retention of the classical notion of determinism, without any basis in experiment. As we noted in §4.1, we may therefore dismiss H.V. theories on the basis of the principle of economy of postulates.

We conclude that a deterministic supertheory containing Q.M. (i.e. a H.V. theory¹) is untenable. Note that this does not imply that H.V. theories are *impossible*: anybody prepared to accept the concomitant non-locality (e.g. Landé) can construct H.V. theories to his/her hearts' content. This would involve a rejection of the notion of free-will in favour of some sort of fatalism² or else the development of a rationale as to the impossibility of measurement or preparation of systems to predetermine the values of the hidden variables.

1. Here we use the term H.V. theory as defined in §4.1. This includes some theories which are not usually considered to be H.V. theories (see §4.3) and excludes any 'non-deterministic H.V. theories'.
2. We consider that a 'conspiracy' theory of hidden variables involves the rejection of the notion of free-will.

5.4 A Complete Quantum Mechanics

Since we have rejected the notion that the behaviour of microsystems is deterministic¹, Q.M. *may* provide a complete description of such systems. As we have pointed out, a strict ban on non-local interactions of any kind would exclude this possibility, leading to the conclusion that Q.M. can, at most, provide an *incomplete* description of this non-deterministic behaviour.

However, we have seen that the non-locality involved in the assumption that Q.M. is complete does not violate the requirements of causality or special relativity. Also, by assuming that Q.M. is complete, we will be able to examine the phenomenon of non-deterministic behaviour. Without this assumption, we would be unable to use Q.M. as a basis for understanding the detailed behaviour of microphysical reality, which remains out of the reach of an incomplete description. While we can easily see how to 'complete' Q.M. on a deterministic basis (i.e. by formulating H.V. theories), this is not so simple to imagine, now that we have shown that the behaviour of microsystems is non-deterministic.

For these reasons, then, we assume that Q.M. provides a complete description of microphysical reality, and leave the alternative as a possible starting point for further research. This means, first of all, that we must accept the non-local transfer of virtual information as a new phenomenon.

1. This applies only to the determinism achieved by 'splitting the cause'. i.e. The situation as seen by an observer in a single universe (w.r.t. the Many Universes Interpretation) dealing with a 'single system'.

Although it has a certain artificiality about it, we may expect something like this to be necessary in the description of non-deterministic phenomena: if a description of the non-deterministic time-development of a system is to encompass all of the possible outcomes we should *expect* something strange to happen when one possibility is fulfilled and the others vanish. Nevertheless, we note that, by extending our notion of 'system', we can achieve a description of the transfer of virtual information in which non-locality need play no part. Consider, again, the singlet state given in Equation 2.5 and 5.1. There is no way in which this state can be written as a product of a vector in Hilbert space $\mathcal{H}^{(u)}$ with one in $\mathcal{H}^{(v)}$, although the state is in the outer product space. This means that Q.M. does not give the states of systems U and V separately in this case. Since we have assumed that the Q.M. description is complete, we can interpret this to mean that U and V do not have separate states: *there is no actual physical situation pertaining to system U alone, nor to system V alone.* The only system which we can consider is the combined system (U + V). This means that, in talking about 'particle 1' and 'particle 2' or 'system U' and 'system V' separately, we have been making a mistake. These systems do not exist independently. Instead of two one-particle systems, we must consider one irreducible two-particle system! From this point of view, the 'two systems' between which a non-local transfer of virtual information was shown (in §2.2 and §2.5) to occur, are, in fact, one system only. This involves an extension of the classical notion of a system, as well as that of atomism (see §1.5). Clearly, if Q.M. is complete, we must accept the existence of 'fundamental systems', the components of which do not have independent existence. In this case, one measurement on the system can be expected to affect the result of another (independent of their relative situations in space). Formally, this amounts to questioning the

assertion of E.P.R. (1935) that two systems can cease to interact: although we have no classical basis for an interaction between U and V in this case, their behaviour cannot be independent because they are both non-separable components of the same system. Here, we are faced with a novel (non-classical) aspect of Q.M. which gives rise to difficulties in that there is no well-known way to describe it in our language. This means that it is no simple matter to ensure that our interpretation is consistently applied.

5.5 Non-determinism and probability

If we prepare a system with normalized quantum state

$$|\psi\rangle = \sum_n c_n |\psi_n\rangle \quad 5.2$$

where, for some hermitian operator A,

$$A|\psi_n\rangle = a_n|\psi_n\rangle \quad \text{for each } n. \quad 5.3$$

then the probability that measurement of the observable corresponding to A will yield a result a_n is $|c_n|^2$. If the result of a given measurement on a classical (deterministic) system was predicted as a_n with probability $|c_n|^2$, we would assert that, in any one case, the result would be determined, but a stochastic description arises from our *lack of knowledge* of the exact state of the system. For this reason, we consider an ensemble of systems, each subject to the same 'blanket' preparation, and determine the relative number of elements of the ensemble which would give the result a_n . This, we assert, is the probability $|c_n|^2$ that a_n will be found. Fundamental to this treatment is that, in each instance, exactly *one* element of the ensemble occurs, but we don't know which it is. i.e. We assume that each element in the ensemble is independent. The probability is an expression of the 'relative likelihood' that a given situation should occur.

In the case of a non-deterministic microphysical system, completely described by the quantum state $|\psi\rangle$, only one situation is possible at the outset of the experiment. The fact that probability enters into the description of the possible outcomes of measurement is *not* a result of our lack of knowledge. If we know the quantum state exactly, we know *all that there is to be known* about the system. This is a consequence of our assumption that Q.M. is complete. Probabilities occur, in this case, because the evolution of the system is non-deterministic. If we attempt to set up an ensemble of systems, as in the classical case, we find that the different elements of the ensemble *must interact*.

For instance, in §3.5, we saw that a system prepared with ' $|L\rangle$ or $|R\rangle$ with probability $\frac{1}{2}$ ' (i.e. an ensemble with non-interacting elements) behaves differently, in some experiments from a system prepared with $2^{-\frac{1}{2}}(|L\rangle + |R\rangle)$ (where the different elements interact).

Alternatively, each element of the ensemble must be the *same* (i.e. the situation which is 'completely' described by $|\psi\rangle$) and must include *all* possible outcomes. Here, probability plays a different role from that in classical theories. The probabilities $|c_n|^2$ are a 'property' of the state $|\psi\rangle$ in that they are determined by it. Since Q.M. is complete, they are a 'property' of the system. Now, we have seen (e.g. in §3.5) that microphysical systems cannot be thought of as 'having' classical properties. Nevertheless, in that the probabilities $|c_n|^2$ are uniquely determined by the state $|\psi\rangle$, we can say that the system described by $|\psi\rangle$ 'has' these probabilities as properties. Here, these probabilities, a 'property' of the microphysical system, may be described as the '*propensity*' that the system has, for yielding a given result upon measurement.

In contrast to the classical case where the probabilities are a property of our incomplete *description* of a physical system, the probabilities $\{|c_n|^2\}$ (or, more accurately, the amplitudes $\{c_n\}$) can be regarded as a property of the *physical system*, in the same sense as the mass of a classical system is considered to be a property of the system itself. In contrast to the classical case where the probabilities can be changed, in principle, by obtaining more information, quantum mechanical amplitudes and probabilities are *absolute* properties determined by the quantum state. Once the Q.M. state has been specified, *there is no more information to be gained*.

The absolute character of the probabilities occurring in Q.M. means that some of the philosophical problems relating to classical probability do not occur. In evaluating a 'betting theory' of probability, Ayer (1957) notes that, in dealing with the probability that a given horse will win a race, different people may arrive, quite justifiably at different results:

"....It makes judgements of probability at least partly subjective. If the stable guards its secrets well, the totality of the evidence that is available to me will fall short of the totality of the evidence that is available to the horse's trainer. Let us make the implausible assumption that both he and I are in fact possessed of all the relevant evidence that is respectively available to us, and that we correctly calculate the degree of confirmation of the hypothesis that Eclipse [the horse] will win, arriving naturally at different results. Both results will be valid, but the one that is valid for him will not be valid for me It follows also, on this view, that there is no such thing as *the* probability of a hypothesis: there are as many probabilities as there are persons who have access to different quantities of evidence."

He also points out that, if, in order to avoid this difficulty, we assume that:

".... everyone has access, in principle, to all the evidence that there is" we, run into the "fatal disadvantage that the probability of every hypothesis becomes either 0 or 1". This analysis does not only apply to horse-racing! Consider, for instance, the spinning of a coin: the assumption that each result (heads or tails, neglecting other possibilities) has a probability of $\frac{1}{2}$ of occurring depends upon an implicit assumption that the coin is spun in a sufficiently complicated way to reduce the evidence available to all concerned to the same level. However, somebody with a high-speed video-recorder could obtain sufficient evidence to make the probability of one outcome 1 and the other 0!

However, the relative nature of this probability, which can be reduced to 0 or increased to 1 without limit by obtaining more evidence (prior to the occurrence of the outcome) depends on the fact that the behaviour of classical systems (such as a coin) is deterministic. It is on the basis of determinism that Ayer assumes that sufficient evidence *exists*, prior to the occurrence of the outcome. In a non-deterministic situation there is an absolute limit on the amount of evidence available. For micro-systems this is the information contained in the complete quantum state. The probabilities that a system has a given, but unknown, quantum state have exactly the same properties as classical probabilities. However, once the state of the system is specified, the probability that a given measurement result, a_n say, will occur is absolutely determined for all interested parties as $|c_n|^2$. The subjective quality of this sort of probability disappears because there is no more evidence to be had.

This removes the objection against the probabilities which occur in quantum mechanical predictions being objective quantities. Müller (1974) has come to the same conclusion by means of a different argument. Although it may be argued that the predictions of Q.M. can only be tested by a sequence of measurements (i.e. measurements on many-particle systems) this does not necessarily imply that we must employ a 'relative-frequency' theory of probability. Although there are some difficulties associated with determining the probabilities for different outcomes by examining a classical system such as a die, without measuring the relative frequency of different outcomes or specifying the amount of evidence relating to the throwing process which is available, these difficulties do not occur for quantum mechanical probabilities. The latter are absolutely specified by determining the quantum state of the system. This may be done by considering the preparation procedure, and without recourse to the measurement of relative frequencies.

As a result of these considerations, we can dismiss the claim of Belinfante (1975) and others that probability concepts are only meaningful with respect to sequences of events (on which relative frequencies can be measured) and hence, that Q.M. can only deal with many-particle systems (See § 3.4). We can therefore use the physical formalism of Q.M. to investigate the behaviour of 'single systems'.¹

5.6 Non-determinism and State Reduction

In §5.3, we come to the conclusion that the behaviour of microsystems (as described by Q.M.) cannot be deterministic. From this and the result of §3.3, we must conclude that the full time-development of such systems cannot be described by a function. i.e. The relationship between the actual situation at

1. In this regard, see also §7.4.

one time and at later times must be many-valued and hence cannot be described by a single-valued function. Since Q.M. is complete, whatever is true for the actual situation pertaining to a system must be true also for the quantum state describing that situation. From different measurement results obtained, in general, on systems prepared with the same quantum state, we infer that the actual situation of the system after measurement (if it still exists) is different in each case. It follows from the completeness of Q.M. that these different situations must be described by different quantum states. i.e. A single quantum state must evolve, in general, into one of several different quantum states. Not only the actual situations, but the quantum states, too, must evolve non-deterministically.

How is it, then, that the time development of Q.M. states is described by a functional relationship as defined by the Schrödinger equation or the action of a unitary operator? The relationship thus defined is single-valued and deterministic. To answer this question, we refer to paragraph (iii) of §3.4. Here we considered two ways of dealing with an inherently non-deterministic situation. In assuming that Q.M. is complete, we reject the first alternative; that the relationship between earlier and later states of a system must at best be described by a many-valued relation, as defined by the physical formalism of Q.M. This leaves us with the second alternative; the development of a quantum state (and hence a microphysical system) can be described deterministically, i.e. by the Schrödinger equation, *provided no measurement takes place* on the system. However, when measurement takes place, or when certain conditions on the quantum state are fulfilled, a non-deterministic transition occurs.

Since Q.M. is complete, this transition must apply not only to our description in terms of quantum states, but also to the physical system itself. This transition cannot be described in terms of a function, since it is many-valued. Thereafter, the development of the system may, once more, be deterministic. This transition, which we call *state reduction*² was postulated by Von Neumann (1932) who recognized two fundamentally different ways in which quantum states change with time¹. The first, which is equivalent to our state reduction, is irreversible and occurs during measurement to account for non-deterministic changes. (This is 'intervention 1' in Chapter V). The other is just the ordinary deterministic development of quantum states as defined by the Schrödinger equation ('intervention 2' in Chapter V).

Many physicists will no doubt question the necessity of introducing the concept of state-reduction to account for non-determinism, especially since it only seems to be needed upon measurement. Surely the indeterminism could be introduced by the interaction of the system of interest with the measurement apparatus! In the following chapters, we shall show that this

1. It is ironic that, in assuming that Q.M. is complete, we are driven to the conclusion that an additional process must be postulated to describe the time development of quantum systems. This is another indication that the choice of the term 'complete' to describe the relationship between a theory and the reality which it describes is not particularly apt.
2. Other names referring to this, or a similar process are 'wave packet collapse' and 'reduction of the wave packet'.

is not possible, by describing the development of the measurement apparatus and the object system together.

Also, we note that, as we saw in §3.4, it is difficult to imagine how else non-deterministic behaviour could be described dynamically. Certainly, the non-deterministic transition (state reduction) cannot occur immediately after the preparation of a state since, in this case, it would make no difference to assume that the different outcomes of the non-deterministic transition were already present when the system was first prepared. It would therefore be possible to construct a H.V. theory¹ in contradiction to the conclusion of §5.3. It is equally certain that state reduction must occur prior to our perceiving the measurement results, otherwise we would not experience the behaviour of microsystems as non-deterministic at all. The fundamental problem in the interpretation of Q.M. which remains is firstly, to show that state reduction must occur, and secondly, to establish the conditions under which it occurs, in a conceptually and logically consistent fashion.

It is possible to link state reduction and the transfer of virtual information. Consider a system prepared with initial state $|\psi(0)\rangle$ which develops deterministically, with time, into the state $|\psi(t)\rangle = \sum_n c_n |\psi_n\rangle$ for where $A|\psi_n\rangle = a_n |\psi_n\rangle$ for A a hermitian operatorⁿ and $\{a_n\}$ real. Suppose that, at time t , an ideal measurement of the observable corresponding to A is made, and the result a_k is obtained. Then, after measurement, the system is described by $|\psi_k\rangle$ or, as time goes by, the state $|\psi_k(t)\rangle$ where $|\psi_k(t)\rangle$ follows deterministically from $|\psi_k\rangle$ by the

1. The only difference is that it would be impossible to measure the hidden variables or prepare them with given values. This may provide the rationale mentioned in §5.3.

action of a time development operator. (If the measurement is non-ideal, the state of the system after measurement may be given by $|\psi'_k\rangle$ and $|\psi'_k(t)\rangle$ respectively).

The transition $\sum_n c_n |\psi(t)\rangle \rightarrow |\psi_k\rangle$ is non-deterministic and represents the phenomenon of state reduction. Suppose A corresponds to position, and the $\{a_n\}$ signify different positions. In this case, the information that the system has been 'found' at a_k must be transmitted instantaneously to all the other positions, in order to ensure that the system is not found there also. (A single system can only have one position, on measurement). This is an example of the transfer of virtual information, over a space-like interval. In the decay of a spin-zero particle into two spin $\frac{1}{2}$ particles considered in the E.P.R. argument (Chapter 2) we would say that a measurement of spin component in the z - direction on one particle (U) giving a result 'spin component in the + z -direction', would cause the following state reduction:

$$2^{-\frac{1}{2}}(|U_{z+}\rangle|V_{z-}\rangle + |U_{z-}\rangle|V_{z+}\rangle) \rightarrow |U_{z+}\rangle|V_{z-}\rangle \quad 5.4$$

From this, it is clear that a measurement on the other particle (V) must give the anti-correlated result 'spin component in the -z direction'. The information that state reduction has occurred must be transmitted instantaneously to the site of the second measurement to ensure that the results are anti-correlated. From this point of view, the virtual information which is transmitted over a space-like interval (involving relative causality) is always that 'state reduction to such-and-such a state has occurred.'

5.7 Conclusion

In the first part of our analysis (Chapters 2, 3, 4 and 5) we have shown the following, without, as far as we can see, making any choice in favour of or against any contentious regulative principle. i.e. We hold the following to be valid, independently of the persuasive arguments in favour of or against a particular regulative principle¹.

- 1). Either Q.M. is not a complete description of (microphysical) reality or else non-local interactions must exist (§2.2, §2.4). However, we showed in §5.2 and §5.3 that this type of non-locality, which cannot be used to transmit intent, is not incompatible with the theory of special relativity, nor our notions of causality. It does not bring into question our belief in 'free-will'.
 - 2). If Q.M. (or any other stochastic theory) is complete, then the behaviour of the reality which it describes cannot be inherently deterministic. Conversely, if the behaviour of this reality is deterministic, Q.M. cannot be complete (§2.4).
 - 3). If the behaviour of reality (in the domain of Q.M.) is deterministic, (i.e. H.V. theories exist) and the predictions of Q.M. in certain instances (e.g. the decay of a spin-zero particle into two spin- $\frac{1}{2}$ particles, with spin-component measurements thereon) are empirically correct, then non-local interactions must exist (§4.3). In §5.2 and 5.3 we
1. These conclusions are, however, meaningless unless we reject the positivist or empiricist position and adopt some sort of realist stance. This could be seen as the adoption of a (primal) contentious regulative principle.

showed that this type of non-locality is incompatible with special relativity. Even without the assumptions of special relativity, experiments are possible, in principle, which violate our notions of causality and free-will.

4. The time-development of a non-deterministic phenomenon cannot be fully described by a function (or any mathematical relation with the property of single-valuedness) (§3,3).

Using these results, we have come to certain conclusions, by choosing some regulative principles in favour of others:

1. As a consequence of result 3 and the conclusions of §4.5 that the predictions of Q.M. are correct, we conclude that the behaviour of microphysical systems cannot be inherently deterministic^{1,2}. This conclusion is partly on the grounds of our belief in the existence of 'free-will' and in the nature of the causal relationship: causes must precede effects. Also, experiments such as that outlined in §5.3 must be considered to be a refutation of the theory of special relativity. If this is not so, certain logically impossible phenomena, like the killing of one's parents before one is born, would be possible. Finally, on empirical grounds, we never see the past being influenced
1. Here we are dealing, specifically, with determinism relating to a single system viewed from the point of view of a single consciousness. This excludes the 'statistical determinism' exhibited by large ensembles and the 'determinism' for the whole universe(s) in the Many-Universes Interpretation of Q.M.
2. One trivial form of determinism remains possible, the kind adopted by fatalists in the face of any argument: what will occur is exactly and unambiguously what will occur, therefore nothing else can occur, therefore what will occur is determined! The determinism involved in a 'conspiracy' theory seems to be of this type.

by activities in the present. H.V. theories may be dismissed on the grounds that they are empirically untestable (§4.2). Result 3 provides a stronger reason for rejecting the possibility of their existence. This result applies also to interpretations of Q.M. which are deterministic, but which are not usually considered to be H.V. theories (e.g. those due to Ballentine (1970), Landé (1955, 1965, 1975) and Pearle (1976)). In particular, the refutation of the interpretations of Ballentine and Landé implies that the 'classical world-picture', in which systems *have* continuously or nearly continuously varying properties with unambiguous values at all times, cannot be maintained.

2. From the above and result 2, we conclude that Q.M. *may* be complete. Because of the difficulties in treating an incomplete description of the non-deterministic behaviour, as well as the fact that the non-locality implied by result 1 is not nearly as serious as that implied by result 3, we assume that Q.M. is complete (§5.4). Further, since H.V. theories have been shown to be untenable, the motivation for assuming that Q.M. is not complete has been removed. We use the completeness of Q.M. and the result that the behaviour of microphysical reality is not deterministic to come to some specification of the notion of probability as it is used in Q.M. (§5.5).
3. From result 4, and the fact that Q.M. is complete, we conclude that the time-development of quantum states must be non-deterministic. Since the time-development defined by the Schrödinger equation (or the unitary time development operators) of the physical formalism of Q.M. is deterministic, we postulate that an additional way in which quantum states change with time must occur. This change, which we call state reduction, must be non-deterministic. Since Q.M. is complete this change must occur, not only in our description of reality, but as an actual physical phenomenon (§5.6).

The second part of this thesis is concerned with the so-called 'problem of measurement' in Q.M. From our point-of-view this problem relates to state reduction: whether or not it occurs; under what conditions it can occur; whether or not it can be accounted for in a way which is both logically and conceptually consistent. The concrete result of this first part is that H.V. theories have been excluded. However, we consider it most important that we have been able to deal with some of the commonly-raised difficulties and present our fundamental requirements on an interpretation prior to embarking on an analysis of the problem of measurement in detail.

Part 2

Chapter 6

MEASUREMENT

6.1 Introduction

There are many problems associated with measurement on both classical and quantum mechanical systems. In the case of Q.M. these include the following:

- i). Under what circumstances does a physical system constitute a measurement apparatus on a second physical system with which it interacts?
- ii). What limitations do the properties of the measurement system give rise to on the types of measurement that are possible?
- iii). To what extent does measurement 'reveal' what is 'already there', and to what extent is the measurement result a function of the properties of the apparatus alone?
- iv). What are the details of the actual interactions that take place between the measurement system and the object system?

However, we are ultimately only interested in these questions in as far as they relate to a fundamental problem of measurement which bears directly on the interpretation of Q.M. This problem can be stated as follows:

When a system is not in an eigenstate of the operator corresponding to the observable to be measured, the quantum state of the system consists of a linear combination of such eigenstates. If we consider that each eigenstate corresponds to a single measurement result (eigenvalue) then such a state corresponds to several (mutually exclusive) measurement results. How is it, then, that upon measurement, only

one measurement result if found, (i.e. one of the eigenvalues). If Q.M. is not considered to be complete, and a deterministic H.V. theory is considered valid, this problem does not occur, at least, not so seriously. In this case, the probabilities given in Q.M. predictions would be an expression of our lack of knowledge as to which actual situation (out of an ensemble of possibilities specified by the quantum state) was present. Measurement would simply reveal, to a greater or lesser extent which possibility *had existed all along*. We would not be disturbed by the fact that the other alternatives disappeared on measurement since they *would never have existed* (except as our constructs, due to lack of knowledge) in the first place.

In the case of a complete theory, however, the fact that all of the possibilities occur in the quantum state means that they must, in some way, have simultaneous existence in reality. Now the fulfilment of one possibility and the disappearance of the others on measurement must correspond to some *actual physical phenomenon*. The problem which interests us is to account for this phenomenon in an objective and local fashion. i.e. The account must be independent of our abilities and our intentions.

For clarity, we illustrate this problem by means of a simple example, prior to a detailed analysis. In §3.5 we considered the double-slit interference of electrons. By assuming i) that the electrons either pass through one slit or the other and ii) that no non-local interaction between the slit-cover and the double-slit assembly or the electrons passing through the uncovered slit takes place, we came to an empirically incorrect result: that a double-slit interference pattern should be obtained in an experiment in which both slits were never open simultaneously.

Thus, one of our assumptions must be at fault. If we reject assumption (ii) we must accept a non-locality of the extremely unacceptable type associated with H.V. theories (see §5.3). We

therefore reject the first assumption, that each electron either passes through one slit or the other. Since the quantum state describing the electron contains terms involving the passage through both slits, this means that Q.M. may be complete. Now, however, we are faced with a new problem. Suppose that we move the detecting system to just behind the double-slit assembly. Equivalently, we could place a detector behind each slit. In this case, each electron is either detected behind one slit or the other, and never behind both. Our problem, then, is to account for the fact that, in a double-slit interference experiment, the electrons must, in some sense, pass through *both* slits (i.e. they do not pass through either one slit or the other) whereas in the second experiment, using *an identical state preparation system*, each electron is detected behind one slit or the other. In this case the 'existence' of the electron at the other slit 'disappears' on measurement.

In order to pose this problem in a more formal manner, it will be necessary to consider the theory of measurement in some detail.

It is by means of a theory of measurement that a correspondence between the properties¹ of a physical system and the properties of a measurement apparatus is achieved. This correspondence, which may be used to justify the use of a given measurement apparatus to perform a given measurement, must be constructed theoretically. Further related aims of a theory of measurement may be to relate the elements of a theory to the elements of experience and thereby to relate the elements of physical reality to the elements of experience. i.e. Measurement interactions produce sense data in the conscious observer as a result of interactions in the domain of physical reality.

1. Here, the term 'properties' is used in the general sense and not specifically as applied to the dynamical properties, of, say, a system in classical mechanics.

By means of a theory of measurement we can relate our experience to what actually occurs, and thereby formulate our ideas about physical reality. The notion of 'physical reality' - whether it is a meaningful term, and whether we can have knowledge of it - is a problem that has occupied a central position in philosophy for centuries. Some of the difficulties in formulating a theory of measurement which accomplishes all of the above aims are obviously closely related to these problems which are traditionally excluded from physics.

In §6.2, we investigate the role of a theory of measurement in the formulation of physical theories, subject to the doctrines of realism and of empiricism (positivism), and obtain a restriction on the theoretical basis of a measurement theory. In §6.3, we treat the 'problem of knowledge' with regard to its relevance to measurement theories, and how it is dealt with in classical measurement theories. We then attempt to apply the same procedure to Q.M. and show that it leads to certain difficulties. In this manner, we achieve an explicit statement of the 'problem of measurement' in Q.M. in general terms.

6.2 Measurement Theory, Empiricism and Realism

From an empiricist point of view, propositions which cannot be verified by 'direct' observation are meaningless. In the strictest sense, this leads to the conclusion that we can talk meaningfully only about sense-experience. Applied more loosely, this doctrine allows us to talk meaningfully about the behaviour of measuring instruments (or any other directly observable physical system) while they are being observed. To induce anything from the observations about other (unobserved) systems which interact with the measuring instruments is held to be meaningless in either case. In this sense, the empiricist may argue that physical

theories can deal only with measurement results (either as directly observed on measuring instruments or as sense-data), which are thus the prime entities of any theory. Alternatively he may insist that physical theories deal with our *knowledge* of physical systems, and not with physical systems themselves. As we have argued in §1,7, and as is obvious from the above considerations, a theory of measurement has no place in an empiricist physical theory. The question "where do measurement results come from and how are they perceived?" is meaningless since the answer would consist of a meaningless sentence.

By contrast, from a realist standpoint, a theory of measurement serves to answer this question. In order for a physical theory to be a satisfactory description of 'reality', it is necessary that the predictions of the theory be consistent with reality. If the theory is to be 'checkable' or 'falsifiable', this 'reality' must be accessible to our experience which must be consistent with the predictions of the theory.

In many cases, subject to classical physics, this requirement presents no problem, since much of the domain of classical theory is 'directly'¹ accessible to experience.

However, in some cases in classical physics, and virtually all cases of quantum physics, experience of the system of interest can only be obtained by employing further 'sensitive' physical systems which interact with the object system. In order to interpret our 'direct' experience of these measurement systems, and thereby induce properties relating to the object system, we must employ the relevant physical theory and apply it to the behaviour of the measurement apparatus. If the choice of measuring

1. We will come to a more definite usage of the term 'directly' below. Here it means that any measurement apparatus used is not obstrusive, and is usually neglected.

instrument is apt, this analysis should correlate directly observable properties of the measurement system with the properties of interest of the object system. By observing these properties on the measurement system, we may then, on the basis of the theoretical correlation, induce information about the object system. In any event, but especially if this information is to be used for checking a physical theory, it would be logically inconsistent to use any theory other than that under scrutiny for the analysis of the measurement apparatus. For this reason, classical mechanics and theories compatible with it are used in the analysis of classical measurement systems. For this reason too, despite assertions to the contrary, *Q.M. should be used in the analysis of measurement on quantum systems.* If Q.M. does not apply to the measurement system, then some theory which is compatible with it should be employed. We have shown that classical theories are not consistent with Q.M. in that some of the regulative principles which apply to the former are inconsistent with the latter. Hence, *to use classical theories in the analysis of measurements on quantum systems is logically inconsistent.* Classical mechanics and Q.M., being mutually inconsistent, can be used in the description of systems which fall into their respective domains. It is only as a result of confusion that they can both be employed in the *same* description of reality.

This important conclusion, although it seems irrefutable when presented in this manner, is neglected in some of the interpretations of Q.M. In a popular interpretation (often called 'orthodox'), it is asserted that measurement systems, being macroscopic, must be described classically. The disturbing features of quantum measurement which we describe in the following sections are explained away as special phenomena which take place at the 'interface' between the classical (macroscopic) and the quantal (microscopic) domains.

Bohr (1958), one of the few authors to attempt a full conceptual justification of his point of view, goes so far as to state as one of his premises that

"the functioning of the measuring instruments must be described within the framework of classical physical ideas" (p89). He arrives at this condition on the basis that the results of scientific enterprise must be communicable, together with the implicit assumption that only "classical physical ideas" are communicable. However, if, in order to communicate our ideas about microphysical reality, we are forced to resort to the concepts and terms of a theory which cannot apply to these microsystems, and is incompatible with the theory that does apply to them (Q.M.), it seems that we are attempting the impossible, and we may as well give up. While the use of a classical description of the measurement apparatus and its function on systems described by Q.M. may be justifiable as a 'stop-gap' for pragmatic reasons (our very patterns of speech and our 'common-sense' embody the familiar notions of classical physics), it is a logical *mistake* to construct a formal theory using incompatible theories.

Bohr, in order to account for this dualism, introduces it as a formal property of reality: complementarity. This is a 'blanket term' which covers any inconsistencies that may occur in an interpretation; the sources of inconsistency are held to be incomparable, because they are 'complementary'. As we show in § 7.6, where we treat the ideas of Bohr in greater detail, the notion of complementarity is incompatible with our notion that an unambiguous physical reality, consisting of actual physical situations, *exists* independently of our thoughts and desires, and independently of what will happen to the system at a later time. We disagree with Bohr's assertion that measurement must be described classically. On the contrary, we consider that this is logically inconsistent. Further, we believe that the dualism implied by the principle of complementarity

is an *unnecessary* departure from traditional ideas about reality, provided that a theory of measurement which is consistent with Q.M. (or a modification of Q.M.) can be found.

The source of the misunderstanding that leads scientists of Bohr's stature to attempt a description of reality on the basis of these two incompatible physical theories (classical mechanics and Q.M.) can perhaps be found in the 'Correspondence Principle' of Q.M. which, like the Heisenberg Uncertainty Principle, appears to be commonly misunderstood and misapplied. A mistaken view is that this principle ensures that, in the limit of large systems, Q.M. becomes identical with classical mechanics. This is erroneous on several counts. Least important to this discussion is that large systems (such as superconducting fluids, or simply a piece of hot metal, radiating) exist which cannot be satisfactorily described in terms of classical theories. There is also a vast difference between the formal structures of the two theories e.g. the superposition principle does not apply to classical particle states. Regulative principles such as locality and determinism are compatible with respect to classical physics and yet they cannot be so for Q.M. (See §4.3). How can they become compatible in the limit of large systems? So we could go on. The formal and conceptual differences between classical and quantum theories are enormous, and many of these differences are not related to the size of the system under discussion.

A careful statement of the Correspondence Principle which would avoid these problems, is that, in the cases where the predictions of classical mechanics are empirically correct, Q.M. must also provide empirically correct predictions. This is no more nor less than the requirement that both theories be empirically correct within their domains of application, together with the assumption that the domains of classical theories and Q.M. overlap. As the Correspondence Principle is not a statement about formal correspondences or compatibility

between the two theories, it is doubtful whether it deserves the status of a separate principle, especially since it is likely to be misunderstood.

Now that we have established the need for constructing a theory of measurement for Q.M. in terms of Q.M. itself, we shall consider the classical theory of measurement and its treatment of certain 'philosophical' problems. This analysis will be useful in that we apply the same procedures in the case of Q.M. This reveals the way in which Q.M. differs from the classical case.

6.3 The 'Problem of Knowledge' and Classical Measurement Theory

There are many philosophical problems relating to the existence of 'things' (real systems) and their properties, and how we can have knowledge of them. (See e.g. Ayer (1956)). Many of these difficulties can be solved by the assumption of a realist point of view, where the 'real world' (physical reality) exists because it is *assumed* to exist, and it is meaningful to talk about properties of constituents of the real world, independently of their being observed because, by assumption, they exist. However, the actual relationship between consciousness and the real world, a domain which could reasonably be considered as within the scope of a theory of measurement, remains problematical. In the words of Shimony (1963);

"There are two distinct problems concerning the relationships between physical objects and consciousness. One is the ontological problem of accounting for the fact that two such diverse kinds of

entities occur in nature and interact¹ with each other. The other is the epistemological problem of justifying physical theories by reference to human experience. A complete solution to either of these problems would surely require a solution to the other as well. In particular, it seems that the epistemological problem cannot be completely solved without understanding how the effects of physical entities can be registered upon consciousness, since performing observations and formulating theories constitute a series of acts of consciousness. It is a remarkable fact about classical physical theory that considerable progress was made on the epistemological problem, at least on that part of the problem which has been demarcated as 'scientific method', while the ontological problem remains obscure".

A brief examination of this ontological problem, which encompasses the 'Mind/Body' Problem and the 'Problem of Knowledge' of philosophy, shows that it does not lend itself to simple solution. Consider, for instance, the following difficulty, as raised by Bertrand Russell (1940):

"The observer, when he seems to himself to be observing a stone, is really, if physics is to be believed, observing the effects of the stone upon himself. Thus science seems to be at war with itself: when it most means to be objective, it finds itself plunged into subjectivity against its will. Naive realism leads to physics², and physics, if true, shows that naive realism is false. Therefore naive realism, if true, is false: therefore it is false. And therefore,

1. The notion that consciousness and physical reality interact gives rise to the "mind/body" problem in some of its forms. It is not a necessary assumption; Consciousness and reality, mind and body could, for instance, be different, non-interacting representations of the *same* thing. Nevertheless, this assumption underlies much of the treatment of the problem of measurement (e.g. by Wigner (1971)).
2. The link between naive realism and physics has been weakened by the failure of 19th century determinism.

the behaviourist, when he thinks he is recording observations about the outer world, is really recording observations about what is happening to him."

The reader may feel, as does the author, that this inconsistency is not as clear cut as Russell makes it out to be. Nevertheless, the problem to which he refers is implied by classical theories, but is not dealt with by them. It is reasonable to assume that the 'considerable progress' made with the 'epistemological problem' is achieved as a result of the way in which the 'ontological problem' is treated in classical theory; it is *totally neglected*. All problems concerning the relationship between physical reality and consciousness are dealt with, in classical theories, by *omission*; the Problem of Knowledge is shunned by physicists, being regarded as strictly within the domain of philosophy. We will now consider an example of a classical description of measurement to illustrate how it is that any reference to consciousness can be omitted.

Suppose that the object system in an experiment is an electro-chemical cell, and the property which is to be measured is the potential difference between the two terminals of the cell. This potential difference is not directly observable. By this we mean that the 'apparatus' provided in the human body does not constitute a measurement apparatus for electric potential differences¹. We therefore employ a measurement apparatus, a galvanometer. Now, by means of an analysis of the behaviour of a galvanometer in terms of classical theory, we construct a correlation between the position of the pointer needle (considered as an element of classical theory) and the potential difference of the cell (also considered as an element of the theory). By using the same procedure we could go on to relate, by means of classical theory, the position of the

1. For convenience, we neglect the well-known method of 'tasting' a battery to see whether or not it is 'flat'.

pointer and the behaviour of the illuminating light, the light and the human eye and retina, the retina and the optic tract etc. However, it is customary in classical physics, and, as we shall see, important, that we stop once the correlation between the position of the pointer and the potential difference of the cell has been demonstrated. The further correlations are of interest in other contexts, but in this experiment, we would say that the observer *sees* the position of the pointer '*directly*'.

The identification of the position of the pointer as an element of the theory and the position of the pointer as an element of the real world is considered to be established by means of this direct observation. By means of the above (theoretical) correlation, we consider a measurement of the potential difference of the cell (as an element of physical reality) to have been performed.

The (implicit) identification of the position of the pointer as an element of the real world and the position of the pointer as a sense-impression registered in the consciousness of the observer, is not considered.

If the 'ontological problem' mentioned by Shimony were solved, we would be able to pursue the process of making correlations into the brain of the observer and, knowing the relation between consciousness and the real world (via the brain, say), into the consciousness of the observer. This would establish a theoretical correlation between the sense impressions of the observer and the potential difference of the cell. Given the sense-impression of the observer (as an element of 'sensorial reality') we could deduce the potential difference of the cell, as an element of physical reality. However, the required 'connection' between sensorial and physical realities is lacking, and, in practice, correlations

between elements of the real world and sense-impressions are always made implicitly. For example, when the experimenter says "The pointer indicates 1.5 volts" and not "I see the pointer indicating 1.5 volts", he has already made this identification. The properties of objects as elements of the real world are *identified* with the elements of experience.

In this way, the Problem of Knowledge is avoided in classical physics. One of the conditions which must be fulfilled for this conceptual device to be implicit and unobstrusive (and hence plausible) is that the relation between the sense impressions of the observer and the elements of reality be one-to-one (i.e. it must be an identification in the mathematical sense). Since the elements of the theory are in one-to-one correspondence with the elements of physical reality (in a *complete* theory), this requirement is equivalent to the following:

The relation between the elements of a physical theory and the elements of experience should be a one-to-one correspondence, if the Problem of Knowledge (Shimony's 'ontological problem') is to be excluded from physics.

This requirement is so fundamental that it may seem obvious and trivial. However, as we shall see in the following chapter, it is not fulfilled by Q.M. without additional postulates. In classical physics, the relation certainly is one-to-one. To each property of a classical system in a given 'state', there is exactly one sense impression.

Once we make this identification, we may brush aside the problem posed by Russell as follows: the observer does *not* observe the effects of the stone upon himself; he 'sees' the stone 'directly'. The effects of the stone on the observer, when limited to physical effects, may be of interest to another physical scientist, the *object of whose observation is the first observer*. The effects of the stone on the

conscious aspects of the observer may be of interest to a psychologist, but not to a physical scientist, unless that observer were reporting what he had 'seen directly'.

Thus the theory of measurement in classical physics is always incomplete¹ in that the final part of the measurement interaction, the interaction between the real world and the conscious observer whereby the observer becomes aware of the measurement result, is always omitted. However, it is upon this incompleteness that the success of classical realism rests. This omission is made plausible by the implicit identification of 'seeing directly', as a conscious act, with 'interacting with' in the sense of physical interactions. In turn, this identity is only possible if there is a one-to-one correspondence between the elements of the physical theory and the elements of experience (sense impressions).

We shall see in the following section that this is not the case when the measurement apparatus and the measurement interactions, as well as the object system are described quantum mechanically.

6.4 The Quantum Mechanical Description of Measurement

As we have seen, (e.g. in §5.6) the behaviour of microphysical systems (at least over time intervals which include measurements) cannot be deterministic. Therefore, we cannot really hope to account for this behaviour completely in terms of Q.M., in which states develop deterministically, (as specified by the Schrödinger equation, or by the action of a unitary operator). However, since this is an important point, it is as well to go into the quantum mechanical description of measurement to show that this cannot account

1. We use the term 'incomplete' here in its general sense and not in the context of the E.P.R. argument as discussed in Chapter 2.

for the non-deterministic behaviour of single, microscopic systems.

The quantum mechanical description of measurement can be traced back to Von Neumann (1932). Similar proofs of his results and some extensions to them have been made by Wigner (1967, 1963, 1971) d'Espagnat (1971) and others. We present in detail only the most elementary considerations, and argue that similar results must hold in the case of more complicated situations.

Consider a system S which is prepared with a state $|\psi_n\rangle^1$ in Hilbert space $\mathcal{H}^{(S)}$, where $|\psi_n\rangle$ is an eigenvalue of hermitian operator A corresponding to some measurable observable of the system S with eigenvalue a_n

$$\text{i.e.} \quad A|\psi_n\rangle = a_n|\psi_n\rangle \quad 6.1$$

Now suppose that a system M is used to measure the observable corresponding to A on S . Let us suppose that the initial state of this apparatus is $|\phi_0\rangle \in \mathcal{H}^{(M)}$. Now, if M is to fulfil its function as a measuring apparatus, it must be left in some state which corresponds to $|\psi_n\rangle$ after interaction with S . Let us call this state $|\psi_n\rangle$. (The states $|\phi_0\rangle$ and $|\psi_n\rangle$ could correspond to the states of a measuring apparatus with the dial pointer indicating 0 and n respectively.) These values would then be eigenvalues of the operator corresponding to the observation of scale-reading on M .

In this case the time development of the system as a whole ($S + M$) can be represented as follows:

$$|\phi_0\rangle|\psi\rangle \rightarrow |\phi_n\rangle|\psi_n\rangle \quad 6.2$$

1. We consider all states to be normalized.

Here the arrow represents the action of a unitary time development operator on the initial state in the outer product Hilbert space $\mathcal{H}^{(M)} \times \mathcal{H}^{(S)}$. $|\psi'_n\rangle$ represents the final state of S after measurement. In an ideal measurement, $|\psi'_n\rangle = |\psi_n\rangle$, but, for the moment, we place no restrictions on $|\psi'_n\rangle$.

For some other eigenvalue, $|\psi_m\rangle$, of A, corresponding to a different eigenvalue a_m , we would demand for M to constitute a useful measurement apparatus that $|\phi_m\rangle \neq |\phi_n\rangle$ and that they correspond to different eigenvalues (pointer positions) m and n, say. In this case, the time development of the whole system can be represented by

$$|\phi_0\rangle|\psi_m\rangle \rightarrow |\phi_m\rangle|\psi'_m\rangle \quad 6.3$$

Now, since the states $|\psi_n\rangle$ and $|\psi_m\rangle$ are possible states for S, by the superposition principle, any linear combination of them must also be a possible state. (Here we ignore the possibility of superselection rules by considering a situation to which they do not apply: $|\psi_n\rangle$ and $|\psi_m\rangle$ could, for instance, be position eigenstates of S).

Suppose that S is prepared with initial state $|\psi\rangle = c_m|\psi_m\rangle + c_n|\psi_n\rangle$ where c_m and c_n are complex coefficients such that $|c_m|^2 + |c_n|^2 = 1$. By 6.2 and 6.3, and the linearity of the time-development operator (or, equivalently, the Schrödinger equation) we must write the development of the state of the whole system over the interval during which interaction takes place as follows:

$$\begin{aligned} |\phi_0\rangle|\psi\rangle &= |\phi_0\rangle(c_n|\psi_n\rangle + c_m|\psi_m\rangle) \\ &= c_n|\phi_0\rangle|\psi_n\rangle + c_m|\phi_0\rangle|\psi_m\rangle \rightarrow c_n|\phi_n\rangle|\psi'_n\rangle + c_m|\phi_m\rangle|\psi'_m\rangle \quad 6.4 \end{aligned}$$

Interpreted according to the physical formalism of Q.M., the r.h.s. of 6.4 means that we will find M in state $|\phi_n\rangle$ ($|\phi_m\rangle$) and hence the result a_n (a_m) with probability $|c_n|^2$ ($|c_m|^2$). Thus far, all is well. This is exactly the result we expect for a measurement of A on S in state $|\psi\rangle$ according to the physical formalism. This means that the description of the measurement process in terms of Q.M. satisfies one of the aims of a theory of measurement: a correlation is provided between the states of the measurement apparatus and the states of the object system. The possible results achieved by observing the object system 'directly' (if this were possible) are correlated with those obtained by observing the measurement apparatus. Further, in the case of an ideal measurement ($|\psi_n\rangle = |\psi'_n\rangle$), the object system is left in the eigenstate corresponding to the eigenvalue indicated by the measurement apparatus. A subsequent measurement of A, if performed soon enough (or if A commutes with the Hamiltonian, after measurement) will give the same result.

This is yet another indication that the problems of interpretation of Q.M. are not formal, but physical or conceptual: the physical formalism is not at fault, since it predicts the correct results. The difficulty here is that the final state of the combined system contains terms relating to *all* possible outcomes, with no indication as to which result, a_n or a_m in 6.4, will actually be found in a specific experiment. The interaction between the object system and the measuring apparatus does not 'choose' one of the possible results. This is in contrast to the fact that, when we observe pointers 'directly', we always see them in exactly one position (at least in studied scientific experiments)^{1,2}. In terms of the

1. This is clearly a generalized version of the 'Schrödinger Cat Paradox'.
2. We can only determine the position of the pointer with limited precision. Nevertheless, we can construct the scale for a discrete spectrum of measurement results so that it is exact and unambiguous. For a continuous spectrum, we can be as accurate and unambiguous as we like, subject to technical difficulties.

analysis of §6.3, we cannot make a one-to-one correspondence between the state of the measurement apparatus¹ after measurement as a theoretical entity and the sense-impression that occurs upon observing it: the final state of the measurement apparatus¹ contains terms corresponding to all possible outcomes whereas we see only one, in each case.

This difficulty cannot be resolved by considering further interactions in the measurement chain! If we consider a third system, E, (some physical part of the observer's body, say,) which interacts with M in such a way as to be 'sensitive' to the results of the measurement on S, we can consider the combined system (M + S) as the object system (in place of S) and E as a measurement apparatus upon it (in the place of M). By exactly the same argument as above, we must write the time development of the whole system (E + (M + S)) over a time interval during which E and M interact as follows:

$$|\theta_0\rangle (c_n |\phi_n\rangle |\psi_n'\rangle + c_m |\phi_m\rangle |\psi_m'\rangle \rightarrow c_n |\theta_n\rangle |\phi_n'\rangle |\psi_n''\rangle + c_m |\theta_m\rangle |\phi_m'\rangle |\psi_m''\rangle$$

6.5

where $|\theta_0\rangle$, $|\theta_n\rangle$, $|\theta_m\rangle$, are states of E (with obvious notation), and $|\psi_n''\rangle$, $|\psi_m''\rangle$ are obtained deterministically from $|\psi_n'\rangle$ and $|\psi_m'\rangle$ respectively.

This indicates that the correlation is 'transmitted' to the states of E so that the result obtained by observing E 'directly' must agree with those obtained by a 'direct' observation of M. However, this procedure is not sufficient to 'choose' one of the possibilities and result in the disappearance of the others, in

1. Strictly, we cannot refer to the final state of the measurement apparatus *alone* since it is 'non-separably' linked with the object system (See §1.5). We feel our incorrect usage is clear.

a particular measurement. This is in contrast to the classical case where the interactions between successive measurement apparatus serve not only to correlate their results, but also pass along the information as to what the measurement result *actually is*. Measurement, on classical systems, serves to *reveal* the values that the system actually *has* prior to measurement. A quantum system on the other hand, does not 'have' properties prior to measurement in the same sense. The quantum system has '*propensities*' which are expressed as the amplitudes of the different eigenvectors of the relevant Hermitian operator, or, on measurement, the probabilities (modulus squared of the amplitudes) that a given eigenvalue will be found. In the process of measurement, exactly one of the relevant propensities must be fulfilled, while the others must simultaneously disappear. It is *this* process that gives rise to unique unambiguous measurement results on quantum systems, and it is *this* process that cannot be described in a quantum mechanical treatment of measurement. Q.M. treats the dynamics of a system by describing how the 'propensities' transform. There is no process in the physical formalism of Q.M. by means of which the propensities can be fulfilled.

Some authors (especially d'Espagnat (1971) and Wigner (1963,1971)) go to great lengths to show that this result cannot be avoided by a less schematic treatment of measurement than that given above. However, we *expect* difficulties to occur in the description of non-deterministic behaviour by means of a deterministic (or single-valued) formalism (See §5.6). The problem of measurement arises formally from the superposition principle (which is absolutely fundamental to the Q.M. formalism) and the linearity of the Schrödinger equation. For these reasons, we anticipate that the result of any detailed description of the measurement interactions, using Q.M., will be the same as that which we have shown above: a system cannot evolve, according to the quantum mechanical description, into a state which indicates a single unambiguous

measurement result (unless, of course, the system is an eigenstate of the relevant hermitian operator).

d'Espagnat (1971) considers systems in which the initial state of the measurement system is unknown (i.e. the initial state is a mixture). The result of Araki and Yanase (see Stein and Shimony (1971) and Yanase (1971)) that ideal measurements are seldom possible, and then only as a limiting case, prompts him to consider non-ideal measurements in detail. He considers the possible effects of super-selection rules and the fact that we cannot expect a measurement apparatus to work perfectly every time. While these detailed considerations are useful in dealing with the dogmatic criticisms of sceptics, to some extent they amount to making paper tigers and tearing them up: they simply tell us that a result which follows from the general principles of Q.M. holds in these specific cases also. Still, it may appear more convincing to the reader if we deal with some of these cases specifically.

As we have seen, it is largely unimportant, in our treatment above, whether the measurement considered is ideal or not. To deal with superselection rules we need simply consider specific cases in which they do not apply. This would be so if A represented, for instance, a position measurement. We cannot understand the motivation behind considering a measurement apparatus which does not perform its function properly, particularly in an 'in principle' analysis. This leaves us with the case in which the initial quantum state of the measurement apparatus is unknown. (Our argument applies equally well to the case where the initial state of the object system is unknown). In this case the initial state of the system is called a 'mixture' to distinguish it from a 'pure state' when the quantum state is exactly specified. Since we have made no assumption as to whether or not the state S or M is *known* in the discussion above, we can hardly expect the result in this case to be different. However, the motivation for considering the case in which the initial state of the measurement apparatus is a mixture

arises from two sources. Firstly, it is unlikely that it will ever be possible to specify exactly the state of a complex macroscopic body like most measurement apparatus. Secondly, it follows from the deterministic dynamics of quantum states that, if the final state of the system is to be unknown (a mixture) then the initial state of the system must also be unknown (a mixture).

Formally, this is most clearly shown using the 'density matrix' formulation. Suppose the initial state of the composite system is a 'pure' state $|\psi\rangle$. Then the density operator representing this system is given by

$$\rho_0 = |\psi\rangle\langle\psi| \quad 6.6$$

We use the following well-known properties of the density operator i). the density operator corresponding to a pure normalized state is a projection operator i.e.,

$$\rho^2 = |\psi\rangle\langle\psi||\psi\rangle\langle\psi| = |\psi\rangle\langle\psi| = \rho \quad 6.7$$

ii). the density operator after time t is given by $\rho_t = U \rho_0 U^\dagger$ where U is the unitary time development operator on $|\psi\rangle$ from time 0 to t .

$$\begin{aligned} \text{Then } \rho_t^2 &= U \rho_0 U^\dagger U \rho_0 U^\dagger \\ &= U \rho_0^2 U^\dagger \quad \text{by the unitarity of } U \\ &= U \rho_0 U^\dagger = \rho_t \end{aligned} \quad 6.8$$

If the density operator for an isolated system is initially idempotent, it remains so as long as the system is isolated. That is if a system is initially in a pure state, it remains so: a system cannot evolve from a pure state into a mixture of states subject to

the deterministic time-development as specified by a unitary operator U (or the Schrödinger equation). This is equivalent to our earlier result but it is more suited to the present discussion.

We require that the final description of the composite system (and hence, also, the initial description) be in terms of a 'mixture' in the (vain) hope that each state in the mixture (i.e. each element of the ensemble of possible states for the composite system) should correspond to a different measurement result. If this were found to be the case, the fulfilling of one possibility and the disappearance of the others would correspond to nothing more than the change in *our knowledge* of the system. This process would be exactly analogous to finding the result of spinning a coin to be 'heads', say, and not 'tails'.

Wigner (1963) and d'Espagnat (1971) show by detailed considerations that, in cases where the object system is in a superposition of states corresponding to different measurement results, each element of the mixture consists of a superposition of similar terms, only now they will show the correlation between the states of the object system and those of the measurement apparatus. This conclusion follows easily from our viewpoint as long as the measurement interactions are described by the deterministic relations of ordinary Q.M., since we made no assumption as to whether the state of the measurement apparatus (or the object system, for that matter) was *known* or not.

We conclude, then that the full description of the evolution of a system over any time interval during which measurement occurs cannot be described in terms of an ordinary deterministic quantum evolution without leading to complications. The final state of any combination of object system and subsequent measurement apparatus thus achieved, will, in general, comprise a superposition of

states each corresponding to different 'mutually elusive' measurement results. Thus, it is not possible to draw a one-to-one correspondence between the final state of the measurement apparatus (which is non-separably linked with the object system) and the impression registered by an observer (that a single unambiguous measurement result is indicated).

6.5 Criticisms

The above result depends on two major assumptions : that Q.M. constitutes a complete description of physical reality (in the sense outlined in §2.3) and that the measurement apparatus can be described, independently of its environment, by a quantum state. The measurement interaction is to be described in terms of the Schrödinger equation or its equivalent. Criticisms of our result stem from questioning these assumptions.

We have given our reasons for assuming that the description of physical reality afforded by Q.M. is complete in §5.4. However, we take this opportunity to remind the reader of the possibility that Q.M. provides an incomplete description of a non-deterministic physical reality. We rejected this possibility on the grounds that we cannot imagine how to 'complete' this description if H.V. theories are not viable. Nevertheless, it may be possible to avoid the conclusion of this chapter in this way.

Our second assumption, that the measurement apparatus can be described independently by a quantum state, has been questioned by some authors (e.g. Belinfante (1975)) on the grounds that a large system like a measurement apparatus cannot be isolated from its environment. (See Zeh (1970) and §9.1 → §9.4). This implies that, due to the problem of non-separability, it is not possible to assign a quantum state to the measurement apparatus

independently of its environment. This invalidates our arguments above.

Our first objection to this criticism is that, since we are dealing with 'in principle' considerations which apply to any measurement, and since, even in classical mechanics, systems are rarely completely isolated, it is somewhat 'unsporting' or pedantic to bring up this point here. All that we require on classical systems is that they be 'significantly' isolated from extraneous influences; that these influences can be made vanishingly small, which is very close to the situation we are faced with here. Nevertheless, we must concede that it is possible that by taking our measurement apparatus (including a radiation shield) into deep space to ensure mechanical isolation, we would obtain significantly different measurement results, corresponding say, to finding the object system in more than one 'mutually exclusive' state. We therefore investigate the possibilities, should our conclusion (in § 6.3) be invalid.

If the measurement system cannot be described independently by a quantum state, it follows that the interaction between the object system and the measurement apparatus cannot be described in terms of the deterministic evolution given by the Schrödinger equation or its equivalent. i.e. Our equation 6.5 is incorrect, since we cannot even write down the l.h.s. as it is there, never-mind conclude that it evolves into the superposition given on the r.h.s.

This leaves the possibilities for the description of the measurement interaction open. However, we require that the measurement apparatus should eventually indicate a single unambiguous result. We can distinguish two possible explanations for how this comes about under these circumstances.

Firstly, we could say that the fact that the measurement system is non-separably bound up with its environment introduces a change in its interaction with the object system which results in a non-deterministic transition from a situation relating to all possible measurement results to one in which exactly one measurement result is seen to occur. No matter how this transition is described, it falls into the class of transformation which we call state reduction, which we introduce in the next chapter. We discuss this kind of viewpoint in §9.1 → §9.4. Since our ultimate aim in this argument is to show that the introduction of non-deterministic evolution in addition to the deterministic evolution of ordinary Q.M. is unavoidable, this viewpoint leads us to the same result as our contested proof.

Of greater interest is the possibility that the fact that different results are obtained in successive measurements on systems prepared with the same quantum state could be explained by asserting that, although the object system is prepared essentially identically, the situation pertaining to the *measurement system* is different in each case. The possible actual situations pertaining to the measurement system could be divided into classes corresponding to each possible measurement result. This is equivalent to applying a kind of hidden variables theory to the *measurement apparatus* as opposed to the *object system*, allowing for the possibility of a deterministic description. We have two major criticisms of this interpretation. The first is that it does not avoid the introduction of a new interaction to account for the interaction between the object and measurement systems, since this cannot be described by ordinary Q.M. (If it is, our argument above excludes this possibility altogether). This also casts serious doubts on the assumption that Q.M. is complete since we describe the actual situation of the measurement system while simultaneously asserting that it cannot be assigned a quantum state.

Secondly, we shall show that, by considering the process of measurement from another perspective, some apparent contradictions occur. We can safely suppose that, prior to measurement, the object system is isolated from its environment, including the measurement system, provided that we suppose that the object system is 'microscopic'. Instead of the measurement system by itself, let us now consider it together with as much of its environment as we need to constitute an isolated system. This super-system could even be the entire universe (with the exception of the object system) if necessary. Since the super-system is isolated, we can assert that it has a unique quantum state. Now equation 6.5 can be considered to describe the interaction between the object and the supersystem which contains and replaces the measurement system. Equation 6.5 shows that, according to the quantum mechanical evolution which is applicable here, the final state of the supersystem and object system combined is a superposition of states corresponding to different measurement results. If the ordinary quantum mechanical evolution and the interpretation outlined above are both assumed to be valid explanations of physical reality, this leads to an ambiguity, if not a contradiction; if we consider the interaction between the object system and the measurement apparatus alone, we conclude that exactly one measurement result is obtained. From this we can infer that the supersystem, which contains the measurement system, should have a state which corresponds to a situation in which only this measurement result exists. This is in contradiction to the result obtained by treating the evolution of the object system and the supersystem quantum mechanically¹.

In order to avoid this ambiguity, we assume that either our argument in §6.4 is substantially correct (or at least in principle) or else the interpretation that S.R. occurs as a result of an interaction involving a non-separable system is valid. In both cases, the

1. This type of consideration is treated differently in §7.6.

same result holds: measurement cannot be fully described by the deterministic relations of ordinary Q.M.

Chapter 7

STATE REDUCTION

7.1 The Concept of State Reduction

In §5.6, we introduced the notion of state reduction (S.R.) to explain how the non-deterministic behaviour of microsystems can be described in terms of the deterministic formalism of Q.M. In Chapter 6, we showed that the description of measurement in terms of Q.M., while providing a correlation between the states of measurement systems and those of the object system, cannot yield a final state for the measurement apparatus which is in one-to-one correspondence with the sense-impression an observer experiences when observing it 'directly'. This means that 'seeing directly' and 'interacting with' cannot be identified as they are in classical physics, thereby avoiding the 'Problem of Knowledge' and related 'philosophical' difficulties.

Both of these problems (which are, of course, closely related) can be dealt with by postulating the occurrence of S.R. at some time before or during the measurement process. For the object system S and measurement apparatus M considered in §6.4, the transformation of the state of the combined system is given by Equation 6.4 as

$$|\phi_0\rangle (c_n |\psi_n\rangle + c_m |\psi_m\rangle) \rightarrow c_n |\phi_n\rangle |\psi'_n\rangle + c_m |\phi_m\rangle |\psi'_m\rangle \quad 7.1$$

The transition brought about by the reduction of this state can be written as follows:

$$c_n |\phi_n\rangle |\psi'_n\rangle + c_m |\phi_m\rangle |\psi'_m\rangle \rightarrow \begin{matrix} |\phi_n\rangle |\psi'_n\rangle \\ |\phi_m\rangle |\psi'_m\rangle \end{matrix} \text{ with probability } \begin{matrix} |c_n|^2 \\ |c_m|^2 \end{matrix} \quad 7.2$$

On the r.h.s. of 7.2, the states of the measurement apparatus and the object system are given separately as $|\phi_n\rangle$ and $|\phi'_n\rangle$ or $|\phi_m\rangle$ and $|\psi'_m\rangle$ respectively. Each of these states corresponds to a single,

unambiguous result (a_n or a_m respectively). In this case, any further suitable interactions will simply pass this result along the measurement chain, in much the same way as in classical mechanics. i.e. Suppose a second measurement apparatus, E, interacts with M. In this case the result will be

$$|\theta_O\rangle|\phi_n\rangle|\psi_n'\rangle \rightarrow |\theta_n\rangle|\phi_n\rangle|\psi_n\rangle \quad \text{with probability } |c_n|^2 \quad 7.3$$

$$\text{or } |\theta_O\rangle|\phi_m\rangle|\psi_m'\rangle \rightarrow |\theta_m\rangle|\phi_m\rangle|\psi_m\rangle \quad \text{with probability } |c_m|^2$$

That is, the state of E is given unambiguously in either case (as $|\theta_n\rangle$ or $|\theta_m\rangle$). This is in contrast with Equation 6.5 of §6.4 where in the absence of S.R. the final state of the composite system (S + M + E) does not correspond to one or other result.

Prior to S.R. we cannot assert that any particular value for a given observable 'exists' (See e.g. §3.5). However, after S.R., exactly one of the measurement results exists, and it remains for the observer simply to discover which one it is. Thus S.R. solves the problem of measurement as posed in Chapter 6.

Furthermore, since the transition described by 7.2 (S.R.) is many-valued, in that, on different occasions, transitions to different (mutually exclusive) states occur, S.R. describes a non-deterministic transition. i.e. The development of the state of the system from initial state to final state after S.R. is non-deterministic. Thus, the quantum mechanical formalism together with S.R. may be used to describe the non-deterministic behaviour of microphysical systems. The results of Chapter 6 show what we suspected all along; that non-deterministic behaviour cannot be introduced by considering the behaviour of the measurement system as well as the object system. This means that S.R. cannot be accounted for in terms of the time-development given by the physical formalism of Q.M. It must be added as an extra postulate.

We note also that the process of S.R. is not time reversible: the 'propensities' which disappear when S.R. occurs would have to be resurrected from extinction in the time-reversed world.

It may be as well to provide a less formal description of the phenomenon of S.R. This process corresponds to choosing (randomly) one of the possible results indicated by the final state, and causing the other possibilities to vanish. In terms of the double-slit experiment considered in §6.1, this accounts for the fact that electrons are detected either behind one slit or the other, despite the fact that we can infer from the interference experiment that each must, in some sense, pass through both slits. Each electron does indeed pass through both slits (as indicated by our argument in §6.1, or by the fact that terms corresponding to passing through both slits occur in the complete quantum state of each electron) but, in the course of a measurement behind the slits, S.R. occurs, and the existence of the electron behind one slit vanishes as it becomes manifest behind the other. In this argument we have used the fact that, since Q.M. is complete (See §5.4) any transition of the quantum state must correspond to a transition in the actual physical situation described by that state. It follows that the transition described by 7.2, state reduction, corresponds to an actual physical phenomenon.

Non-determinism is a notion that is novel in physical theories. S.R., which describes the non-deterministic transition, is therefore also a novel phenomenon which, from the classical point of view, appears very strange indeed. The 'strangeness' of S.R. is further enhanced by the fact that it involves the transmission of virtual information over a space-like interval, a non-local interaction (see §5.2). While this does not conflict with the basic tenets giving rise to the prohibition of such interactions (see §5.3) it retains all the aspects of novelty, artificiality and unfamiliarity of non-local interactions. These considerations have led many interested physicists to reject the notion of S.R. altogether. We consider S.R. to be the least *unacceptable*

alternative, as we have illustrated by our development so far. In the following sections, we deal with some of these objections (some of which we have already considered).

7.2 Criticisms

Many authors have recognised that the notion of S.R. has at the heart of the problems of interpretation of Q.M. in general, and the problem of measurement in particular. The response of some of these authors has been to attempt to eliminate the concept from Q.M., either by restricting the purposes of physical theories or else by formulating an interpretation in terms of which the concept is unnecessary.

If the aims of a physical theory are restricted to the correlation of empirical results and the prediction of further results, any theory of measurement becomes redundant (§6.2). Indeed, as we observed in §1.7 this aim is adequately fulfilled by the physical formalism of Q.M. It is therefore inconsistent to consider anything other than a trivial solution to the problems of interpretation of Q.M. (i.e. that no interpretation other than that in the physical formalism is necessary), while at the same time rejecting the notion of S.R. on these grounds. Nevertheless, Margenau and Park seem to espouse this view. Park (1973) writes:

"The flaw in F.Q.T.M. (interpretations including S.R.) is at root philosophical, inhering in a steadfast refusal to accept quantum mechanics for what it is, a theory about the *statistics of measurement results*". (My italics).

Again, in Park & Margenau (1968) we read

"The numbers they (measurements) produce are called *measurement results*, and it is the responsibility of quantum theory to regularize, interpret and make predictions about them". (p216).

Here, the word 'interpret' leaves open the possibility of a theory of measurement, but the emphasis on the primacy of measurement results indicates the empiricist viewpoint outlined above.

Heisenberg's observations that Q.M. deals with our *knowledge* of physical systems and not the physical systems themselves, can be viewed as an unfortunate truism, relating to the Problem of Knowledge, and necessarily applicable to all physical theories. If, on the other hand, it is viewed as an *a priori* restriction on the aims of Q.M., it is equivalent to a statement of the empiricist doctrine whereby a physical formalism is itself an acceptable physical theory. No problems of interpretation or measurement can occur, since neither interpretation nor theory of measurement is meaningful or necessary.

While the adoption of this point of view certainly 'solves' the problems of interpretation and measurement of Q.M. (if trivially) we reject it on the grounds that it 'pulls the teeth' of science by denying the existence of its second purpose (see §1.8). If the 'objects' of physical theories are 'measurement results' then it is difficult to see what physics has to do with 'physical reality'. If we restrict the aims of physical theories to those of predicting and correlating measurement results, we need some other discipline to deal with the relationship between the measurement results and their 'source': physical reality. Rather than formulate such a new discipline we include it as part of physics by accepting the second purpose: to understand and explain our environment.

Indeed, most authors find this doctrine overly restrictive and impossible to apply consistently. When Park and Margenau (1968) assign the property of *latency*¹ to quantum systems, or even consider physical systems at all, they go outside the bounds of the empiricist philosophy and enter the domain where the problem of measurement in Q.M. is significant. A view rather like a 'weak' form of empiricism is ascribed to Margenau and subscribed to by Leibowitz (1975) who states

"Measurement is a matter of *observation, not prediction*. There is no problem of measurement since measurement is *outside the scope* of quantum theory. In this respect I follow Margenau even though his interpretation is based on the concept of an ensemble of systems, whereas ours is based on a single system concept".

Surely, from a realist point-of-view, measurement is just the place where prediction and observation coincide. If the relation between 'prediction' and 'observation' may, and even must be accounted for in classical theories (by means of a theory of measurement), what grounds are there for asserting that it is unnecessary with respect to Q.M.?

Among the work of those authors who accept the second purpose of physical theories either explicitly or tacitly, by accepting the problems of interpretation and measurement, we can distinguish several different interpretations in which it is claimed that S.R. can be dispensed with. Some authors (e.g. Belinfante (1975)), employ more than one of these, as well as the assumption that the problems are trivial, in a single interpretation. This 'belt and braces' policy indicates some deeper aversion to the concept of S.R. than those dealt with explicitly.

1. This terms means much the same as our 'propensity' in §6.4.

7.3 Ballentine's Statistical Interpretation

We dealt with this interpretation (which is similar to that of Landé (1955, 1965, 1975) in §3.5 where we showed that it is unacceptable on the grounds that it involves the assumption that non-local effects exist. Since this interpretation satisfies our definition of a H.V. theory (§4.1) it follows that these 'non-local' effects must be of the extremely unacceptable variety described in §5.3. Nevertheless, as this theory still seems to carry some weight amongst physicists,¹ we will deal with it here in the context of measurement. In this theory, the quantum state specifies an ensemble of different actual physical situations, each of which 'has' definite values for its classical-type properties (position, momentum, energy, spin, etc.). This is especially true for the final state of the combined system of measurement apparatus and object system given, for instance, by the r.h.s. of Equation 6.4.

In this case, by observing the measurement apparatus, we simply *find out* which of the possible actual situations had existed all along. This process is exactly analogous to looking at a spun coin (which may be described as having the 'state': heads with probability $\frac{1}{2}$ or tails with probability $\frac{1}{2}$) and finding out that the results is 'heads', say. In this case, it is unnecessary to assert that something must have happened to the system for the result 'tails' to 'disappear': this result or a state of the coin compatible with it never existed in the first place. The phenomenon of S.R. corresponds, in this interpretation, to nothing more than the change in our knowledge of the system which occurs when we make an observation.

It is this deceptive similarity between S.R. and the process

1, Ballentine (1974) considers his results to be 'well-known',

of increasing our knowledge or 'finding out' in classical stochastic theories that leads many physicists to favour the 'statistical interpretation'. However, this interpretation can be maintained only if the severe non-local effects described in §5.3 can be accepted. We think not.

The more sophisticated attempts at formulating H.V. theories (in which the behaviour of microphysical systems can be viewed as deterministic) are unacceptable for the same reason. In view of the fact that Ballentine's theory is untenable, these theories can at most predict what will be found upon measurement, i.e. They cannot give values for the classical-type properties prior to measurement. This means that we must expect something like S.R. to occur when the variable being measured 'takes on' a specific value at measurement. Indeed, the concept of S.R. occurs in some H.V. theories. In the theory of Bohm and Vigier (1954) it is accounted for by the inclusion of non-linear terms in the Schrödinger equation which only became evident in 'measurement interactions'. We mention this in passing since we rejected H.V. theories in §5.3.

7.4 Ensembles and Single Systems

In Park and Margenau (1968), we read "... the idea of wave packet reduction does not survive close scrutiny. Such reduction cannot be consistently attached to quantum theory by postulation because of the inherent statistical nature of quantum states; i.e., the physical reference of the density operator to *ensembles* rationally precludes its changing abruptly in response to a single measurement".

Belinfante (1975) states that

"When in quantum theory we assign exact values to probabilities, we consider the idealized case of an "infinitely" large collection of cases among which a relative frequency is calculated. The theory, therefore, deals with *ensembles*. State vectors determine the probability distributions in ensembles: *State vectors are properties of ensembles and quantum theory is a theory about properties of ensembles*". (p8).

Here Belinfante finds support for the idea that Q.M. deals only with ensembles (i.e. actual collections of many, similarly prepared systems) in the 'relative frequency' theory of probability. As we showed in §5.5, the absolute character of the probabilities that occur in Q.M. makes possible an unambiguous definition of probability from the point of view of the 'betting' theory. This type of probability can refer to single systems.

Belinfante also finds support for this view in the 'Schrödinger Cat Paradox' or, equivalently, the results of our Chapter 6¹, where he views the occurrence of a state 'containing' terms corresponding to mutually exclusive measurement results as indicating that such a state must apply to an ensemble.

What motivation do these and other authors have for restricting the domain of Q.M. to large (actual) ensembles of systems, and excluding the quantum mechanical description of a single system? Firstly, we note that Q.M., as applied to ensembles, is deterministic. This is d'Espagnat's (1971) principle of 'statistical determinism' (see §3.4). Each quantum state can be unambiguously related to a distribution of measurement results for an ensemble, depending on the observable being measured. The restriction of Q.M. to exclude single systems means that the predictions of Q.M. are really 'distribution patterns' as opposed to 'probabilities for the occurrence of given measurement results'. The distribution pattern 1. See the quote at the end of this section.

resulting from repeated measurements of a given observable on systems prepared in the same quantum state is completely determined.

The 'existence' problem of measurement, the main problem which we are concerned with here, also falls away, since it can only be arrived at by considering single systems. If we are *not allowed* to talk about single systems (in terms of Q.M.) we cannot arrive at the conclusion (as in §6.1) that, in the double slit experiment, *each* electron cannot pass through either one slit or the other, but must, in some sense, pass through both. Further, we are *not allowed* to infer that, because *each* electron is either detected behind one slit or behind the other, something (S.R.) must happen to its 'existence' at the other slit.

The only remaining 'problem of measurement' is to account for the fact that 'interference effects' between the superposed states for the composite system corresponding to different measurement results are never observed. There are three related methods for doing this. One deals with the introduction of different random phase factors for each element of the ensemble¹. As a result of these random factors, while interference may be thought of as occurring for each system, the net effect will be 'washed out' because the 'interference pattern' will be in a different position for each system, and no net effect will be observable. This explanation is used in the case of the double-slit interference experiment when attempts are made to measure through which slit each electron passes. If the measurement results on each electron are significant, (i.e. greater than the random fluctuations allowed by the uncertainty principle) it can be shown² (see e.g. Bohm (1951), Bohr (1949)) that the state of each electron is disturbed sufficiently for the 'interference pattern' to which each electron belongs to

1. Here we are breaking the restriction of Q.M. to ensembles and dealing with the elements separately
2. See, in this regard, §9.6.

overlap just so as to be unobservable.

Secondly, it is argued that the states of the measurement apparatus are 'macroscopically' distinguishable and hence should be orthogonal. In this case, the cross terms of the inner product (those containing terms from different superposed states) will each contain an inner product of state vectors representing macroscopically distinguishable states. If these are orthogonal, their inner product must be zero. In this case, all the 'cross-terms' will be zero. Since the interference effects arise from these cross-terms, this may be taken as proof that no interference effects can occur. We note that the term 'distinguishable' introduces the abilities and limitations of the observer. We cannot change this term to 'distinct', since interference between 'distinct' states (i.e. passing through the left/right slit) certainly do occur.

Finally, it has been noted by some authors (e.g. Moldauer (1972)) that the final state of the composite system is a *non-separable* superposition of states for the composite system. In order to observe interference effects it would be necessary, according to this view, to perform a measurement on the *composite system* i.e. *both* the object system *and* the measurement apparatus.

Moldauer (1972) writes about this in terms of the Schrödinger cat paradox:

"The veterinarians' stethoscope examines only the cat, and not simultaneously the atom, to discover whether the animal has been (quite literally) "collapsed" by its interaction with the atom. And a separate radiation counter will tell us about the state of the atom.... It is even very difficult to think of an apparatus ... that measures the combined condition of cat and atom. If we attempted to learn this combined state by means of a scattering experiment, we would have to scatter a wave whose wavelength λ is large enough

to encompass both the vital organs of the cat and the atom in order to detect their coherent state. Such a λ would be clearly too large to be sensitive to the details of the internal state of the atom. In fact, it appears that the complexity required for such a measurement is comparable to the complexity of operations required to revive a dead cat". We shall consider this question again in §7.5 and §8.4.

How ever we choose to account for the non-occurrence of interference between superposed states which include terms relating to different measurement results, this is claimed to be sufficient, provided we are restricted to dealing with ensembles only, to 'solve' the problem of measurement. The predictions obtainable from the superposed final state of the composite system (as in the r.h.s. of Equation 6.4) can be no different from those obtained by substituting a 'mixture' of states corresponding to different results. Daneri, Loinger and Prosperi (1966) show that 'if the instrument is ergodic, then a mixture of the above described type exists which is suitable at a time t and *remains suitable* at all times posterior to t '. (d'Espagnat (1971) p282). Hence, it is argued that we might as well replace the superposition by a mixture. We find the 'ad hoc' nature of this replacement disturbing in the face of the fact that it is exactly what we are trying to explain. If the 'mixture' and the 'superposition' give exactly the same predictions, why bother to replace the superposition by a mixture? d'Espagnat (1971) expresses our view concisely when he says

"... it is *in practice* always possible to attribute definite macroscopic properties to the measuring instruments. The danger that the error we thus make should ever be detected has been proven to be vanishingly small. These theories do *not* show, however, that the instruments can, without contradictions, be said really to *have* these properties...." (p283).

If we are allowed to deal with single systems, then, even if we can be sure no interference takes place, we still have to account for the fact that the 'physical existence' of all but one of the possible measurement results vanishes on measurement. This is most forcibly illustrated in Chapter 8.

However, our main quarrel is not with the arguments that no subsequent interference can occur, but with the restriction of the domain of Q.M. to ensembles. Our main objection to this restriction is that it is unprecedented and artificial. Even in the case of classical statistical mechanics, where a relative frequency theory of probability *is* indicated, we are still not prevented from asking the question

"What actually happens to a single system?" In fact, it is just by answering this question (many times over) that we determine the properties of the ensembles. We see no logical reason why a similar question should be arbitrarily excluded from Q.M., especially since experiments have been done (by e.g. Janossy and Naray (1969) Clauser (1976)) which show that interference effects occur for single systems. The restriction to a description in terms of ensembles only precludes the treatment of a single electron, and yet it is incorrect to assert that Q.M. gives no information on such a system. If we know its quantum state (either by measurement on similarly prepared ensembles or by theoretical analysis of the preparation apparatus) we can enumerate the possible measurement results as well as give the relative likelihood of the occurrence of each. However, the treatment of single systems by themselves is seldom of interest. It is rather in the treatment of single systems as the possible elements of ensembles that our interest lies. Since ensembles can be built up by repeating 'single system' experiments (which yield the same results as those predicted by Q.M.) the elements of the ensemble must be non-interacting and the quantum state must apply to each single system. That it is the *same* quantum state follows from the fact that the systems are so prepared. That the actual physical situation

is initially the same in each case follows from the fact that H.V. theories are untenable (§5,3) and Q.M. is assumed to provide a 'complete' description of physical reality (§5,4). Instead of excluding the possibility of dealing quantum mechanically with single systems, we would derive the properties of an ensemble of similarly prepared systems from the properties of each element of the ensemble (in 'complete' Q.M., these must be identical since they are described by the same state). This is in line with the treatment of ensembles in classical theories. We also contend that it is more satisfying from a realist point-of-view to be able to consider what happens to each system. From this realist point-of-view, an a priori restriction to dealing with ensembles makes no sense; if ensembles of large numbers of systems are the 'smallest' entity we can consider, why are they considered to be 'ensembles' and not 'atoms' of some kind or another?

Finally let us note that here, as in all choices between regulative principles (the principle at stake here is 'Q.M. cannot give information relating to single systems') the arguments of neither side can have logical force. It remains a matter of 'taste' or preference as to whether or not we accept this regulative principle. We, of course, consider our arguments to be persuasive! The assumption of this regulative principle is at odds with our aim of understanding 'what goes on' at the microphysical level in a realist, objective fashion. Indeed, it seems that the restriction of the application of Q.M. to ensembles is just a *device* to avoid the difficulties and novel concepts involved in the description of S.R. This is evident in Belinfante (1975) who says "..... The statement that ψ describes an ensemble, obvious without explanation to many, is at the basis of much of the following, and therefore does require a few paragraphs of explanation in view of the tenacity with which others have tried to adhere to the idea that state vectors ψ should describe individual elementary systems". Whence, I wonder, does he think this tenacity arises? He goes on:

"As these persons usually have found that this interpretation of ψ leads to difficulties in understanding quantum theory, arguments in favour of the conventional interpretation of ψ as describing ensembles ought to be welcome to them as a way out of their difficulties". He goes on to justify this 'opportunistic' methodology on the basis of the empiricism described in the last section, and rejected in §1.8.

"That this 'easier interpretation' does not explain everything is obvious. Quantum theory *does not claim* to explain everything. It merely claims to *describe* and *predict* the behaviour of *ensembles* of elementary systems. If it could explain or predict what in the one universe we know exactly happens to one individual elementary system we meet, the theory would be deterministic like a hidden variables theory, and would be entirely different from the conventional theory which we want to discuss here" pps 7 - 8.

Let Belinfante's quantum theory speak for itself. Our quantum theory does not claim anything, but *we* claim that it should be possible to use it to interpret and understand the behaviour of physical reality at a microscopic level. If this were *not* possible then Q.M. would belong to the more pragmatic disciplines like engineering rather than physics, whose second purpose it could not satisfy.

In his final statement he concludes that, if we could *predict* results for individual systems, the theory would be deterministic. This is a tautology which in no way prevents us from using Q.M. to investigate 'individual elementary' or single systems. In describing non-deterministic reality, we can say *everything* (from a physical point-of-view) about a system *without* being able to predict its future behaviour. In assuming that Q.M. is complete, we assume that this is accomplished by the state vector itself.

7.5 The system after measurement

Much of the criticism of the postulation of S.R. relates to the state of the object system after measurement; whether or not it is an eigenstate corresponding to the measurement result, i.e. whether or not an ideal measurement is possible. This arises from a commonly accepted formulation of the phenomenon of S.R.: that 'on measurement' the state of the system changes from a superposition of the eigenstates of the operator corresponding to the observable being measured to one of these eigenstates (i.e. that corresponding to the measurement result obtained)¹.

This formulation was supposed to account for the fact that subsequent measurements on a system gives results which are correlated with the first one.

As an example, Pearle (1967) considers consecutive measurements of the position of a particle in an evacuated room. Suppose the room is divided into N zones of equal volume, and equipped so that, at anytime, the presence of the particle in one of the zones can be detected. We suppose that the measurement does not destroy the particle. Let the eigenvectors of the operator A associated with this measurement be $\{|\psi_i\rangle\}$ for $i = (1, \dots, N)$ with eigenvalues i

$$\text{i.e.} \quad A |\psi_k\rangle = k |\psi_k\rangle \quad 7.4$$

If the room is large enough, and if we wait long enough, the state of the particle will be given by

1. We note at the outset that the final state of the object system has no bearing whatever on our difficulty with the quantum mechanical description of measurement (without S.R.) as expressed in Chapter 6.

$$|\psi\rangle = \sum_n a_n |\psi_n\rangle \quad \text{where } |a_i| \approx |a_j| \quad \text{for all } i, j \quad 7.5$$

and $\sum |a_n|^2 = 1 \quad 7.6$

That is, the particle is in the room, and it is equally likely that it will be found in any one zone.

Suppose that we now perform a position measurement and obtain result k : the particle is in the k^{th} zone. Now, whether or not this is an ideal measurement, as long as the particle is not destroyed by the measurement, a second measurement very shortly after the first cannot give as a result that the particle is very far from the k^{th} zone.

However, Pearle contends that, if no S.R. is assumed to occur, the state of the particle after measurement will again be $\sum_n a_n |\psi_n\rangle$. This implies that the second measurement result will indicate that the particle is in any zone, with equal probability, an empirically incorrect result. Pearle therefore assumes that S.R. is postulated to account for the fact that the results of two subsequent measurements are correlated. i.e. After the first measurement and S.R., the state of the system will be $|\psi_k\rangle$ (or $|\psi'_k\rangle = \sum_n b_{nk} |\psi_n\rangle$ where n is restricted to those zones near to the k^{th} zone). A second measurement on this 'reduced' state will show the desired correlation.

Of course, Pearle's assumption that, in the absence of S.R., the state of the object system is given by $\sum_n a_n |\psi_n\rangle$ is incorrect: as we have seen, the state of the object system alone is *not* given after measurement, in the absence of S.R. Only the state of the composite system consisting of the object system and the measurement apparatus is given. If the measurement system M has initial state $|M_0\rangle$ and states $\{|M_i\rangle\}$ corresponding to measurement

result i , the final state of the combined system after the first measurement will be given by

$$|\psi\rangle = \sum_n a_n |M_n\rangle |\psi'_n\rangle \quad 7.7$$

By considering the correct formulation for the state of the combined system after measurement, Pearle accounts for the correlation between subsequent measurement results. For the measurement system to yield two successive measurement results which are comparable afterwards, it must record both results (even if we have to include the (physical) 'memory' of the experimenter as part of the apparatus). Thus, we require that the final state of the measurement apparatus reflect both results. To this end, let the state of the measurement apparatus prior to any measurements be $|M_{00}\rangle$. After the first measurement on the system in state $|\psi_i\rangle$ it will be $|M_{i0}\rangle$ and after a second measurement on the system in state $|\psi_j\rangle$, it will be $|M_{ij}\rangle$. Hence, the time development of the composite system (object system + measurement apparatus) over time intervals in which both measurements occur should be given by

$$|M_{00}\rangle (\sum_n a_n |\psi_n\rangle) = \sum_n a_n |M_{00}\rangle |\psi_n\rangle \rightarrow \sum_n |M_{n0}\rangle |\psi_n\rangle \rightarrow \sum_n |M_{nn}\rangle |\psi_n\rangle \quad 7.8$$

in the case of ideal measurements. This gives the desired correlations. In the case of non-ideal measurement we should write

$$\sum_n a_n |M_{00}\rangle |\psi_n\rangle \rightarrow \sum_n a_n |M_{n0}\rangle |\psi'_n\rangle \quad 7.9$$

where $|\psi'_n\rangle = \sum_k b_{nk} |\psi_k\rangle \quad 7.10$

However, since the particle is constrained to travel at finite speed, at time $t + \epsilon$ ($\epsilon \rightarrow 0$) even in the event of disturbing measurement, it cannot have travelled far from the zone indicated

by the first measurement. Hence b_{nk} is finite only for values of k such that zone k is within some finite range of zone n at time $t + \epsilon$. Thus, the development of the system over the time interval in which the second measurement occurs must be given as

$$\sum_n a_n |M_{n0}\rangle |\psi'_n\rangle = \sum_{nk} a_n b_{nk} |M_{n0}\rangle |\psi_k\rangle \rightarrow \sum_{nk} a_n b_{nk} |M_{nk}\rangle |\psi_k\rangle \quad 7.11$$

Now the constraint on the values of k for which b_{nk} is finite ensures that the final state of the measurement apparatus $|M_{nk}\rangle$ still reflects the desired correlation.

Thus, Pearle has shown that the desired correlation can be obtained by a quantum mechanical treatment without S.R. As he assumes that the only purpose of S.R. is to account for this correlation, he considers that this is a demonstration that the postulation of S.R. is unnecessary.

The fact that an (almost) continuous line is obtained as a particle trajectory in a Wilson cloud-chamber can be accounted for by an analogous treatment, and S.R. is not needed to explain why subsequent ionizations and droplet formations are along a trajectory and not randomly scattered.

Belinfante (1975) concludes that the postulation of S.R. is 'optional'. By using S.R. we can give the state of the object system by itself, after measurement, and obtain the correlations shown above, without the complicated description involving correlated non-separable states. Belinfante therefore considers the introductions of S.R. to have pragmatic value as a labour-saving device. As such, its usage requires no other justification than that it gives the correct results.

We note, however, that these considerations leave the 'existence problem' untouched; this problem is to account for the fact that each particle yields a *single* trajectory in a Wilson cloud-chamber or gives a *single* pair of position measurements, despite the fact that the final state of the composite system contains terms relating to *all possible* trajectories or pairs of position measurements.

To summarise, Pearle accounts for the correlations between subsequent measurement results by a correct application of Q.M. He does not account for the fact that definite unambiguous results are obtained in measurements on quantum states. His conclusion that S.R. is unnecessary is based on a mistaken idea of the reason for introducing the concept.

7.6 Other objections

Pearle (1967) also makes the assertion that the difference between the superposition $\sum_n a_n |M_n\rangle |\psi_n\rangle$ and the mixture ' $|M_n\rangle |\psi_n\rangle$ with probability $|a_n|^2$ ' can be demonstrated by a measurement on the composite system. He considers such a measurement to be so complicated as to be impossible. This is the same as the point-of-view of Moldauer (1972) quoted in §7.4. We present a counter-example in Chapter 8, by comparing the state of a system for which interference *does* occur with that resulting from the treatment of measurement.

Moldauer (1976) points out that the measurement chain considered in §6.4 should not be *nested* (where, for an object system I and two measurement systems II and III, III performs a measurement on (I + II) and not just on II), as in the our treatment in Chapter 6 where we consider that each measurement apparatus interacts with a composite system (or object system, in

the case of the first measurement) and not with a single system. Moldauer's view is in accordance with what actually happens in measurement. The measuring instrument interacts with the object system. Suppose it has a visual display. Then the illuminating light interacts with the measurement system *only*, and not the object system as well. Likewise, the eye of the observer interacts *only* with the light, and neither with the measuring apparatus nor the object system. However, the mathematical treatment is identical.

In a private communication (submitted to Epistemological letters) Moldauer concludes, on this basis, that measurement effected by such a chain does *not* give the state of the combined system, even though the states of all the component systems can be known. i.e. Even though we know, as a result of measurement, that the object system is in state $|\psi'_k\rangle$ and the measurement apparatus is in state $|M_k\rangle$, Moldauer asserts that we can not say that the state of the 'combined' system is $|M_k\rangle|\psi'_k\rangle$. On the contrary, it must still be given by the superposition $\sum_n a_n |M_a\rangle|\psi'_n\rangle$. That definite unambiguous results are obtained in measurements on quantum systems follows, in Moldauer's conception, from the fact that we observe only *separate* parts of a non-separable system.

While there does not seem to be any *logical* objection to this point of view, it is certainly not in accord with the idea that a single unambiguous reality exists, independently of our desires and our point-of-view. If Moldauer and those who agree with him are prepared to say, while looking at a measuring instrument, and perceiving its indicator in a definite place, that it is 'actually' involved non-separably with the object system in a quantum state which does not indicate any particular measurement result (in particular the one they perceive) and

further to infer that the object system 'by itself' has the state indicated by the measurement result, then there is nothing to stop them from rejecting the concept of S.R. While we agree that there are situations in which the "whole is greater than the sum of its parts", we cannot accept this ambiguity in the existence of physical reality. We feel that it is altogether more credible that a non-deterministic random change (such as S.R.) should occur than that the existence of elements of physical reality should be this ambiguous. This remains a matter of personal choice, but we choose to disagree with Moldauer.

In order to escape the conclusion that physical reality is ambiguous in this manner, we could reject the assumption that Q.M. is complete. However, this excludes the possibility of using Q.M. to infer what microphysical reality 'is like'.

In the same communication, Moldauer says that "...it is clear that in going from the correlated mixtures to the pure components (of the mixture), we proceed from a description of the ensemble to a description of an individual member of that ensemble. This transition can therefore in no way be interpreted as representing a physical change in the individual system". However, in describing the final state of the separate systems as a 'correlated mixture', Moldauer has already made the significant step.

While the separate systems may give the same results whether described by a non-separable superposition or by a 'correlated mixture'¹, this does not mean that the descriptions are equivalent.

1. The only description for such a correlated mixture that is known of by the present author is a mixture of the states $|M_n\rangle|\psi'_n\rangle$, exactly what Moldauer asserts that it is not!

Each particle in the ensemble is described by the non-separable superposition, whereas the particles described by a mixture either 'have' one of the previously superposed states or another. Moldauer solves the problem of 'existence' by postulating that it doesn't exist! i.e. He assumes that the 'correlated mixture' and the non-separable superposition are equivalent descriptions because they give the same results. He then uses this assumption to show that they are equivalent when we are not considering the measurement results but the physical system itself.

Leibowitz (1975), who we mentioned briefly in §7.2, contends that measurement, being a matter of *observation* and not *prediction*, is outside the domain of Q.M. In view of our realist aims, we must disagree with him. However, he also says that

"On measurement, the apparatus, *purely randomly*, selects just one of the a_i and ψ_i or a combination of ψ 's in the case of interference". How does he suppose this process can be described dynamically, in terms of Q.M., classical mechanics, or any other deterministic theory? He claims to reject the projection postulate and dismisses measurement theory as outside Q.M. He nevertheless feels the need to explain measurement (in terms of what cannot be Q.M., by his own assumption) in a manner which looks very much like the 'orthodox' or traditional way of explaining S.R. It is difficult to detect any significant difference between the account quoted above and the traditional assertion that S.R. occurs as a result of the interaction between the object system (which is described by Q.M.) and the measurement apparatus (which is not).

Müller (1974) follows Fok's conception of Q.M., especially in this regard, where he says:

"Fok comes to the conclusion that the state function ψ refers to the probability distribution of the *potentially* possible state, while the distribution obtained from the results refers to the *realized* one. This is meant to dispose of the paradox connected with the "jump", or momentary change, of the wave function, since if ψ does not refer to the realized but to the potentially possible state, its "jump" during the measurement does not mean the structural transformation of the object - that is a physical process of non-temporal character - but a change in the relationship of the object and the external conditions" (p.40). We feel that these semantic gymnastics, much like those of Leibowitz, are not in keeping with the realism we see as fundamental to physics. From a realist standpoint, such a change in the 'relationship of the object and the external conditions' should be explained in terms of the behaviour of the object and its environment, using the relevant theory. In Chapter 6 we showed that this cannot be done by Q.M. without the introduction of S.R. Müller explains the behaviour of systems during measurement by the existence of a special class of 'measurement-like' interactions (see Müller (1974) p. 40). This is very much like the interpretation considered in §9.2 where S.R. is assumed to occur as a result of such interactions.

It will have been noted by any reader familiar with the literature that we have, so far, dealt very skimpily with the work of Nils Bohr, despite the fact that he has written profusely and profoundly on many aspects of the subject of our discussion. For instance, we have not even mentioned Bohr's equally famous reply (in 1935) to the paper of Einstein, Podolsky and Rosen which we dealt with in detail in Chapter 2. Indeed, it is difficult to assess the impact that the writings of Bohr have had on the arguments and illustrations which we present here. There is however, a basic point on which we differ, making all of Bohr's work formally inconsistent with our approach. In adopting the dualistic doctrine of complementarity Bohr, 'explains' many of

the problems and 'paradoxes' which concern us, either by asserting that they are formulated in terms of 'meaningless' questions or else by incorporating them as fundamental ideas.

In §6.2, we mentioned our difference of opinion as to which theory should be used to describe the measurement apparatus. Another example is afforded by our treatment of the double-slit interference experiment in §6.1. According to the doctrine of complementarity, it is not possible to compare systems if they are to be subject to measurements which are 'complementary' (i.e. those that cannot be performed simultaneously; measurements of those observables whose corresponding operators do not commute). For this reason, while it *may* be possible to infer from the interference experiment that electrons 'pass through' both slits, we cannot assert that this is so in the case when we place detectors immediately behind each slit, since this is a 'complementary' situation. From our realist point-of-view and the assumption that Q.M. is 'complete', it follows that physical systems which are prepared in the same way, to have the same quantum state, must be the same. In as far as we can deal with this type of situation in terms of Bohr's positivistic doctrine, the way the system behaves depends on the measurements that *will be performed* on the system in the future. Clearly, Bohr's doctrine can only be interpreted in a realistic manner by the assumption of non-local forces. This non-locality must, furthermore, be of the same unsavoury kind as that concomitant with H.V. theories (see §5.3) since it involves admitting situations in which 'effects' precede their 'causes'!

We feel that Bohr's rejection of realism (materialism, in Müller's (1974) discussion) can only be justified in the case that no realistic interpretation of Q.M. can be found. We feel that his proscription of the processes of inference used to describe

'physical reality' in classical theories is a little hasty. Bohr's enthusiasm for conceptual change is understandable at a time when the foundations of physics were being rocked by new discoveries. Nevertheless, no matter how commendable such revolutionary imagination may be, we must take care that we do not allow it to carry us off into the depths of *unintelligability*. Judging by the variety of interpretations which are ascribed to Bohr by his followers, and which go under the name of 'Copenhagen Interpretation' after his school, it seems that this is where the imagination and zeal of Bohr for new ideas have lead us.

It may be that we shall find a consistent realistic interpretation of Q.M. impossible. In this case we should return to the work of Bohr for guidance. However, as we have indicated in §1.8 and elsewhere in the present work we feel that physicists should be extremely loth to give up the doctrine of realism, at least as an ultimate goal.

7.7 The Many Universe's Interpretation of Q.M.

There is an interpretation of Q.M. due to Everett and Wheeler, and championed by de Witt (1971) known as the 'Many Universe's Interpretation' (M.U.I.) in which it is claimed that the concept of S.R. is unnecessary. As we shall see it contains the phenomenon of S.R. in a disguised form. Neveththeless, this interpretation has some advantages from a logical (if not a conceptual) point-of-view and it is consistent with our interpretation of Q.M. We shall therefore review it briefly.

The conclusion of §5.3 that Q.M. cannot be rendered deterministic by 'splitting the cause' as in classical stochastic theories can be interpreted as follows: If we suppose that the statistical predictions of Q.M. arise from the fact that the quantum state describes an ensemble of essentially different

possibilities, then we must conclude that, in order to account for interference phenomena, the different elements of this ensemble must interact. This is very disturbing from a classical point-of-view since, in such theories, the different elements of an equivalent ensemble cannot interact, since, in each case, *only one element actually exists*. We cannot make this conclusion in the quantum case, but must assume that *all* elements of such an ensemble must *exist*, in each case, for them to interact.

From this standpoint, the problem of measurement which interests us is to account for what happens to all the possible outcomes other than the one actually found in a given measurement. In the M.U.I. it is assumed that all such possibilities continue to exist after measurement (or, more accurately 'measurement-like interactions') but that they exist in different universes. Every time a measurement or a 'measurement-like' interaction occurs, the universe 'splits' into at least as many components as there are possible results¹. In each resulting universe or branch, the observer sees a different result. In this way, the entire universe (meaning the 'super-set' consisting of all branches) can be described by a quantum state which evolves deterministically as specified by the Schrödinger equation or its equivalent. The observer is considered as an 'automaton' i.e. no more nor less than a physical system consisting of his body², who also 'splits'

1. In the case of a measurement involving an observable with a continuous spectrum of eigenvalues, this implies the occurrence of a non-denumerably infinite number of 'branches'. Some critics find this a bit hard to swallow!
2. In so doing, the complete identification of 'interacting with' and 'seeing directly' is achieved. Consciousness is relegated to the position which it occupies in classical physics: while it is somehow related to certain physical systems, (especially living human bodies) these systems can be described physically without reference to it, and it is therefore excluded from the domain of physical theory.

whenever a measurement-like interaction occurs. That we do not experience any strange, schizophrenic sensations as a result of this follows from the fact that the consciousness of the observer which is associated with his body also 'splits'. de Witt concludes from the fact that each of the summands of the superposition of states of the composite system (object + measuring instrument + observer) contains terms relating to the same result only (a consequence of the requirement that the observer and apparatus function as measurement systems) that an observer conscious in one branch cannot be aware of consciousnesses or bodies or other physical systems in other branches. He likens the criticism that we cannot feel the universe split to the anti-Copernican argument that we cannot feel the earth move: an apt comparison in that both phenomena, puzzling in terms of the old theory, are explained in terms of the new.

From this interpretation, the protagonists of the M.U.I. are able to deduce the statistical predictions of Q.M. by considering the viewpoint of a single consciousness moving through time on a single branch of the universe. This is, perhaps, the strongest point of this interpretation. Instead of having to introduce a non-physical entity (observer) which 'observes' the system in a given quantum state in order to introduce the statistical predictions of the conventional physical formalism, these predictions can be obtained as a *consequence* of the M.U.I. (where the observers are simply physical systems) and the algebra of quantum states. Many critics state that the M.U.I. can be faulted on the basis of the principle of economy of postulates (Occam's Razor). On the contrary, as we see from the above analysis, the M.U.I. shows up better against this standard than interpretations employing the orthodox physical formalism. What the M.U.I. *does* multiply, without bound, is universes. Since all but one of these is unobservable to any one branch (and hence to us who read these words) we might argue that they are unnecessary, since they have no physical consequences. This

criticism can be repudiated by pointing out that the consequences of the existence of other branches are precisely the form and behaviour of microphysical systems as described by Q.M! The M.U.I. provides a possible description of what happens to the 'existence' of the other possibilities on measurement.

What particularly concerns us is the claim that the M.U.I. dispenses with the need for S.R. If we view the world from the local point-of-view of a consciousness related to a single branch (as we must) then the 'splitting' of the universe would be manifest as the disappearance of all the propensities except one in a given measurement on a single system. This corresponds exactly with our notion of S.R. Prior to the branching, the observer must describe the state of the system as a superposition of terms each relating to different measurement results. This is necessary in case the intended experiment is replaced by one in which interference between the superposed terms can be observed. After branching has occurred, he can safely describe the system in terms of *one* of the states which were initially superposed. This change in description of the system can be written just like Equation 7.2 which represents S.R.

de Witt claims that, in the M.U.I., this represents nothing more than a change in the knowledge of the observer. i.e. The observer becomes aware of in which branch of the universe he is conscious. He stresses that it has nothing to do with a change in the physical system itself and is therefore not a physical transition at all. de Witt first assumes that branching occurs, and thereafter shows that no *further* physical change is necessary. This is similar to Moldauer's argument where he first replaces the superposition by a 'correlated mixture' and then shows that S.R. is unnecessary. In both cases, we contend that the *first* change corresponds to S.R. In the M.U.I., S.R. corresponds to the branching of the universe. In Moldauer's terminology, it is the

occurrence of S.R, that allows us to replace the superposition by a 'correlated mixture'.

It is true that it is not necessary to introduce S.R. as a separate postulate in the M.U.I. This is simply because it has been introduced already as the postulate that the universe splits when 'measurement-like-interactions' occur. It is useful to bear in mind that, in the M.U.I., measurement can be accounted for without introducing any processes which are not described by Q.M. The behaviour of the universe as a whole, which is deterministic, is adequately described by the deterministic Q.M. formalism. However, this is achieved only by viewing the universe from a position which is not available to any observer (except, perhaps, God!). The 'local' observer will see splitting or branching as a non-deterministic transition and he will not be able to apply Q.M. 'locally'. In order to build up a 'picture' of what is happening in a single universe, we will describe 'branching' as the non-deterministic phenomenon of S.R.

de Witt gives as a criterion for branching the occurrence of a 'measurement-like interaction'. However, if measurement is to be described in terms of Q.M., it is difficult to see what special characteristics some interactions must have in order to be 'measurement-like' and to cause branching.¹

In attempting to distinguish between measurement-like and non-measurement-like interactions we are faced with exactly the same difficulties as in looking for objective criteria for S.R. In the next chapter, we take up this problem. The arguments considered there can be 'translated' into M.U.I. terminology. For instance, the theory due to Wigner where S.R. occurs as a result of the interaction between the consciousness of the observer and the physical system can be translated as a demand that a conscious observer must interact with the measurement system in order for the interaction be 'measurement-like' so that branching should occur.

1. See, in this regard, §9.2.

In conclusion, while the M.U.I. provides an explanation of 'what happens to the other possibilities', and an account of S.R. in terms of Q.M. applied to the whole universe, it does not provide objective criteria for the occurrence of branching or, equivalently, S.R. Since we are interested in the universe from the local point-of-view of a single branch, we will henceforth abandon the term 'branching' in favour of the concept of S.R. However, when we say that S.R. is a non-deterministic phenomenon which cannot be described by Q.M., we should remember that this is so only if we restrict ourselves to the local point of view. Although it is essentially untestable, the M.U.I. reminds us that it is possible to explain this non-determinism in terms of Q.M. and any criterion for S.R. which we come to may be taken as a criterion for a 'measurement-like' interaction.

7.8 Dualism in quantum mechanics

We conclude and summarise our review of the criticisms of S.R. by considering the notion of dualism as it applies to microphysical reality.

In §4.3 we showed that all hidden variables theories consistent with Q.M. must be non-local, and in §5.3 we rejected these theories after demonstrating the unacceptable character of this non-locality. Since any deterministic description of microphysical reality is covered by our definition of a H.V. theory (given in §4.1) we concluded that the behaviour of microphysical systems must be non-deterministic (at least at the level of single systems). Since this means that the behaviour of such a system cannot be determined unambiguously, it is difficult to imagine how it could be completely described by anything other than a stochastic theory such as Q.M. Thus, we assumed that Q.M. is complete. Since microphysical reality is completely described by Q.M., it must be

represented, for a given system, by the quantum mechanical state. Furthermore, the behaviour of the system under given conditions must be represented by the way in which these states transform. Now the quantum state of a system does not specify unambiguously the values that the dynamical variables (such as position, momentum, spin) of the system have at any given time: what it does specify is the *probability* that a measurement of a given variable will yield a given value. Hence, these probabilities or propensities should be taken as the dynamical properties of microphysical systems¹. In a sense, the 'equations of motion' of Q.M. describe how these probabilities transform under given conditions. We can infer that, although a microphysical system cannot 'have' properties like the dynamical variables of classical physics, or the variables which are *measured* in Q.M. (observables), it can 'have' these propensities or probabilities.

However, if we construct our 'picture' of microphysical reality (microphysical ontology) on this basis (and it is difficult to imagine how it could be constructed on any other), we are at once faced with a serious problem. The 'reality' which we observe, and to which we are accustomed does not appear to consist of probabilities. Indeed, if it did, we would have to ask what the probabilities represented, and what was more or less probable. Even in measurements on microphysical systems, we never simultaneously see mutually exclusive results, each occurring with a given probability. Clearly, we always find measurement results which are entirely present or entirely absent. Herein lies the problem which we have been discussing.

Many authors respond to this situation by postulating, either implicitly or explicitly, that reality is dualistic: microphysical reality is completely described by the propensities of Q.M.

1. Margenau calls this phenomenon 'latency'.

These somehow give rise to the definite properties of macrophysical reality and the corresponding definite sense impressions. Bohr subscribes to this duality explicitly in his Principle of Complementarity as well as implicitly, by insisting that the measurement apparatus be described classically. Leibowitz distinguishes between prediction and observation. This is similar to Moldauer's point-of-view where we can ascribe quantum states corresponding to a definite result to both the measurement apparatus and the object system *separately*, whereas the state of the *composite system* is given by a superposition of such states, each corresponding to a different measurement result. Fok likewise contends that there is a dualism between the 'potentially possible' described by Q.M. and the 'realized' regarding measurement results. Margenau, in addition to his other objections to S.R., deals with this problem by insisting on a dualism between state preparation and measurement, in that he argues that a measurement on a quantum system cannot be used to prepare a given quantum state. Park and Margenau (1968), referring to work presented by the latter in 1937, write

"Incidentally, the possessed quality of classical observables brought the concepts of measurement and preparation conceptually close to one another. Since a measurement operation simply revealed a possessed value, the same operation could also be called a preparation method for obtaining systems having that value of the measured observable. Despite such classical intuition, however, the constructs measurement and preparation must be severed in quantum theory. Failure to do so leads to the projection postulate with its attendant physical and philosophical problems". The M.U.I., in as far as it avoids S.R., postulates a plurality of realities in the many universes resulting from each measurement,

No discussion of dualism in Q.M. could be complete without treating the notorious "wave-particle duality". Many authors (e.g. Park (1974)) contend that this duality arises from applying out-moded concepts to microphysics. He contends that 'wave' and 'particle' are classical concepts which should not occur in a rigorous formulation of Q.M. Although this may be correct, strictly speaking, we feel that the concern generated over 'wave-particle duality' is not without premise, and that it should not be dismissed so lightly. It may be more logically satisfying if we rename it the 'continuum/quantum' duality. This duality can be traced back to its 'discovery' when experiments such as that of Michelson and Morley failed to reveal an 'aether' consisting of ordinary matter as a medium of transmission for electromagnetic radiation. The 'electromagnetic field' began, thereafter, to take on a reality of its own, in addition to the older 'material' physical reality. Despite the unsatisfying character of such a duality, between electromagnetic waves, on the one hand, and matter on the other, 'light' was always a wave and never a particle, and 'matter' always consisted of particles, and never of waves. The only problem generated by this duality is the question of the interaction of a continuum with the infinitesimal mass points of point particle mechanics. In the absence of non-local interactions, a point-particle can only 'experience' a vanishingly small part of a continuous wave, and there should be no interaction between 'light' and 'matter'.

The work of Planck, Einstein and de Broglie in postulating a dualistic 'wave-particle' nature for 'light' and matter is favoured by text-book authors, and hence well-known. However, as a result of the quasi-historical presentation of Q.M., an erroneous interpretation of this duality has become quite widespread, if not amongst researchers in Q.M., then, at least, amongst students, lecturers and text book authors. This is that the 'wave' aspects

reality are evident in some phenomena (e.g. double-slit interference using electrons and photons) and not in others, such as the photoelectric effect and scintillation counting, where the 'particle' aspects are evident. Although we agree that certain experiments are useful for demonstrating either the continuous wave-like behaviour or the discontinuous particle-like behaviour of microphysical systems, it is entirely consistent to state that *both* aspects are evident in *every* experiment on microphysical systems. In some cases, the continuous aspects are hidden by the fact that, either interference does not occur, or else the interference effects are 'smeared out' by the introduction of randomly varying phase factors. In others, the discontinuous aspects are hidden by the fact that vast numbers of single systems are detected simultaneously. Nevertheless, we can say that every microphysical system (with the possible exception of those prepared in position eigenstates) exists in a wave-like configuration, occupying more than one position at a time, *prior to measurement*. This follows from the completeness of Q.M. and the fact that, except in the case of position eigenstates, quantum states are non-zero at more than one position at a time.

On the other hand, all detection events have a particle-like or quantized nature. i.e. At low intensities, detection events are seen to occur randomly at different localizations, indicating particle-like behaviour. This is so even when the 'particles' are detected in a distribution corresponding to an interference pattern!

Thus we contend that the 'wave-particle' or 'continuum-quantum' duality is closely allied with the duality between 'potentially possible' and 'realized' states considered above. *The 'potentially possible' states of Q.M. are, in general, wave-like, whereas the 'realized' states are particle-like.*

This 'wave-particle' duality is formally embodied in de Broglie's Theory of the Double Solution or 'Pilot wave theory' (see e.g. de Broglie (1952)) where he argues that the wave and particle aspects of nature co-exist.¹ It is the 'wave' aspect of reality which is described by the physical formalism of Q.M. The particle aspect is 'guided' by the wave aspect until measurement occurs, when the particle aspect gives rise to definite measurement results. The difficulty with de Broglie's theory is that the particle aspect is viewed as interacting with its environment via the wave aspect *except* at measurement, when the particle is detected directly. However, from a realist point-of-view, every experiment consists simply of a sequence of interactions between the system of interest and its environment. There is no prior reason why some of these interactions should be different, in principle, from the others, simply because they are being used to make a measurement on the object system. If the particle usually interacts via the 'pilot wave', it is difficult to see, in the absence of S.R., why it should interact *directly* with the detection apparatus. This is especially so since it is, to a large extent, a matter of choice whether or not a given part of the environment of the object system constitutes a measurement apparatus. i.e. There are no unambiguous objective criteria as to which aspect of the duality should be evident in a given interaction. A similar objection can be raised against the dualisms of Leibowitz, Moldauer, Fok and Margenau: if we are simply describing an on-going sequence of physical interactions, the *names* which we give to different parts of the environment of the object system (i.e. the preparation system, the measurement apparatus) should have no bearing on which aspect of a dual reality occurs. The processes of prediction and observation, observing part of a system or all of it, system preparation and measurement, when seen as conscious acts of human beings can be distinguished and separated at will. However, from the realist point-of-view which we are trying to achieve, preparation and measurement, whether it be of part of a

1. de Broglie's 'pilot wave' interpretation has been referred to as a 'non-deterministic H.V. theory'. Subject to our definition, all H.V. theories are deterministic (and vice-versa).

system or all of it, are simply taken as physical interactions, and as such, these should be described in terms of a single *consistent* theory which applies to *all* physical systems, irrespective of their application in the eyes of physicists.

There is, however, no need to introduce any kind of ambiguity in the nature of reality, provided we are prepared to investigate and solve the physical and philosophical problems attendant on the projection postulate (i.e. the postulate that S.R. occurs) mentioned by Margenau in the passage quoted above. The seemingly dualistic nature of reality can be explained in the same way that we account for the non-deterministic behaviour of microsystems; by regarding S.R. as an objective physical phenomenon that occurs during certain interactions, including all measurement interactions. e.g. Consider the following schematic representation of an experiment:

Each "single system" is produced in a 'wave-like' state (i.e. one which can occupy more than one position or a spacial continuum) which is described by the quantum state with its concomitant propensities or probabilities. It must remain in the 'wave-like' state at least until any interference effects have been produced. At some stage during the measurement process S.R. occurs. In the case of a position measurement, the system 'takes on' a definite position and becomes 'particle-like'. The measurement apparatus likewise 'takes on' a state corresponding to the state of the object system, and a measurement is effected.

This schema, making allowances for the 'loose usage' employed for simplicity, can satisfy the requirements of unreality. However, in order for it to do this, we must find a criterion for S.R. which is objective and consistent. This task, which is not as simple as it may appear, is the subject of the following chapters.

Chapter 8

THE ROLE OF THE OBSERVER

8.1 Introduction

It is a conclusion of the preceding chapter that measurement cannot be described in a realist manner without the introduction of S.R. as an objective phenomenon. Further, as we showed in Chapter 6, S.R. cannot be described in terms of the deterministic relationships of ordinary Q.M.¹ Thus, in order to introduce S.R., it would seem that Q.M. must be changed in some way, either by restricting the domain of applicability of Q.M. or by changing Q.M. as it applies to all physical systems. The former possibility is considered in §9.1 and the latter is the main subject of Chapter 9. However, there is a third possibility where Q.M. may be considered to apply, unchanged, to all physical systems. S.R. is introduced as the result of a 'non-physical' interaction between the 'consciousness' of the *observer* in a particular experiment and the interacting physical systems which constitute the 'measurement-chain' and which must include parts of the body of the observer.

This interpretation due to Wigner (see e.g. Wigner (1967)) is sometimes ascribed to von Neumann. However, we consider von Neumann's interpretation of measurement to be non-realistic in very much the same way as the dualistic interpretations considered in §7.8. Von Neumann considers a measurement chain consisting of interacting microsystems, and shows that any number of instruments can be added to the chain without resulting in S.R. Consider, for example, a chain consisting of the object system, measuring apparatus and the eye/optic tract of the observer. We can show, as in Chapter 6, that a description in terms of Q.M. cannot, in general,

1. We shall use the term "Q.M." to describe the theory as is given in the physical formalism, *without* the introduction of S.R.

result in a final state which corresponds to only one measurement result. However, whether we consider the state of the object system alone, that of the object system and measuring apparatus or the state of the whole chain, and apply the dualistic rule for predicting measurement results expected in a measurement on that system, we get the same predictions. From this, von Neumann postulates his notion of psychophysical parallelism : that the place at which we make the 'cut' separating the 'observing system' from the 'observed system' is *arbitrary*. This psychophysical parallelism is unsatisfactory from a realist standpoint. By employing the interpretive rules for obtaining probabilities from a quantum state von Neumann introduces a dualism between systems which observe and systems which are observed. This is an acceptable dualism which has many precedents in physical theories. However, he also insists that the point at which we consider the observed system to end and the observing system to start is arbitrary. In the absence of a dualism between observer and observed, this too cannot be faulted. In our ontology, for instance, 'observer' and 'observed' are both physical systems, and the distinction between them is arbitrary since it does not affect the description of 'what happens'. Both of these assumptions together cannot be realistic. Since the systems are described differently on either side of the cut, due to the dualism introduced, the theory cannot be realist and complete if the principle of psychophysical parallelism is correct i.e. the position of the cut is arbitrary. From a realist standpoint, a change in the description of a system (such as that which occurs at the cut in the measurement chain) must correspond to an objectively occurring phenomenon (i.e. S.R.). The occurrence of such a phenomenon cannot depend on the wishes of the observer (i.e. be arbitrary) in a realist theory.

Wigner¹ assumes that Q.M. is universally applicable to *physical* systems. He interprets the result of our Chapter 6 that S.R. cannot be described by Q.M. to mean that S.R. cannot occur as the result of an interaction between physical systems. He assumes a naive realism in terms of which physical systems and consciousnesses interact. i.e. He assumes that the measurement chain leads into the brain of the observer via the sense-apparatus, and 'up to the door' of consciousness. Here, an interaction between physical reality and the domain of consciousness occurs whereby the result is 'deposited' in the consciousness of the observer. Wigner argues that S.R. occurs as a result of this interaction. This accounts for the occurrence of S.R. while preserving the form and the universal applicability of Q.M.

There are several objections to this schema: the first is that it involves adopting a particular stance with respect to the 'mind-body problem' and the criticism of naive realism formulated by Russell as quoted in §6.3 applies. i.e. The 'Problem of Knowledge' is introduced as an integral part of the description of physical reality. We have indicated (in the footnote on page 122) that alternative views on the relation between consciousness and physical reality are possible. Indeed, it is difficult to see why consciousness should be associated with the last link in the measurement chain (somewhere in the brain) and not other parts of the body of the observer as well. Current research into the function of the human brain indicates that this view in terms of linear chains of interaction is simplistic and that interactions of a dialectical nature occur. Nevertheless, this interpretation provides a realist schema which accounts for the existence of S.R. It still involves a dualism between mind and body, but the distinction between the dualistic aspects is not seen as arbitrary. Subject 1. A theory similar to that of Wigner has been formulated independently by London and Bauer (1939).

to this interpretation, physical reality exists and behaves independently of the wishes and desires of human observers, although its mode of existence is radically altered by interaction with a conscious observer. A difficulty in identifying systems which are associated with consciousness occurs for all but the most anthropocentric amongst us, but if this interpretation should prove to be viable, it may be possible to identify those systems which are conscious (at least to the extent of being able to cause S.R.) by means of empirical tests.

8.2 On the possibility of an experimental test.

We have repeatedly stressed that S.R. must be considered as an objective phenomenon in a realist interpretation of Q.M. This is further exemplified by the introduction of a second observer and the consideration of the minimal requirement of intersubjective agreement on empirical evidence (measurement results in particular). This is the famous example of 'Wigner's friend'. (See e.g. Wigner (1967)).

Consider an experimental situation such as the measurement of spin-component on a fermion where there are two possible outcomes. If the primary observer (myself) observes the detector (a fluorescent screen, say) I either see a flash in one place or the other. By Wigner's postulate, immediately before I become conscious of the flash, photons are in a superposition of the states 'coming from point 1 on the screen' and 'coming from point 2 on the screen,' in so far as non-separability allows us to talk of the states of the photons alone after interaction with the microsystem. The state of my retina, optic nerve and the relevant parts of my brain will go into a non-separable superposition containing terms relating to both possible outcomes. As soon as my 'consciousness' interacts

with the system, I see the flash in one position or the other. Before this occurs, the system is completely described by the statement 'There is a certain probability ($\frac{1}{2}$ say) that I will see the flash in either position'. After I have seen it, however, S.R. must have occurred since the description of the system contains only one possible outcome, with certainty; the outcome I observe.

Suppose that I place a photographic plate behind the fluorescent screen. Then, disregarding non-separability for simplicity, the photons which strike the plate in a superposition of two position states cause the production of spots in two positions on the photographic plate. When I become conscious of the outcome by observing the fluorescent screen the state of the photographic plate is reduced to one consistent with a spot in one position or the other. The correlations in the unreduced state ensure that, when the photographic plate is developed, the result recorded there will be correlated with the observed result.

Suppose, now, that I ask a friend to observe the fluorescent screen, while I leave the room. Thereafter, he is to refrain from communicating the result he observes to me until I myself have developed the photographic plate and looked at it. I then ask my friend which result he observed.

Assuming that my friend is both clear-sighted and honest, we should agree on the outcome of the experiment. There are two possible explanations for this agreement, which is absolutely necessary for any scientific enterprise to succeed. If my friend is considered to be an automaton, then the agreement follows for exactly the same reason as agreement between my observations on the fluorescent screen and the photographic plate occurs. The correlations occur in the unreduced state of the composite system (See e.g. §7.5). However, this means that I must assert that

my friend, along with all the rest of the apparatus and the object system, was in a superposition of the states 'having observed outcome 1' and 'having observed outcome 2' until I caused S.R. by developing and observing the photographic plate.

Alternatively, I can say that he caused S.R. to occur when he made his observation of the fluorescent screen, and my observation of the photographic plate was the same as an observation of a spun coin in the classical description. That is, the result *was* either 1 or 2 after his observation and before mine, but I did not know which it was. (The state of the system was a 'mixture' for me).

Since solipsism is incompatible with realism (as well as with the communal activity of physics) I must assume that the same description would hold if our roles were reversed. However, I have never experienced any mental state, under scientific conditions at least, whereby I have felt that I am in a superposition of states. Neither have I obtained relief from schizophrenic indecision by my friend's developing and observing a photographic film in a nearby dark-room.

Furthermore, at the same time as I describe my friend as being in a non-separable superposition of states (along with the rest of the measurement chain) *he* thinks, if I am to believe him, that he knows the result of the experiment unambiguously. This introduces a dualism of the arbitrary kind unacceptable to the realist. Physical reality is described differently, depending on the point of the view of the observer. If such a description is viewed as complete, it cannot satisfy the requirements of realism.

For these reasons, then, we prefer the second alternative. Once again, this is really a matter of personal choice, but we feel that the reasons for our choice in favour of the second

alternative are both pressing and clear. I can now relinquish my special position as primary observer: all conscious beings possess the ability to reduce superpositions of states involving different measurement results, and thus S.R. is an objective (or, at least, intersubjective) effect.

It is therefore possible that this effect should be detectable, providing an empirical test for Wigner's hypothesis. In order to detect the physical effects of Wigner's hypothesis, it will be necessary to demonstrate the existence of a superposition of states (by interference, say) in a system which does not include an interaction with a conscious observer. Then, with the addition of a conscious observer, S.R. should occur, and the effects of the superposition should thereupon disappear. Although the notion of such an experiment seems manifestly absurd, it is necessary to pursue the consequences of this interpretation to their final conclusion, thereby possibly revealing the reasons for its absurdity.

It is difficult to imagine how to construct two situations which differ only by the presence or absence of a consciousness. The human consciousness carries with it, necessarily, the apparatus of the senses. It may be possible to include the apparatus of the human body without its state reducing element by ensuring that the potential observer is unconscious of the measuring apparatus (including those parts of his body which interact with the measuring apparatus proper). This could be effected by the use of drugs, hypnotism, sleep or by means of rigorous mental discipline on the part of the potential observer. However, none of these measures can ensure that the state reducing element will be absent. It is possible though, to include or exclude an observer in such a way as to ensure that no disturbances of a physical nature (i.e. via the known force types) occur. This

could be done, for instance, by alternating the observer with a dummy which has the same mass, charge, temperature, conductivity and colour distribution as the observer. Such stringent measures should not be necessary since we already know that changes in the inanimate environment of the system (apart from obvious changes to the measuring instrument or the preparation system) do not affect the Q.M. predictions appreciably.

Suppose, however, that we can demonstrate interference between two states of a composite system which contain terms corresponding to observably different states of a macroscopic system, i.e. that macroscopic systems can exist 'in a superposition of states' (albeit nonseparably involved with other systems). If the macroscopic system is observed by a conscious observer to have a definite position, he cannot simultaneously observe interference effects without ambiguity. Therefore S.R. must occur, and we can reasonably assume that this is as a result of the interaction between the physical system and the consciousness of the observer. This would support Wigner's hypothesis. If, on the other hand, we are unable to demonstrate the applicability of the principle of superposition to macroscopic systems, even in the case where no consciousness is present, we can suppose that S.R. occurs independently of the presence of a conscious observer and that Wigner's hypothesis is false.

8.3 The 'Interfering Schrödinger's Cat'¹

Macroscopic bodies are never seen to occur in more than one place at one time, and further, interference between macroscopic states (such as 'cat alive' and 'cat dead') is

1. This experiment and the related analysis is the subject of two papers by Bedford and the author (Bedford and Wang (1976a) and (1976b)).

very difficult to imagine. For this reason, we do not attempt to demonstrate the existence of macroscopic bodies in a superposition of states directly. Instead, we use the macroscopic system as a trigger for a microsystem, with the intention of observing interference effects on this microsystem.

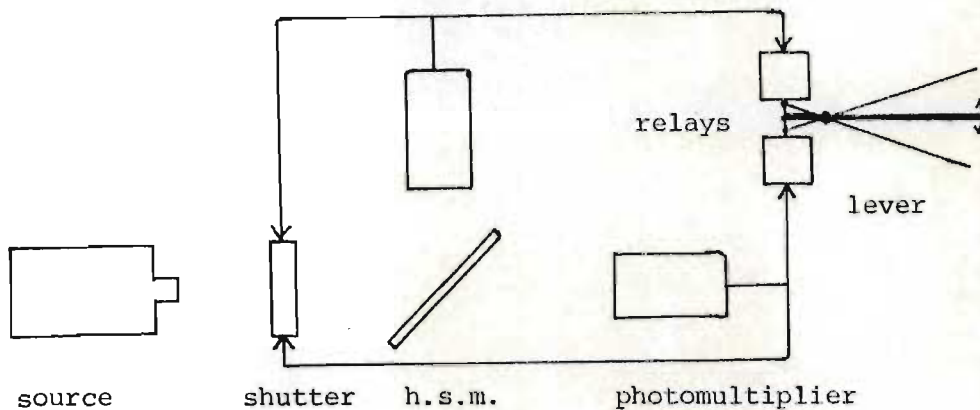


Figure 8.1

In Figure 8.1, a low intensity light source is directed at a massive half-silvered mirror (h.s.m.). Light which is transmitted by the mirror enters a sensitive photomultiplier tube which triggers a shutter and a relay. The shutter excludes the light source, while the relay activates a lever which moves in one direction. Light which is reflected at the mirror activates a second photomultiplier which triggers an identical chain of events, except that the lever is moved in the opposite direction. If the intensity of the light source is sufficiently low, this arrangement makes it extremely unlikely that more than a single photon would impinge on the half-silvered mirror. Suppose that the photon is prepared with initial state $|A_0\rangle$. We know from considerations involving an interferometer that, after interaction with h.s.m., the state of the photon must be given by the

superposition¹.

$$|A\rangle = 2^{-\frac{1}{2}}(|A_T\rangle + |A_R\rangle) \quad 8.1$$

where, $|A_T\rangle$, $|A_R\rangle$ correspond to the states 'transmitted by the h.s.m and 'reflected by the h.s.m.' respectively.

Suppose now that the initial states of the photomultipliers are $|P_{OT}\rangle$ and $|P_{OR}\rangle$ respectively. The evolution of the state of the combined system (photon + multipliers) over a time interval in which the photon interacts with the photomultipliers is given by

$$\begin{aligned} 2^{-\frac{1}{2}}|P_{OT}\rangle|P_{OR}\rangle(|A_T\rangle + |A_R\rangle) \rightarrow 2^{-\frac{1}{2}}(|P_T\rangle|P_{OR}\rangle|A'_T\rangle \\ + |P_{OT}\rangle|P_R\rangle|A'_R\rangle) \end{aligned} \quad 8.2$$

Here $|P_T\rangle$, $|P_R\rangle$ represent the triggered states of the two photomultipliers. $|A'_T\rangle$ and $|A'_R\rangle$ are final states of the photon. Since they both correspond to the state 'photon absorbed', they could equally well be omitted. For convenience, we may consider $|P_{OT}\rangle$, $|P_T\rangle$, $|P_{OR}\rangle$, $|P_R\rangle$ to apply to the relay as well, with obvious interpretation.

Suppose the lever is described by state $|L_0\rangle$ before interaction and by state $|L_R\rangle$ ($|L_T\rangle$) corresponding to 'having moved to the right (left)'. The evolution of the composite system (including

1. Rigorously, the state of the photon alone may not be given after interaction with the h.s.m. In this case, the state of the combined system (h.s.m. + photon) should be given by $2^{-\frac{1}{2}}(|H_T\rangle|A_T\rangle + |H_R\rangle|A_R\rangle)$, where $|H_T\rangle$ and $|H_R\rangle$ correspond to the h.s.m. states 'having transmitted a photon' and 'having reflected a photon' respectively. In our simpler usage, we follow common practice. In any event, this simplification makes no difference to our development.

the lever) over a time interval during which the relay activates the lever is given by

$$2^{-\frac{1}{2}} |L_O\rangle (|P_T\rangle |P_{OR}\rangle |A'_T\rangle + |P_{OT}\rangle |P_R\rangle |A'_R\rangle) \rightarrow 2^{-\frac{1}{2}} (|L_T\rangle |P_T\rangle |P_{OR}\rangle |A'_T\rangle + |L_R\rangle |P_{OT}\rangle |P_R\rangle |A'_R\rangle) = 2^{-\frac{1}{2}} (|T\rangle + |R\rangle)$$

8.3

Now, according to Wigner's hypothesis, this will be the final state of the composite system-consisting of the photon (in an 'absorbed' state'), the photomultipliers and relays and the levers - unless it is involved in an interaction with a conscious observer. This could be brought about by looking at the photomultiplier and relay system (if they give any visual indication of having been triggered) or at the lever itself. If this happens, S.R. occurs and the final state of the system is given by $|T\rangle = |L_T\rangle |P_T\rangle |P_{OR}\rangle |A'_T\rangle$ or by $|R\rangle = |L_R\rangle |P_{OT}\rangle |P_R\rangle |A'_R\rangle$ with a probability of $\frac{1}{2}$ for either outcome.

Suppose that we adjust the lever so that, if it is moved to the left, it covers the left slit of a two-slit diaphragm, and if it is moved to the right, it covers the right slit. If it is unmoved, we suppose that it takes its place in a normal two-slit interference experiment as in Figure 8.2.

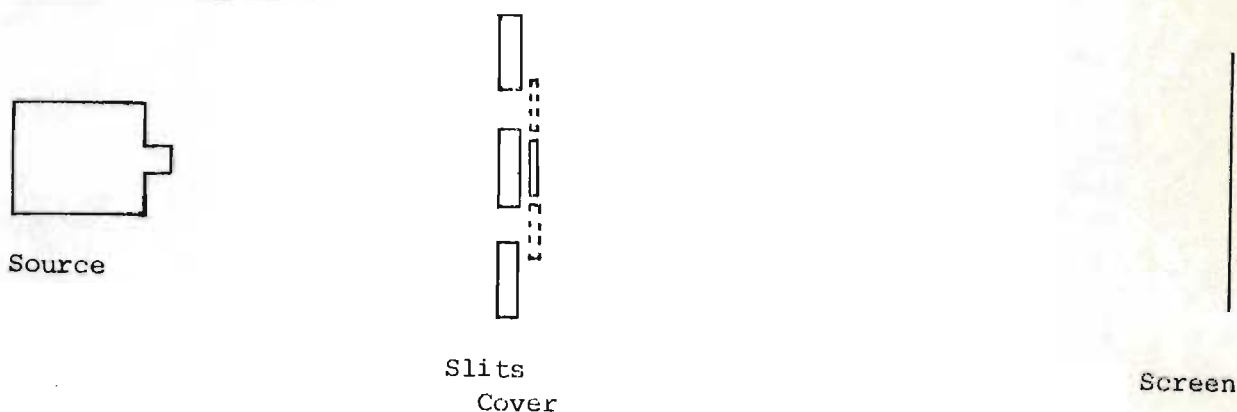


Figure 8.2

Let the photons in this part of the experiment be prepared with initial state $|B_0\rangle$, and let us suppose that the initial state of the double slit assembly is $|S\rangle$.

In the case where the combined state containing the slit cover prepared in state $2^{-1/2}(|T\rangle + |R\rangle)$ as above, the state of the whole system including the slit cover and its preparation system, the double slit diaphragm and the 'secondary' photon will evolve as follows, over a time interval in which the secondary photon interacts with the double slit diaphragm:

$$\begin{aligned}
 & 2^{-1/2}(|T\rangle + |R\rangle)|S\rangle|B_0\rangle \\
 & \rightarrow 2^{-1/2}(|T\rangle + |R\rangle)C(|S'\rangle|B_L\rangle + |S''\rangle|B_R\rangle) + \text{absorbed terms} \\
 & = C'(|T\rangle|S'\rangle|B_L\rangle + |R\rangle|S''\rangle|B_R\rangle + |T\rangle|S''\rangle|B_R\rangle \\
 & \quad + |R\rangle|S'\rangle|B_L\rangle) + \text{absorbed terms} \quad 8.4
 \end{aligned}$$

where $C' = 2^{-1/2}C$ and C is a (complex) number proportional to the amplitude for the secondary photon's passing through the slits. The absorbed terms are all those terms containing photon states which do not influence the final screen. $|S'\rangle$ ($|S''\rangle$) is the state for the double-slit diaphragm corresponding to 'having deflected a photon at the left (right) slit' and $|B_L\rangle$ ($|B_R\rangle$) is the secondary photon state corresponding to 'having passed through the left (right) slit'.

If the slit cover apparatus is described by the state $|R\rangle$ ($|T\rangle$) this means that the right (left) slit is covered. Hence, after interaction with the slit-cover apparatus, the states $|T\rangle|S'\rangle|B_L\rangle$ and $|R\rangle|S''\rangle|B_L\rangle$ must be absorbed terms, which are no longer of interest in calculating the distribution of photons on the screen. Hence, after interaction with the slit-cover assembly, the state of the entire system, given initially by the last term in 8.4, must evolve to

$$C(|T'\rangle|S''\rangle|B_R\rangle + |R'\rangle|S'\rangle|B_L\rangle) + \text{absorbed terms} \quad 8.5$$

i.e. Those parts of the final state for the entire system that affect the distribution of secondary photons on the screen contain terms from *both* slits.

In order to interpret this final state, it is useful to determine the final state of the system when the slit-cover assembly is not triggered at all, since this corresponds to an ordinary double-slit experiment where we *expect* double-slit interference. Since neither the slit cover nor its preparation system take part in this interaction, we can omit its state entirely. Over the time interval during which the photons interact with the double-slit diaphragm, the state of the composite system (photon + double-slit + diaphragm) must evolve as follows:

$$|S\rangle|B_O\rangle \rightarrow C(|S'\rangle|B_L\rangle + |S''\rangle|B_R\rangle) + \text{absorbed terms} \quad 8.6$$

Now Equation 8.6 differs from Equation 8.5 only by the inclusion of the final states of the slit-cover assembly in each case. The only difference, in principle, between these states and the states $|S'\rangle$ and $|S''\rangle$ is that the former are distinguishably different whereas, if we want interference to occur, the latter cannot be.¹ Both pairs are, however, *different*² states of macroscopic systems. Hence, at first sight, we might interpret Equation 8.5 to mean that double-slit interference occurs. We shall see that this cannot be so, but for simplicity, we leave any objections until the following section.

Recall that the final state of the slit-cover assembly is

1. See in the regard Bohr (1949).
2. This may be contested. See §9.3 and §9.4.

only given by Equation 8.3 in the event that no conscious observer has interacted with the system. If an observer sees the slit-cover (or becomes aware of its position by any other means) then, according to Wigner's hypothesis, S.R. will occur and the state of the slit-cover assembly will be given either by $|R\rangle$ or by $|T\rangle$. In this case, we must replace Equation 8.5 by either $|T'\rangle|S''\rangle|B_R\rangle$ or by $|R'\rangle|S'\rangle|B_L\rangle$ (neglecting the absorbed terms). In either case, all the secondary photons must pass, unambiguously, through one slit or the other, and double-slit interference *cannot* occur. We expect to see precisely *one* single-slit diffraction pattern, displaced a little to the right, if the slit-cover is seen over the left slit or vice versa.

Indeed, we could go so far as to set up conditions for two-slit interference by isolating the slit-cover assembly from any conscious observer, and then cause the two-slit pattern to change spontaneously to a single-slit pattern simply by looking at the slit cover!

This result would provide explicit (and astonishing) confirmation for Wigner's hypothesis if it were found to be so empirically. However, as the result of such an experiment proves to be negative (thus maintaining the author's sanity, but cheating him of fame and fortune) we must deal with several objections to the supporting analysis before it can be taken as a refutation of Wigner's hypothesis.

8.4 Criticisms

The most pressing objection which we must meet is that, as was mentioned in §7.4, it is often contended that interference between terms containing macroscopically distinguishable states is impossible. Several reasons were given for this in §7.4. Perhaps the most important, is that macroscopically distinguishable states must be mutually

orthogonal. This is argued in a different context by Wigner (1971). This means that, when we take the inner product to determine the probability for detection of the photon, an inner product between these macroscopically distinguishable states will occur in the 'cross-terms'. Since these are mutually orthogonal, inner products must be zero.

$$\text{i.e.} \quad \langle R' | T' \rangle = \langle T' | R' \rangle = 0 \quad 8.7$$

Hence the 'cross-terms', which give rise to the interference effects, must be zero, and no interference can occur.

We have some objection to the insistence that Equation 8.7 must be valid since the fact that states are distinguishable may depend on our ability as observers. (It is this question of distinguishability which differentiates between Equations 8.5 and 8.6, also). However, this distinguishability can be seen as *objective* by noting that the minimum uncertainties of the Heisenberg Uncertainty Principle, upon which the concept is based, are properties of the quantum state and hence, if Q.M. is complete, of microphysical reality. A second counter argument to this objection is that the quantum states of microscopic systems are only considered to be orthogonal *while* they are distinguishable. Thus, in an ordinary doubles-slit experiment, the states 'having passed through slit 1' and 'having passed through slit 2' are orthogonal if a measurement is made immediately behind the slits. In the case where interference is allowed to take place, they are no longer either distinguishable or orthogonal. It may be that by excluding the possibility of a measurement of the position of the slit-cover assembly, albeit by design, we render the states of this system indistinguishable and hence non-orthogonal.

A second argument against the occurrence of interference in our experiment is that uncontrollably varying quantum mechanical phases are introduced during interactions involving a macroscopic

system.¹ Thus, while interference may indeed occur for individual photons, the 'patterns' to which each photon belongs would be randomly displaced with respect to each other. The net effect would be to smear on any interference that occurred, making it unobservable. While we cannot counter this objection in terms of Q.M., we do note that the 'wave-like' behaviour of electromagnetic radiation is adequately dealt with by Maxwell's equations. Thus, while it is not possible to consider the photons by themselves in terms of Q.M. (due to their inclusion in a non-separable state) it is difficult to imagine how these random phases could be introduced in terms of the phase of an electromagnetic wave. The phase difference is determined electromagnetically by considering the geometry of the situation (i.e. the difference in path length) which is the same in our experiment as in an ordinary double-slit interference experiment. Moreover, while this may indicate a deficiency in the Q.M. treatment of composite systems, it is not sufficiently definite that we should demand that Q.M., which is the subject of our investigation, be changed on this point.

It is clear that our defence of the existence of interference effects in our experiment is insufficient. However, this does not affect the usefulness of our experiment. Even if interference effects do not occur, and cannot be expected, either because they smear out over the detection of the many photons needed to make them observable or because the states of the slit-cover assembly are indeed orthogonal, we must still expect to see *two* superimposed single slit diffraction patterns, according to the interpretation of equation 8.5.

In this case, the expected outcome of the experiment must be slightly altered: in the event that the slit-cover assembly is unobserved, we must expect to see *two* superimposed single-slit diffraction patterns on the screen. When the position of the slit-cover is determined one pattern must vanish, leaving only *one*

1. See, e.g., §9.7.

such pattern on the screen. This experiment strips away the smoke-screen of interference considerations from the 'existence' problem of measurement.

Another class of objection which we anticipate is that, while the state of the composite system is given by the superposition in equation 8.5, the state of any particular system *by itself* must be given by a 'mixture' of the relevant states. Hence, the state of the secondary photons in our experiment must either be $|B_L\rangle$ or $|B_R\rangle$ in each case, and so interference cannot occur. We rejected the dualism entailed by this point of view in §7.8. Nevertheless, it may be as well to demonstrate this as misunderstanding can arise in this context. Firstly, if it is rigorously applied, this argument implies by consideration of equation 8.6¹, that double slit interference can *never* be observed! The notion that the state of a system which is non-separably involved with other systems is given by a *mixture* (when considered by itself) follows from the arguments discussed above which imply that interference effects, which are limited to those arising from the cross-terms in the inner product, cannot be detected under these circumstances. If, however, we assert that the slit-cover assembly is either in state $|T'\rangle$ or $|R'\rangle$, and *hence* that the secondary photons are *all* described either by $|B_L\rangle$ or $|B_R\rangle$, we are extending the argument outside the range of its validity. d'Espagnat (1971) stresses that the description of part of a non-separable composite in terms of a mixture (what he calls an 'improper mixture') is only valid if no *correlations* between the non-separable systems are considered. If we conclude that the secondary photons all have the same state ($|B_L\rangle$ or $|B_R\rangle$) then we can only do so on the basis of a *correlation* between the slit-cover assembly and the secondary photons. In so doing the 'improperness' of

1. We assume that $|S''\rangle \neq |S'\rangle$. This may not be the case. See footnote 2 on page 237 and §9.4.

d'Espagnat's improper mixture becomes evident and the prediction on the basis of equation 8.5, that only one single-slit pattern will be observed is incorrect and invalid.

On the other hand, the statement that the state of the secondary photons is given by a 'mixture' of the states $|B_R\rangle$ and $|B_L\rangle$ implies that two superimposed single-slit diffraction patterns will be detected on the screen. This is in accordance with our conclusion above, and it does not invalidate the experiment.

A third type of objection relates to the description of a macroscopic system in terms of a quantum mechanical state. Zeh (1970) has observed that the spectrum of energy levels of a macroscopic system must be extremely dense, and, as a result of this, it is virtually impossible to isolate a macroscopic system from its environment: a minute change in the mass distribution of the environment, even at substantial distances, is sufficient to cause a transition in energy for a macroscopic (massive) system. Hence, or for other reasons (see §9.3), it may not be possible to describe a macroscopic system simply in terms of a quantum state. It is difficult to see how this could be so, except in that a macroscopic system may be continuously non-separably linked with different parts of its environment. By considering the fact that this observation applies equally to the macroscopic apparatus in ordinary interference experiments, such as the double-slit diaphragm in ordinary double-slit interference, we can show that these considerations have no effect on our analysis: if ordinary double-slit interference can occur, then we must expect to find two superimposed single-slit patterns (or possibly a double-slit pattern) on the screen. The detailed analysis can be found in Bedford and Wang (1976a).

The final objection which we consider is that, although our analysis of the system up to its final state is correct, observation of the distribution of secondary photons on the screen may

constitute a measurement of the position of ~~the~~ slit-cover. In this case, according to Wigner's hypothesis, S.R. would occur, and the secondary photons would be seen distributed on the screen in exactly one single-slit diffraction pattern corresponding to the position of the slit-cover. Indeed, from one point of view, the final observation of the screen does appear to constitute a measurement of the position of the slit-cover. The secondary (double-slit) system is analogous to the illumination of, say, the pointer of a dial used to display the result of a measurement on a microscopic system, and looking at the 'shadow' cast by the pointer. In this case, we expect to see the 'shadow' in a single unambiguous position.

However, in the absence of an unambiguous specification of what constitutes a measurement on a given system, we should be more careful: if the final result of our experiment *is* as we predict, i.e. two superimposed single slit patterns, we would be unable to determine which slit was covered by observing this pattern, and, according to Wigner's hypothesis, no S.R. would occur!

This conclusion is supported by the conditions under which part of a composite system in a superposition of states can be described as a mixture; considered above. If we consider the secondary photons *alone*, we can describe them as being in a mixture of states $|B_L\rangle$ and $|B_R\rangle$. i.e. Each secondary photon can safely be described as having either state $|B_L\rangle$ or $|B_R\rangle$. If this were the case, two single slit patterns, resulting from either state would appear on the screen. In order to assert that a single-slit pattern would be observed, we must suppose that *all* the secondary photons have state $|B_L\rangle$, or else they all have state $|B_R\rangle$. If this is justified on the basis that the state of the slit-cover can be described as a mixture of the states 'covering slit 1' and 'covering slit 2', we are violating the restriction under which

either system can be described as a mixture.

From this point-of-view, when we observe the secondary photons *alone*, we expect to find two overlapping single-slit patterns on the screen. This supports the notion that an observation of the distribution of secondary photons does not constitute a measurement of the position of the slit-cover. How this is possible can be illustrated by considering the arrival of secondary photons at the screen if the experiment is performed at low intensities. Consider the first photon detected at the screen. While S.R. occurs as a result of this observation, this is the reduction of the state of the photon from 'all possible positions on the screen with differing probabilities' to 'exactly one position on the screen with certainty'. We cannot infer the position of the slit-cover from the position in which this photon is observed. If we could, then we could likewise infer through which slit a given photon had come in an ordinary two-slit experiment, and no interference could be observed. Hence, according to Wigner's hypothesis, the state of the slit-cover is not reduced. Thus, when the second photon interacts with the slit-cover assembly, the latter is described by virtually the same state as when the first photon interacted with it. At the time of interaction with the second photon, the slit-cover is certainly not covering one slit or the other since S.R. has not occurred. The positions of the first two photons are not sufficient evidence for us to infer the position of the slit-cover. Therefore, by induction, we can extend this conclusion to any number of photons. It is possible that S.R. would occur when the pattern on the screen becomes distinguishable from the possible alternatives i.e. when sufficient photons are detected for it to be clear that one single-slit pattern is being formed. However, the very formation of *one* pattern depends on S.R. having occurred. In any event, for S.R. to occur in this way depends upon the introduction of new effects as more photons land on the screen. It is difficult to see how a discrete change such as

S.R. could be introduced as the pattern on the screen builds up almost continuously: we could ensure that no single observer was conscious of which pattern is 'favoured' by building up the pattern one photon at a time. Each observer could take a photographic plate containing a single detection event and observe it, fixing the position objectively. When a large number of results had been obtained (this would take an impossibly long time) the results could be superimposed. If one single-slit pattern is obtained, then either some observers will find that 'their' results are missing (as a result of S.R.?) or else we must assert that S.R. had occurred prior to the recording of most of the results. The former possibility is clearly unacceptable, and the latter, since it involves S.R. to one or other result without any observer becoming aware of it, is in contradiction to Wigner's hypothesis. Since it has been shown that 'quantum interference' is independent of the intensities involved, we can expect this analysis to apply at ordinary intensities. We conclude provisionally that either Wigner's hypothesis is incorrect or else two superimposed single-slit diffraction patterns will be seen on the screen, when no conscious observer has interacted with the slit-cover.

8.5 Performing the Experiment

The final objection considered above introduces some doubt as to the outcome of the proposed experiment, even if Wigner's hypothesis is correct. Nevertheless, it is important that the experiment should be performed. In the event that our analysis is correct, it should yield some novel and interesting results.

There are some difficulties which must be overcome in order to perform this experiment. The first is of a purely technical nature and concerns the triggering system which prepares the slit-cover. In the absence of detectors with unit quantum efficiency, we are unable to use the single-photon trigger outlined in §8.3

This problem can be overcome by using a radioactive source and Geiger counter: the distance between the source and the Geiger-Muller Tube and the sensitivity of the counter are arranged and adjusted so that the mean time between counts is Δt . Then, by activating the mechanism for a time $\frac{\Delta t}{2}$, we can ensure that there is a probability of $\frac{1}{2}$ that a count is registered. Since atomic decay is a quantum mechanical process, we suppose that this produces a superposition of the states 'registering a count' and 'not registering a count' with an amplitude of magnitude $2^{-\frac{1}{2}}$ in each case. A pulse from the Geiger-Muller tube activates a relay which moves the slit-cover from one slit to the other. This is the same triggering system as that considered by Schrödinger in the formulation of his 'cat paradox'.

The second difficulty which we consider is of a more fundamental nature, and may indeed be insoluble. Since the nature of any interaction between 'consciousness' and physical systems is unknown, it is not possible to be certain that the triggering system and the slit-cover are sufficiently isolated from any 'consciousness' to prevent S.R. from occurring. Whilst we can ensure that no observer becomes conscious of any clues as to the position of the slit-cover, it may be that S.R. occurs as the result of some subconscious or preconscious interaction. This problem could presumably be avoided by ensuring that any signal (electromagnetic, gravitational, etc.) from the system is below the noise level of the environment.

In order to effect this isolation, a switch was inserted between the Geiger counter and the relay, which was on a long lead. The switch was activated from outside the room containing the experiment. Thereafter, it was assumed that the only significant interaction between the observer and the slit-cover or triggering system would be visual, and this was prevented by suitable screening. A suitable exposure of a photographic plate by the secondary photons was effected by means of a shutter.

In performing the experiment, two types of exposure were made; one taking all the precautions to prevent interaction between any conscious observer and the slit-cover and the other when a deliberate conscious observation and record of the slit-cover's position was made prior to exposure of the photographic plate. The resulting photographs, each representing what appeared to be one single-slit diffraction pattern, were compared under a stereoscope. Not surprisingly, no difference between them was observed.

8.6 Interpretation

Even if our analysis of this experiment in terms of Wigner's hypothesis is correct and S.R. does not occur as a consequence of the observation of the distribution of secondary photons on the screen, this negative result is still inconclusive as a test of Wigner's hypothesis. Some interaction between the slit-cover and a conscious observer may have occurred in which case the state of the slit-cover would have been reduced, and the empirically observed result would be expected. However, as reasonable precautions were taken to prevent such an interaction, it seems unlikely that a different result would be observed if the experiment were performed under conditions under which such an interaction would be impossible. i.e. When any signal from the slit-cover assembly is demonstrably below the noise level of the environment. Certainly, no observer was conscious of the result¹. Hence, Wigner's hypothesis is, at best, applicable in a restricted form: it is not necessary that an observer become conscious of the result for S.R. to occur: the state reducing element of 'consciousness' must occur at some sub or preconscious level.

1. Here we may be in error by assuming that the bacterial or insect life that may have been present in the experimental chamber at the crucial time is not conscious!

Further, if the negative result obtained empirically results from an interaction between an observer and the slit-cover, this interaction must be so 'delicate' as to make Wigner's hypothesis empirically untestable: in any reasonable situation where Wigner's hypothesis is testable, we can expect an interaction between an observer and the apparatus which will bring about S.R., giving a negative result. Hence, if we decide to reject Wigner's postulate, it is unlikely that an empirical demonstration of the incorrectness of our choice will be possible.

Other grounds for rejecting Wigner's postulate arise from two sources. The first relates to the ambiguity inherent in the application of this postulate while the second is concerned with the existence of certain non-local interactions.

Clearly, S.R. cannot occur as the result of *any* interaction between a conscious observer and the physical system of interest. If this were the case, then interference effects (in, say, a Michelson interferometer) could never be recorded, except, perhaps, by remote control. To result in S.R., the interaction must be such that the observer can *distinguish* between the different (superposed) possibilities. This introduces the abilities of particular observers as a criterion for S.R., but a realist formulation may still be possible on this basis. We allow that human abilities affect the way in which physical reality behaves in classical physics. For instance, we could describe classically the procedures used by an artist in producing a drawing. Nevertheless, the fact that artists with differing abilities will produce different drawings does not detract from the physical reality of the pencil strokes and the paper upon which they are executed. However, the difficulty which concerns us here may be stated as follows: if one single-slit pattern is formed on the screen in our experiment, then the observation of that pattern is sufficient to determine the position of the slit-cover. Hence, according to Wigner's hypothesis,

we expect S.R. to occur when the screen is observed, with the result that one single-slit pattern will be observed on the screen. On the other hand, if two slightly offset, superimposed single-slit patterns are formed on the screen, we would not be able to determine the position of the slit-cover from an observation of the screen. Hence, according to Wigner's hypothesis, S.R. cannot occur as a result of this observation. Consequently, we expect to see two slightly offset superimposed single-slit patterns on the screen.

The circularity and concomitant ambiguity of both of the above arguments is clearly demonstrated. By accepting either alternative, we can introduce arguments which support that alternative!

The situation is not much improved by introducing other arguments in favour of one or the other alternatives. Our argument in §8.4 that S.R. cannot occur as a result of the observation of the screen may appear convincing. However, it applies equally well to any observation of a system in a superposition of states corresponding to different measurement results! If this argument is unambiguously valid, we would not see pointers in specific positions indicating different measurement results, but interference effects in the illuminating light, resulting from the pointer's being in all possible positions: S.R. could not occur. On the other hand, if S.R. occurs as a result of the interaction between the observer and the screen, it is difficult to see how interference effects could ever be observed. In short, if Wigner's hypothesis is applied unambiguously, then either S.R. never occurs, in which case single unambiguous measurement results cannot be explained, or else interference is never observed and the principle of superposition and S.R. are both unnecessary. Clearly, neither alternative is satisfactory from empirical considerations. A criterion for S.R. which is ambiguous is unacceptable from a conceptual point-of-view. We note that this

difficulty does not arise if, in contradiction with Wigner's hypothesis, we assume that the state of the slit-cover (and its preparation system) is already reduced to a state containing one possibility or the other, prior to illumination by the secondary photons. In this case, we expect the result which is achieved empirically.

Our second criticism of Wigner's hypothesis arises from the nature of non-locality that occurs in this interpretation. Let us suppose that our proposed experiment is set up and that two superimposed single-slit patterns appear on the screen. We could then trigger a camera by remote control to photograph the slit-cover, without bringing about S.R. The state of the light illuminating the slit-cover assembly would be included in the non-separable superposition describing that system, along with the state of the film in the camera. When the film is developed and observed, however, the image of the slit-cover will appear covering one or the other slit¹. If the state of the slit-cover assembly has not already been reduced, this observation of the film will bring about S.R. Now, we anticipate an objection that a photograph taken when the slit-cover was 'in a superposition of the two position states' cannot show the slit-cover in one place, on the grounds that history cannot be ambiguous. However, this objection is based on a misunderstanding of the behaviour of the camera which, in this case, will fail as a historically correct recording device. It is not that 'history is ambiguous' since, according to our analysis we can assert that the slit-cover *was* in

1. This is a case in which the ambiguity mentioned above arises: in view of the arguments of §8.4, we might expect *two* images of the slit-cover to appear on the film. However, since taking a photograph corresponds more nearly to 'viewing directly' and since cameras have never been known to fail in this way ('The camera never lies!') we assume that one image appears.

a 'superposition of position states' at the time the photograph was taken. The camera simply fails to provide an accurate record.

Suppose that the state of the slit-cover had not been reduced at the time when the film was developed and observed. The S.R. brought about by observing the film would have as a consequence that one of the single-slit patterns displayed on the screen would instantly disappear. This would happen even if the photograph were taken miles away for developing and observation. We expect some sort of non-local interaction to accompany S.R. (See e.g. §5.4). However, by means of the interaction outlined above, it would be possible to transmit intent. The disappearance of one of the single-slit patterns (it would not be possible to determine which) could be used to trigger some chain of events (such as the death of a cat!). An observer could then look at the film at any distance from the site of the experiment with the intention that the cat should die. This would bring about the death of the cat instantaneously. Since this is a situation in which causes and effects are identified by more than just their time-ordering, i.e. by the transmission of intent, we cannot allow the reversal of this time-ordering without introducing logical inconsistencies as considered in §5.2 and §5.3. Since the observing of the film and the death of the cat are separated by space-like interval, they will be seen to occur in different time-ordering depending on the reference frame of the observer, according to the special theory of relativity. Thus, Wigner's interpretation of Q.M. and the special theory of relativity are mutually inconsistent. Indeed, we can extend this discussion to apply to any interpretation of Q.M. in which it is possible to demonstrate the occurrence of a superposition of macroscopically distinct states.

If we assume that S.R. occurs without the intervention of a conscious observer, prior to the inclusion of macroscopically distinct states in the non-separable superposition then this situation cannot arise. Non-local interactions must still occur together with S.R.,

but these involve the transfer of virtual information only. (See §5.2 and §5.6) and so are not prohibited by special relativity.

These conclusions cannot be sufficient to make Wigner's hypothesis *impossible*. The non-local effects considered above are not nearly as disturbing as those considered in §5.3 relating to H.V. theories since in the present case acausal effects cannot be demonstrated in the rest frame of the experiment. However, the special theory of relativity is widely believed and held to provide a correct description of the macroscopic world. If Wigner's hypothesis were correct and testable we would be able to find counterexamples to special relativity.

For these reasons, we would have been very surprised had the result of our experiment turned out to be positive, i.e. if two single-slit patterns had been found on the screen. Nevertheless, we trust that our analysis has made it clear that it is not unreasonable to expect such a result on the basis of Wigner's postulate.

8.7 Conclusions

We have shown that Wigner's hypothesis that S.R. occurs as a result of the interaction between a conscious observer and the physical system of interest (measuring apparatus + object system) is either false or else untestable. This conclusion follows from the empirical negative result of our experiment. i.e. Exactly one single-slit pattern is observed on the screen, even when no conscious observer has interacted with slit-cover. It is reinforced by the fact that, if this interpretation is correct and testable, i.e. if a positive result to our experiment were possible, a counterexample to the special theory of relativity would be obtained.

In view of the ambiguity which this interpretation gives rise to, as well as the fact that it includes both consciousness and matter in the same ontology thereby inescapably introducing the Problem of Knowledge and its concomitant difficulties into physics, we shall abandon it in favour of possible alternatives which we consider in the next chapter. As is the case with all interpretations which are not demonstrably empirically incorrect, the decision to accept or reject it can be made on the grounds of belief only, and is not supported by logical imperative. We feel that our analysis shows clearly why we should reject this interpretation in favour of one in which the superpositions of macroscopically distinguishable states for macroscopic systems (or nonseparable superpositions which include such terms) do not occur.

Finally, we note that, if we assume that S.R. does not occur, we must arrive at the conclusion that two single-slit patterns appear on the screen, irrespective of whether or not a conscious observer has interacted with the slit-cover assembly. Thus our negative result provides powerful empirical evidence in favour of the existence of S.R. as an objective phenomenon.

Chapter 9

CHANGING QUANTUM MECHANICS9.1 Restricting the Domain of Q.M.

In rejecting Wigner's hypothesis (Chapter 8), we exclude the possibility that the behaviour of microphysical reality be described in terms of Q.M. as it is given by the physical formalism. We must either restrict the domain of Q.M. so that it does not apply to all physical systems (measurement systems in particular), or else, if Q.M. is to be universally applicable, we must introduce changes in order to account for S.R. The former alternative can give rise to objective interpretations of Q.M. subject to certain difficulties. However, we prefer the latter which we consider in §9.5 et seq. As we shall see, these alternatives are not strictly mutually exclusive as they can be viewed as different perspectives on the same physical phenomena.

Our primary complaint against the restriction of the domain of Q.M. is that it introduces a dualism, with all its attendant physical and philosophical problems: i.e. physical systems must be divided into those which are subject to a quantum mechanical description and those which are not. Further, in order to deal with measurement on systems within the domain of Q.M., it will be necessary to formulate a theory which applies to those systems which cannot be described by Q.M. as well as a formula for the interaction between the two kinds of system. As was pointed out in §6.2, this theory cannot be a classical theory or any other theory which is not logically compatible with Q.M. Hence, if we restrict the domain of Q.M., we will have to formulate a *new* theory to describe those systems outside the quantum domain. Since this new theory would only be useful, at the present time, in the description of measurement on quantum systems, it seems that this alternative entails a lot of work with limited purpose. If, on the other hand, we assume that Q.M., as

modified by some changes or additional postulates relating to S.R., is universally applicable to physical systems, the need for such a new theory does not arise.

As in all cases where dualisms occur, when restricting the domain of Q.M. we are faced with the difficulty of formulating an unambiguous criterion for the theory to be used in the description of a given physical system: we must be able to decide, both objectively and unambiguously, to which of the dual aspects of reality a given system belongs. In §9.2 and §9.3 we will be concerned mainly with this problem. We see that it is likely to be soluble, but it is unsolved.

We consider two related ways in which the domain of Q.M. is commonly restricted in order to account for S.R. One is to assert that Q.M. does not apply to *measurement apparatus* or certain parts of such measurement systems. This may or may not be a special case of the second way which is to restrict the domain of application of Q.M. to *microscopic* systems.

9.2 Measurement Interactions

It is often asserted that S.R. happens 'on measurement'. As a statement of one of the properties of physical systems, this is, of course, true (as we have taken pains to demonstrate in the preceding three chapters). In all measurements on a system which is not described by an eigenstate of the operator corresponding to the measured observable, S.R. must occur. However, if this statement is taken to be an explanation of (or a criterion for) S.R., it is equivalent to asserting that Q.M. does not apply to the measurement apparatus, some part of it, or the interaction between the object system and the measurement apparatus. This point-of-view, that S.R. occurs *as a result* of interactions which take place during

measurement is held by many authors. The details of the different interpretations vary widely, but they coincide in the belief that the criterion for S.R. is the occurrence of measurement.

For example, de Witt (1971), in his presentation of the Everett-Wheeler Many Universes Interpretation, asserts that the universe 'branches' whenever a 'measurement-like' interaction occurs. As we pointed out in §7.7, the branching of the universe corresponds, from the local point-of-view of the single consciousness of an observer, to S.R.

Rosenfeld (1965), who claims to support the interpretation of Daneri et al (see §7.4), believes that S.R. occurs as a result of interactions within the measurement apparatus. He writes in apparent contradiction with the theory he supports, that "the reduction of the state has nothing to do with the interaction between this system and the measurement apparatus, in fact, it is related to a process taking place in the latter apparatus after all interaction with the atomic system has ceased". In the absence of any specific postulates to account for S.R., this is equivalent to asserting that Q.M. does not apply to the measurement apparatus since, as we showed in Chapters 5 and 6, the non-deterministic phenomenon of S.R. cannot be accounted for in terms of the deterministic relations of Q.M.

In one of his attempts at a H.V. interpretation, Bohm (1957) accounts for the occurrence of S.R. by the inclusion of non-linear terms in Schrödinger's equation. Since these terms are only assumed to be operative 'during measurement', this theory belongs to the type considered here.

Müller (1974) writes that

"By the help of *measurement-type interactions*, or as a result of them, one of the potentially possible states is realized.... These interactions stabilize or realize one momentary state of the many potentially represented by the latter" (p 65).

Apart from the general conceptual objections to the introduction of a dualism, it is difficult to see why the interactions which occur during measurement should be different, in principle, from any other physical interactions. The measurement apparatus does, after all, consist of physical systems, and the interactions within the measurement apparatus or between the measurement system and the object system should, on the face of it, be nothing other than ordinary physical interactions. These interactions should therefore be describable in terms of Q.M.

If the special character of these interactions is justified on the basis that the measurement apparatus or part thereof is macroscopic, this is equivalent to asserting that Q.M. does not apply to macroscopic systems. This point-of-view is considered in §9.3, 9.4 below. If, on the other hand, interactions which result in S.R. are explained by the fact that they are part of a measurement, we are faced with the problem that it is a matter of human choice whether or not a given chain of suitable interactions constitutes a measurement. This makes the criterion for S.R. (or alternatively the criterion for the dualism: 'measurement type/ non-measurement-type' interaction) subjective and ambiguous . i.e. We cannot achieve an objective criterion for S.R. by assuming that a given interaction results in S.R. *because* it constitutes part of a measurement. Müller (1974) recognizes this difficulty when he states that, while measurement-type interactions are necessary for measurement to occur, "it is nevertheless unjustified to limit the sphere of those interactions which "realize the potential" to *de facto* measurements."

If measurement-type interactions can occur in the absence of *de facto* measurements, we can avoid the problem of subjectivity. However, we now require another criterion for the occurrence of these interactions. Instead of seeking and formulating a criterion for the occurrence of a type of interaction which is not described by Q.M., and which brings about S.R., we find it more satisfying to look for and formulate a criterion for S.R. directly. In this latter case, we can assume that Q.M. applies to all physical systems.

Both of the above programmes, if they achieve their objectives, are capable of yielding an objective interpretation of Q.M. However, we feel that the latter, apart from being more economical conceptually and preserving the universal applicability of Q.M., can provide a more general solution to the problem of measurement: in this case, interactions would be 'measurement-like' if they lead to states which satisfy the criteria for S.R. In any event, it is clear that the occurrence of non-quantum mechanical interactions which give rise to S.R. cannot be explained objectively on the basis that they constitute part or all of a *de facto* measurement process.

9.3 The Restriction of Q.M. to Microscopic Systems

It has been noted by many authors (e.g. Bohr (1935)) that at least some part of every measurement system must be macroscopic. This conclusion appears to be valid despite Wigner's assertion that the well-rested eye can respond to a few photons, since here the eye itself constitutes a macroscopic measurement device. Also, as we showed in Chapter 8, the assumption that the superposition principle can be shown to apply to macroscopically distinguishable states leads to difficulties. These observations, coupled with the empirical fact that we never do experience cats as both alive and dead or pointers in more than a single position at any instant, have lead many physicists (e.g. Ludwig (1971)) to believe that Q.M. does not

apply to macroscopic systems, and that S.R. can be explained in terms of the interaction between microscopic (quantum mechanical) and macroscopic systems. As we pointed out in §6.2 and again in §9.1 above, we cannot describe macroscopic systems in terms of classical theories if they are to be considered as part of the same ontology as microsystems, and indeed, to consist of many interacting microsystems. Hence, this view-point involves us in having to construct a new theory of the behaviour of macroscopic systems which is consistent with their parts being described by Q.M. and in terms of which S.R. can be explained. At the present time, such a theory would have the limited purpose of explaining measurement on quantum mechanical systems. Nevertheless, it may become useful in the future when new macroscopic effects, inexplicable in terms of both classical theories and Q.M., may be discovered. The detailed development of such a theory is beyond the scope of the present work, especially since we shall present an alternative explanation of S.R. in terms of which Q.M. applies to *all* systems and a new theory of macroscopic systems is unnecessary. We confine ourselves here to outlining some of the major difficulties to be encountered in formulating such a theory.

Firstly, we must be able to distinguish, objectively and unambiguously, between micro- and macroscopic systems. While it is clear that a billiard ball, a galvanometer and the human body are macroscopic and an electron, a photon and an atom are microscopic systems, this clarity is achieved only by taking extreme examples. There exists a range of systems whose size, complexity and many other properties vary continuously or nearly continuously from the values associated with one extreme to those associated with the other. It is difficult to see how a point on any such continuum could be fixed unambiguously as a dividing mark between those values indicating that a system is definitely macroscopic and those belonging characteristically to microscopic systems. The distinction implied by the names is one of 'size'. However, we are faced with the difficulty that

some systems which display essentially quantum characteristics are greater in size (dimension, volume, mass) than others which are commonly considered to be macroscopic. A beaker of superfluid, for example, can have greater size than the proverbial macroscopic billiard ball.

We may therefore attempt to distinguish between micro- and macroscopic on the basis of *complexity*. It is generally believed that macroscopic systems, in being composed of many interacting microsystems, must have a more complex internal structure. In order to render this notion sufficiently definite as to achieve an unambiguous criterion, it may be necessary to come to a precise understanding of the notion of elementary or single systems, and then specify the *number* of elementary subsystems which must be present for the combined system to be macroscopic. Although the former requirement would be extremely difficult, if not impossible to fulfill, we can use the general intuitive notion of elementarity to demonstrate a problem that arises in attempting to fulfill the latter.

One atom is clearly a microscopic system within the domain of applicability of Q.M. A two-atom system is likewise microscopic, whether or not the atoms interact. Indeed, it is difficult to see how the inclusion of a single atom (or any other elementary or near elementary microscopic system) could change a microscopic system to one which is macroscopic. It follows by induction that any system composed of finite (or even countable) numbers of microscopic systems must itself be microscopic and describable in terms of Q.M. If the generally accepted notion of atomicity, whereby macroscopic systems are assumed to be composed of many interacting microsystems, is valid, then all systems must be microscopic and Q.M. must be universally applicable! While this conclusion seems a little too far-reaching, it depends on assumptions which look innocent enough.

More generally, there is a procedure in the physical formalism of Q.M. (taking the outer product) which enable us to extend a Q.M. description to include any other system described by Q.M., whether or not these systems interact. The domain of the physical formalism is closed under this inclusion in that, for any two systems describable by Q.M. their combined system is also describable in terms of Q.M. i.e. Unless our ideas of atomicity are incorrect, or Q.M. breaks down within its domain of applicability, Q.M. must be universally applicable to physical systems. This built-in generality of the formalism of Q.M. in effect excludes any restriction of the domain of Q.M., subject to generally accepted views on atomism.

It seems that we have been seeking in the wrong direction for a distinction between micro- and macroscopic systems. The change from micro- to macroscopic systems appears to be one of *quality* which cannot be strictly correlated with *quantity* per se. The preceding arguments support our view that the division of physical reality into macro- and microscopic systems is a subjective distinction made for convenience and for historical reasons, and that it does not represent a dualism in the objective existence of physical systems. Still, it is essential to the interpretation under consideration that such a dualism be 'discovered' or formulated. For the purposes of the remainder of this section, we shall assume that such a dualism is possible, even if it implies some relaxation of our notion of atomism¹.

It may, for instance, be possible to come to an objective distinction between micro- and macroscopic on the basis of Zeh's observation. Zeh (1970) has noted that the spectrum of energy levels for macroscopic (massive) systems is extremely dense. Energy

1. See in this regard §1.5.

transitions as a result of uncontrollable interactions in the environment cannot be avoided in such a system. In terms of Q.M., this means that a macroscopic system (i.e. one with a sufficiently dense energy spectrum) *on its own* cannot be described in terms of Q.M. which could only give a non-separable state for the combined system including the interacting parts of the environment. It seems plausible, therefore, that a distinction between microsystems which can be described individually by Q.M. (under suitable circumstances), and macrosystems, which can *never* be so described, can be achieved in this manner. There are, however, at least two serious difficulties involved in this approach.

Firstly, this criterion does not allow us to identify any particular system on its own as either macro- or microscopic. Before we can apply it, we must know the magnitude of the energy transitions possible as a result of uncontrollable environmental interactions. If we could reduce the level of environmental interaction (by moving into deep space, say) we could ensure that the entire system (including the body and necessary life-support systems of any human observer) would become sufficiently isolated as to be microscopic, and describable in terms of Q.M. In this case, if S.R. is to be explained as a result of the interaction between micro- and macroscopic systems, we would expect no S.R. to occur! While this is not impossible and the consequences with respect to the observer's consciousness are not entirely clear, (needless to say, the experiment has not yet been performed) it is unsatisfying, to say the least.

Secondly, Q.M. does not apply to macroscopic systems in this schema only if we wish to describe them *by themselves*, independently of their environments. This seems, at first sight, to introduce a subjective element, since it is a matter of human choice whether we consider a system together with its environment or by itself. This objection can be overcome, in a sense, by observing that we have no

choice as to how we see macroscopic bodies: we see them primarily as independent entities. Their interaction with other systems (including their environments) is seen (at least in Western cultures) as a secondary characteristic. This shifts the basis of the criterion from a subjective human decision to a fact about our senses and our perception.

In any event, if we suppose that we can find an unambiguous objective distinction between microscopic systems which can be described by Q.M. and macroscopic systems which cannot, there are still some problems to be overcome. The analysis of Chapter 6 no longer applies. By assuming that macroscopic systems cannot be described by Q.M. (in order to account for S.R.) we forfeit the ability to describe the time-development of composite systems involving macroscopic subsystems in terms of a unitary operator or the relations given by the Schrödinger equation, even supposing that a 'state' for a macroscopic system can be written down. Consequently, we cannot *prove*, using this schema, that S.R. must occur under certain conditions. There is no existing theory in terms of which such a proof could be formulated. The problem to be solved is to construct a theory which is conceptually satisfying, logically consistent and which accounts for the empirically verifiable data on S.R.

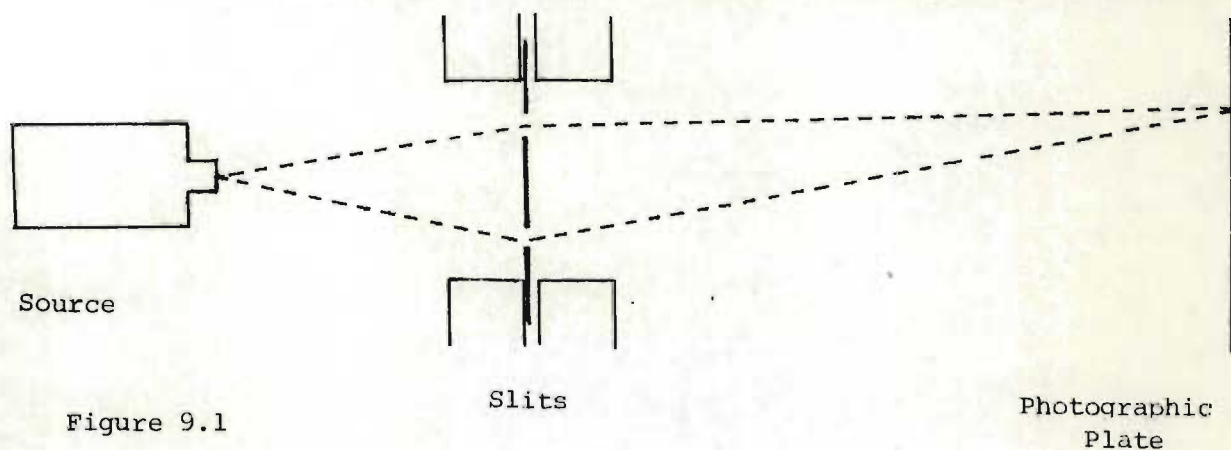
9.4 Towards a Macroscopic Mechanics

It is commonly stated that S.R. occurs when a microscopic system interacts with a macroscopic system. If this is understood as a criterion for S.R., then it is easy to come up with counter-examples. Consider, for instance, the double-slit interference experiment. Here, if S.R. occurred as a result of the interaction of a microscopic system (the interfering system: a photon, electron, etc.) with a macroscopic system (the double-slit diaphragm) then interference effects involving terms relating to different superposed

states for the microsystem would never be observed. Likewise, in the Michelson Interferometer, a microsystem (photon) interacts with macrosystems (the half-silvered mirror, the reflecting mirrors) without resulting in S.R., as is demonstrated by the occurrence of interference fringes. Clearly, the criterion for S.R. must be refined if it is to account for empirical observations.

We can show that interference effects can only be observed if the state of the macrosystem is not *distinguishably* altered by interaction with a microsystem in each of the superposed states.

Consider the double-slit arrangement (illustrated in Figure 9.1) in which photons of wavelength $\sim \lambda$ pass through the slit assembly and are detected on a photographic plate, one at a time. After the passage of each photon, the slit diaphragm (which is free to move in the x direction, perpendicular to the direction of propagation of the incident photons) is repositioned to within a specified precision Δx_H of some value $x = 0$. In the normal course of events, a double slit pattern will eventually form on the plate.



If the slit-spacing is $d \ll D$ and the distance between the slits and the photographic plate is D , the first subsidiary maximum should occur at a distance of $w = \frac{\lambda D}{d}$ from the central maximum at $x = 0$. Now, consider a photon which lands at a point x on the screen. If it traversed the double-slit assembly through slit 1, it would impart a momentum $P_1 = \frac{h}{\lambda} \frac{x + d/2}{D}$ in the x - direction to the diaphragm.

If it passed through slit 2, it would transfer momentum $p_2 = \frac{h}{\lambda} \frac{x - d/2}{D}$

If the difference between these momenta $\Delta p_T = \frac{hd}{\lambda D}$ is greater than the uncertainty in the momentum of the diaphragm predicted by the Heisenberg Uncertainty principle, we could, in principle, detect through which slit the photon had passed by examining the motion of the diaphragm.

i.e. In order to be able to distinguish through which slit the photon passed, we must have the following:

$$\Delta p_T > \Delta p_H$$

$$\text{or} \quad \frac{hd}{\lambda D} = \frac{h}{w} > \frac{h}{\Delta x_H} \quad 9.1$$

$$\text{i.e.} \quad \Delta x_H > w$$

The uncertainty in the initial positioning of the diaphragm must be greater than the separation of the interference fringes on the plates. This means that, even if we consider each photon detection to form part of an interference pattern, each detection event will belong to a different pattern, randomly distributed with a standard deviation of at least the fringe spacing. The net effect is that no interference pattern will be observed.

Similar results have been shown (e.g. by Feynmann (1965) and Bohr (1949)) for other attempts to obtain measurement results indicating different eigenstates while simultaneously observing interference between those states: in each case, the condition that the eigenstates be distinguished is just sufficient for the interference pattern to become undetectable.

1. This calculation is for $|x| \geq \frac{d}{2}$. For $|x| \leq \frac{d}{2}$, similar results follow, giving an identical expression for Δp_T .

In the case where the macroscopic system (the double slit diaphragm) responds in a detectably different way to each of the superposed microstates, no interference effects can be observed. This fits in well with the conclusion of § 8.7 that superpositions of macroscopically distinguishable states cannot be shown to exist. Since the existence of a superposition can only be demonstrated by interference effects, which cannot be observed in this case, we would be safe to assume that S.R. had occurred.

In order to make this idea more generally applicable and transparent, let us suppose that macroscopic systems such as the double-slit diaphragm, although they are not subject to Q.M. in this schema, can be described by a state something like a ket. Let the initial state of the slit diaphragm be $|S_0\rangle$. The state $|S_1\rangle$ ($|S_2\rangle$) corresponds to "having been traversed via slit 1 (2)". Let $|A_0\rangle$ be the initial (quantum mechanical) state for the photon and let $|A_1\rangle$ ($|A_2\rangle$) correspond to the state "having passed through slit 1 (2)". Then, over the period during which interaction with the diaphragm takes place, the combined system may evolve something like this:

$$|S_0\rangle|A_0\rangle \rightarrow C_1|S_1\rangle|A_1\rangle + C_2|S_2\rangle|A_2\rangle + \text{absorbed terms.} \quad 9.2$$

Now, if $|S_1\rangle$ and $|S_2\rangle$ are detectably different, no interference effects will be observed. On the other hand, if we cannot, in principle, detect the difference between $|S_1\rangle$ and $|S_2\rangle$, we can expect to see a double-slit pattern on the plate.

In the latter case, if we can formulate the definition of a macroscopic state so that states which are indistinguishable in principle are identical, i.e. $|S_1\rangle = |S_2\rangle = |S\rangle$, then 9.2 becomes

$$\begin{aligned} |S_0\rangle|A_0\rangle &\rightarrow C_1|S\rangle|A_1\rangle + C_2|S\rangle|A_2\rangle + \text{absorbed terms} \\ &= |S\rangle(C_1|A_1\rangle + C_2|A_2\rangle) + \text{absorbed terms} \end{aligned} \quad 9.3$$

That is, the states of the microsystem and macrosystem become separable, and the state of the microsystem, in particular can be given *by itself* as

$$|\phi\rangle = C_1|A_1\rangle + C_2|A_2\rangle \quad 9.4$$

as is common practice.

If, $|S_1\rangle$ and $|S_2\rangle$ are significantly different, no interference effects will be observed, and we can assume that S.R. has occurred without fear of empirical contradiction. i.e. From 9.2 we can write

$$C_1|S_1\rangle|A_1\rangle + C_2|S_2\rangle|A_2\rangle \rightarrow |S_1\rangle|A_1\rangle \text{ or } |S_2\rangle|A_2\rangle \quad 9.5$$

with relative frequency $|C_1|^2$ and $|C_2|^2$ respectively.

In this case, the final state of the composite system is again separable into macroscopic and microscopic components, but interference is not expected since S.R. has occurred. Note that it is no longer necessary to employ the Heisenberg uncertainty principle to show that no interference pattern will be observed if we can detect 'through which slit each photon passed'. Interference cannot occur, subject to this formalism, because S.R. has removed one of the interfering states.

Clearly, whenever S constitutes a macroscopic measurement device on A, $|S_1\rangle$ and $|S_2\rangle$ must be different in order for S to perform its function. Hence the situation described by 9.5 obtains. When a macroscopic system plays a 'passive' role in the interaction in that it does not respond appreciably to the microsystem, the situation is described by 9.3 and 9.4, and no S.R. occurs. This 'explains' the common practice of describing the microsystem *by itself* when it has interacted with a macrosystem.

In general, we can formulate a reduction postulate as follows:

S.R. occurs if and only if a superposition containing different macroscopic states is formed. After S.R. the macroscopic system is left in exactly one macroscopic state. The microscopic system will be left in a state or superposition of states compatible with this macroscopic state.

We take this opportunity to stress the need for a distinction between microscopic and macroscopic states in this schema. If the above criterion for S.R. were applied to microscopic systems, then the superposition principle could not be valid or useful since all non-identical quantum states are, in principle, detectably different. i.e. If two states are not identical (or equivalent) there must be some experiment which would give different results with certainty, on each of the two states. By distinguishing between micro- and macroscopic and defining what is meant by similarity and dissimilarity for *macroscopic* states, we avoid this difficulty.

In conclusion, we remind the reader that the contents of this section are speculative in that postulates have been made without an analysis to show whether or not they are mutually consistent. The development of this schema depends on the formulation of an unambiguous, objective distinction between micro- and macroscopic. The criterion which we considered has serious defects as we demonstrated in §9.3. Further, the proof of the closure of Q.M. under the combination of systems places the whole concept of the restriction of the domain of applicability of Q.M. in jeopardy. Therefore, having demonstrated how it may be possible to formulate a new macroscopic mechanics, thereby 'solving' the problem of measurement in Q.M., we move on to the alternative which we prefer.

9.5 Spontaneous State Reduction

While an objective interpretation of Q.M. may be obtained by restricting the domain of Q.M., we have shown that this type of approach leads to non-trivial difficulties. These can be avoided by assuming that Q.M., in a modified form, applies to *all* physical systems, including macroscopic systems, measurement apparati and the human body. The modifications we make to Q.M. must account for the occurrence of S.R. and the related fact that superpositions of macroscopically distinguishable states (or non-separable superpositions containing such states) are never observed (see §8.7)

In order to do this, we propose, as did von Neumann (1932) and several others, that the quantum state of a system evolves with time in two distinct ways. Firstly, there is the continuous, deterministic evolution of the state which is formally described by the Schrödinger equation (or the action of a unitary operator). Secondly, there is the discontinuous, non-deterministic change of state which we call S.R. Together, these two processes are suitable for the description of the behaviour of microphysical systems, which must be non-deterministic (See § 5.3). The apparently deterministic behaviour of most macroscopic systems can also be accounted for by noting that, in the limit of large numbers of similarly prepared single systems, every possible outcome of a given measurement will occur, with relative frequencies in proportion to the relative probabilities given by the quantum state for each outcome. This is d'Espagnat's (1971) Principle of 'Statistical Determinism'.

In contrast to the assertion by von Neumann that the position in the measurement chain at which S.R. occurs is arbitrary (his Principle of Psychophysical Parallelism), we note that, since we have assumed that Q.M. is complete (see §5.4) the formal

occurrence of S.R. must correspond to some actual physical phenomenon, which must therefore occur at a definite point in the evolution of a physical system, subject to definite conditions. We therefore propose that S.R. occurs spontaneously whenever certain objective criteria are satisfied. These criteria should be formulated only in terms of the *quantum states* of the systems concerned, since these are supposed to provide a complete description of the physical situation. They should be objective in that they are unambiguous and independent of the knowledge, desires or the point of view of any human observer. In particular, the criteria for S.R. should not depend on which eigenvectors we choose as a basis for the decomposition of the quantum state, or what will be done to the system in the future. Clearly, the conditions for S.R. must be satisfied during the course of any measurement on a system which is not described by an eigenstate of the operator corresponding to the measured observable. However, the occurrence of states which satisfy these criteria should in no way be restricted to de facto measurements. (See §9.2).

Provided that criteria which satisfy the above requirements can be found, this interpretation satisfies the requirements of an objective, realist theory. Physical systems are seen as existing and evolving according to the deterministic relations of the Schrödinger equation. Under certain circumstances, particularly during measurement, a system will evolve, by this means, into a superposition of states which cannot be maintained. When this happens, S.R. occurs spontaneously. Since S.R. is a non-deterministic phenomenon, it is not possible to predict the state in which it will leave a system, even when its state prior to S.R. is completely known. However, the *spectrum* of possible states, describing the system after S.R., together with the probability that each should occur, is determined by the state of the system immediately before S.R. occurs.

Further, the human observer is treated in much the same manner as in classical theories: the human body is treated as an ordinary

(if complicated) physical system, and the phenomenon of consciousness is excluded from the domain of physical reality. The influence of human consciousness on physical systems is limited to those changes brought about by any control it may have over the human body with which it is associated.¹ Since the occurrence of S.R. is seen as independent of any conscious act (in contrast to the consequences of Wigner's hypothesis outlined in §8.6) only *virtual* information is transmitted non-locally in this process. Hence, it is not necessary to admit non-local interactions of the kind which give rise to acausal relations and contradictions with the special theory of relativity. (See §5.2 and §8.6).

We take this opportunity to assert most strenuously that this interpretation is *not* a H.V. theory of any kind. Firstly, the overall evolution of quantum states (and hence, physical systems) is seen as non-deterministic. Secondly, in assuming Q.M. to be complete, we have assumed that the quantum state contains *all* relevant information relating to the system at that time. This excludes the possibility of any additional 'hidden' variables, particularly those which give rise to a deterministic description of physical reality 'at another level'. It is for this reason that we insist that the criteria for S.R. be formulated only in terms of the quantum state of a system: by looking at the quantum state of a system (usually a composite system) we should be able to decide unambiguously whether or not S.R. will occur.

Clearly, the success of this interpretation depends on whether or not suitable criteria for S.R. can be formulated, or, at least, shown to exist. It may be that many different sets of criteria can be found to satisfy our requirements. In this case, we can

1. As with classical theories, there may be a 'scientific' (e.g. behaviourist, microbiological) explanation of any such changes, making any conscious sensations of control and free will illusions from this point of view.

hope that empirical testing or further analysis will support one in favour of the others. However, we only really need one example of a satisfactory set of criteria to justify our interpretation, which can be used to account for the non-deterministic behaviour of microphysical systems as well as the success achieved in their description by the deterministic relations of the physical formalism of Q.M. without restricting the domain of Q.M. or imposing a dual nature onto physical reality.

The empiricist may argue that our arguments have been speculative and metaphysical all along. However strongly we have denied this, at this point in our treatment we must employ assumptions which are clearly speculative and inductive in order to formulate a criterion for S.R. Although our attempts in this direction appear, at the moment, to be successful, we must point out that the viability of our interpretation does not depend on the success of the particular set of criteria we shall formulate. Bearing in mind the speculative nature of our approach, it would be surprising, historically, if one of our first attempts turned out to be satisfactory. As we shall see, our proposed criterion gives rise to an empirical test. Thus, it may be hoped that, as more empirical evidence comes to light, the path to a satisfactory criterion will become clearer, and a less speculative approach will become possible.

9.6 Towards a criterion for State Reduction

What amounts to a criterion for spontaneous S.R. was postulated by Bohm and Aharonov (1957) where they assume that the quantum mechanical description breaks down for non-interacting non-separable systems (e.g. systems which have interacted in the past) which are sufficiently far apart spacially. They propose that the non-separable state decomposes into a statistical ensemble of pure component states, with suitable correlations. If we consider this statistical

ensemble from the point-of-view of 'maximum possible information' this amounts to postulating that S.R. occurs, leaving the composite system in one of the states which were initially superposed. Their description in terms of a statistical ensemble simply emphasises the fact that S.R. is non-deterministic and, in the absence of measurement, we cannot *know* to which state the combined state will reduce. In some ways, this approach is equivalent to taking the *size* of the composite system as a criterion for S.R.

It can be shown (see Ghirardi, Rimini and Weber (1976) and footnote 2 on page 242) that results in contradiction with the predictions of Q.M. should be found in the experiments discussed in §4.5. There we indicated that most of the results obtained empirically favour Q.M. and hence exclude this interpretation. It may however, be argued that in experiments thus far performed, the systems of interest have not been sufficiently separated prior to measurement for S.R. to occur according to this interpretation.

In any event, it is clear that, even if this criterion for S.R. is correct, it cannot be the only one. This criterion cannot account for S.R. when it occurs in measurements where the object system and the measurement apparatus do not become widely separated in space (as when the object system is absorbed). Further, since Q.M. does not give the state of each subsystem on its own, it is not clear that their spacial separation can always be defined purely in terms of the quantum state of the composite system. (This will be so when the quantum state of the composite system is an eigenstate of its position operator).

Another criterion for S.R. can be postulated as follows: it can be shown (see e.g. Bohm (1951)) that, in sufficiently complex interactions, random variations in the relative phase of the components of a superposition must occur. We showed in §9.4 that this must be the case, particularly if a measurement giving results corresponding to one or another of the superposed states is performed. If these

fluctuations are large enough, as they must be in the case of measurement, they ensure that no interference effects can be observed. Even if S.R. does not occur, and each single system in the experiment 'interferes with itself' giving rise to a (potential) interference pattern, each such system, on detection, can only give rise to, say, a single dot on a photographic plate¹. If S.R. is assumed not to occur, each dot can be viewed as a (minute) part of a definite interference pattern. Since the relative phase of the interfering systems varies randomly each time a single system passes through the apparatus and is detected, each dot will form part of a *different* interference pattern which is shifted randomly with respect to all the others. The net effect of this random fluctuation, if it is large enough, is that the interference pattern will be 'washed out': the maxima of some patterns will fall at the minima of others etc. We could therefore postulate that S.R. occurs whenever the relative phase of successive superposed states varies sufficiently for interference effects to be unobservable. Since the existence of a superposition of states can only be demonstrated by allowing the superposed states to interfere and produce observable effects, this postulate cannot be shown to be false by experiment. The non-occurrence of observed interference would then be ascribed to the fact that S.R. had occurred, removing all but one of the interfering states. Further, it follows from a generalization of the gedanken-experiment considered in §9.4 that such a 'washing out' of the interference pattern must occur whenever a measurement which distinguishes the superposed state is performed. Hence, according to this postulate, S.R. must occur in every such measurement, thereby explaining the occurrence of single, unambiguous measurement results. This postulate therefore seems quite

1. S.R. must occur for this to happen but this reduction, from a state containing non-zero terms all over the photographic plate to a state corresponding to a single, discrete detection event, does not concern us directly here.

appealing at first sight. Nevertheless, it contains some serious difficulties which we consider below.

Firstly, it is extremely unacceptable from the falsificationist viewpoint since it is untestable, even in principle. In any case where S.R. is predicted, we would expect not to find interference effects *whether or not S.R. had occurred*. Conversely, when interference effects *are* observed, and, empirically, S.R. cannot have occurred, the relative phase fluctuations *cannot* be sufficiently large for S.R. to be predicted by this postulate. Any test whether or not S.R. has occurred must consist in producing interference (or correlation) effects when S.R. is predicted or demonstrating the absence of such effects when S.R. is supposed not to have occurred. No such tests of this criterion are possible because of its tautological nature. There is a viewpoint from which such a tautological nature indicates the aptness of this criterion. Nevertheless, we would prefer, if possible, the confirmation or rejection of an empirical test.

A second, more serious difficulty relates to the form taken by this criterion. Although it is unambiguous, in that we can determine a definite limit on the magnitude of phase fluctuation that can occur without destroying the interference pattern, it is difficult to imagine how it could be formulated as a general principle involving only the information contained in the quantum state of an *individual*¹ system. The phase fluctuation which features so prominently in this criterion is an accidental or statistical phenomenon which is defined only on an *ensemble* of single system experiments. In an interaction where the fluctuations are just sufficient to 'wash out' any interference effects, many individual systems will be disturbed from the mean value (which, in any case, is undefined for an individual system) by less than the

1. By 'individual' here, we mean the systems involved in a single run of the experiment, culminating in single detection event.

the critical amount. Far from preventing the washing out of interference, these sub-critical fluctuations are essential for it to occur. In contrast, S.R. is meaningful in terms of individual systems in the ensemble. It is therefore difficult to see how this criterion could be applied to individual systems, particularly those with sub-critical phase disturbances¹.

It is possible that a criterion applying to individual systems could be formulated in terms of the conditions which give rise to the washing out of the interference pattern. i.e. The occurrence of sufficiently large phase fluctuations and the occurrence of S.R. could be viewed as common effects of the same cause, which is applicable to single systems. In §9.7, we attempt an analysis along these lines, but we shall see that the resulting criterion does not have identical consequences to the one considered here.

We seek a criterion which can be formulated in terms of single systems and which is, at least in principle, testable. Nevertheless, the fact that the above criterion can be formulated, even in such an *ad hoc* manner, indicates that our concept of spontaneous S.R. may indeed be justifiable, and sheds some light on the way ahead.

9.7 Conditions on Interference²

In §9.4 we used the Heisenberg uncertainty relations to show that we cannot observe interference between quantum states while simultaneously obtaining measurement results corresponding to one or other of the interfering states. In terms of the double-slit interference experiment considered there, this condition is expressed

1. For consistency, we could treat the statistical fluctuation in phase as a 'superposition'. In the same way that probabilities in Q.M. are applicable to single systems, the 'spread' in phase could be a property of a single system. In this case, this objection to this criterion is invalid.
2. Similar considerations are treated in Bedford and Wang (1975).

as a limit on the minimum uncertainty (as given by the Heisenberg Uncertainty Relations) in the momentum of the double-slit diaphragm:

$$\Delta_{P_H} > \frac{h}{\omega} \quad 9.6$$

This condition, while *necessary* for interference to be observed, is not *sufficient*. In the case where ω , the fringe spacing, is greater than d , the distance between the slits, we can have the above condition satisfied with $\frac{h}{d} > \Delta_{P_H} > \frac{h}{\omega}$. The minimum uncertainty in the position of the double-slit diaphragm according to the uncertainty principle, is

$$\Delta_x \sim \frac{h}{\Delta_{P_H}} > d \quad 9.7$$

i.e. The uncertainty in the position of the slits is greater than the slit separation. From the point-of-view from which we derived 9.6, this simply means that the 'patterns' resulting from each photon will be mutually displaced by an amount greater than d . Since $\Delta x < \omega$, the overall pattern should still be observable.

On the other hand, a photon detected at a given point on the screen will have, as possible trajectories, *all* paths passing the diaphragm at $-\Delta x < x < +\Delta x$. Since, the uncertainty in position as given by the Heisenberg relations is a property of the quantum state of the diaphragm, we should argue that this is so, even in the case of a single photon. (i.e. The diaphragm is 'superposed' in position over the range $-\Delta x < x < +\Delta x$). Hence, in this case, we should expect to observe a *single-slit* pattern, corresponding to one emanating from a slit of width $\sim \Delta x$. Thus, for $\omega > d$, the condition for two-slit interference becomes $\Delta x < d$

$$\text{or} \quad \Delta_{P_H} > \frac{h}{d} \quad 9.8$$

Suppose we prepare the position of the diaphragm, before the passage of each photon, to a precision consistent with 9.6 or 9.8 as the case may be. Then, after a time δt , the slits will have moved a distance

$$\delta x \sim \frac{\Delta P_H}{m} \delta t$$

$$\text{i.e. } \delta x > \frac{h}{km} \delta t \text{ where } k = \min(d, \omega) \quad 9.9$$

and m is the mass of the diaphragm. This places a limit on the length of the time interval during which each photon must interact with the double-slit diaphragm since it must do this *before* the 'spread' in its position reaches the critical value of k

$$\text{i.e. } \delta t < \frac{k^2 m}{h} \quad 9.10$$

Now, since $\omega = \frac{D\lambda}{d}$, 9.10 becomes

$$\delta t < \frac{D\lambda m}{h} k' \text{ where } k' = \min\left(\frac{\omega}{d}, \frac{d}{\omega}\right) \quad 9.11$$

This limit on the interval during which the photon must interact with the diaphragm implies that the initial photon state (prepared with a shutter open for δt , say) has a frequency spread of $\delta \nu \sim \frac{1}{\delta t}$. This contributes to the washing out of the pattern. In order for the pattern to be observable, we must have

$$\frac{D}{\lambda} \delta \lambda < \lambda$$

$$\text{or since } \frac{\delta \lambda}{\lambda} = \frac{\delta \nu}{\nu},$$

$$D\delta \nu < \lambda \nu = c$$

$$\text{i.e. } \delta t > \frac{D}{c} \quad 9.12$$

This condition¹, when combined with 9.11 gives a condition on the mass of the double-slit diaphragm which is necessary for interference

1. This condition has not been seen elsewhere by the author. It is interesting in that it is equivalent to a requirement that the 'front end' of the photon state should reach the screen as the rear passes through the double-slit.

to occur. This is

$$m > \frac{h}{\lambda c} k'' \text{ for } k'' = \max\left(\frac{d}{\omega}, \frac{\omega}{d}\right) \quad 9.13$$

We now seek a criterion for S.R. which reproduces, as nearly as possible, the above condition which, if it is not satisfied, implies that no interference pattern will be observed, whether or not the motion of the diaphragm is actually observed, and whether or not the state of the photon is reduced.

We postulate that the 'lifetime' of a superposition of two quantum states, i.e. the time from formation to reduction, is the order of $\Delta t_s \sim \frac{h}{\Delta E}$ where ΔE is the difference in energy between the two states of a single system.

We shall see that this 'reduction postulate' will require some refinement, in order to specify clearly what is meant by ' ΔE ' in more complex situations, as well as to avoid some obvious counterexamples. However, for the meanwhile, we shall consider some of the consequences of this 'loose' formulation.

On encountering the slits, a photon (if it gets through) goes into a continuous superposition of trajectories corresponding to detection events all over the photographic plate, but we consider only those states which give rise to detection at a point x from the central maximum. The momentum transfers to the diaphragm, for the two states of interest, are (for $x \geq \frac{d}{2}$)¹.

$$p_1 = \frac{h}{\lambda} \frac{x + \frac{d}{2}}{D} \quad \text{and} \quad p_2 = \frac{h}{\lambda} \frac{x - \frac{d}{2}}{D} \quad 9.14$$

1. For $x < \frac{d}{2}$ a similar analysis applies, giving an identical expression for ΔE .

For a diaphragm of mass m , the difference in energy transfer for these possibilities is

$$\Delta E = \frac{p_1^2 - p_2^2}{2m} = \frac{h^2}{\lambda_D^2} \frac{2dx}{2m} = \frac{h^2}{\lambda_D m} \frac{x}{\omega} \quad 9.15$$

Therefore, by the reduction postulate, the superposition of the two photon states which, by the conservation of energy, differ in energy by ΔE , will last for a time

$$\Delta t \sim \frac{h}{\Delta E} = \frac{\lambda_D m}{h} \frac{\omega}{x} \quad 9.16$$

In order for interference to be detectable, the superposition must be intact at the time of interaction with the screen. This means that the 'time of flight' or transit time for the photon from the slits to the screen must be less than the 'decay time' of the superposition.

$$\text{i.e.} \quad \delta t_f = \frac{D}{c} < \Delta t_s = \frac{\lambda_D m}{h} \frac{\omega}{x}$$

$$\text{i.e.} \quad m > \frac{h}{\lambda_D} \frac{x}{\omega} \quad 9.17$$

Since we require that the first subsidiary maximum, at least, be visible, we can set $x = \omega$ so that 9.17 becomes

$$m > \frac{h}{\lambda_D} \quad 9.18$$

Comparing 9.18 with 9.13, we find that, as the mass of the double-slit diaphragm is reduced, interference effects will be washed out *before* S.R. occurs, except in the case $\omega = d$ when these conditions coincide. This means that our postulate is empirically correct as regards this experiment: the absence of a pattern when S.R. has not occurred can be explained in terms of phase randomization.

In other experiments, this criterion for reduction can be fulfilled before phase randomization washes out the interference pattern. This indicates the possibility of an experimental test. Indeed, there are some well-known experiments which provide counter-examples to the reduction postulate in its present form.

For example, radio-waves from different sources with different frequencies (and hence, different energies) can and do give rise to beats (interference) at distances much greater than $\Delta t = \frac{c}{\Delta \nu}$. Also, spin-rotation experiments indicate the preservation of coherence, or, equivalently, superposition for time intervals longer than $\frac{h}{\Delta E}$.

Furthermore, the way in which a composite system is viewed as consisting of subsystems is not clearly or objectively defined. ΔE is so loosely defined that our criterion is not even Galilei invariant. We used semi-classical arguments to arrive at the energy difference ΔE , between the photon states, whereas ΔE should be clearly and objectively defined in terms of relevant quantum states only.

We find that the above short-falls can be remedied by a more detailed analysis and specification of the criteria for S.R. Although some of the above analysis will become superfluous, we shall do this without altering the fundamental idea of spontaneous S.R. or the notion that the criterion is somehow related to the energy difference between superposed states.

9.8 The Criterion for Spontaneous State Reduction¹

We introduce our revised criterion in terms of a simple schematic example. Consider two systems A and B, initially non-interacting, and prepared with initial states $|A_0\rangle$ and $|B_0\rangle$ which

1. This work has appeared in Bedford and Wang (1977a).

are eigenstates of the respective Hamiltonians. (This is for simplicity: if they are not in energy eigenstates, their states can be so expanded). Assume that the systems interact by means of a short-range (or effectively short-range) interaction, so that later the sub-systems of what is now a non-separable composite system are again non-interacting. Suppose further (also for simplicity) that the state of the composite system after interaction is a non-separable superposition of energy eigenstates (eigenvectors of the individual sub-system Hamiltonians) so that the evolution of the system over a time interval in which the interaction occurs will be given by the Schrödinger equation and can be written as follows:

$$|A_0\rangle|B_0\rangle \rightarrow \alpha_1|A_1\rangle|B_1\rangle + \alpha_2|A_2\rangle|B_2\rangle \quad 9.19$$

where $H_A|A_1\rangle = E_{A_1}|A_1\rangle$, $H_B|B_2\rangle = E_{B_2}|B_2\rangle$ etc. where

where E_{A_1} , E_{B_2} , etc. are real numbers. $|E_{A_1} - E_{A_2}| = \Delta E_A$ and $|E_{B_1} - E_{B_2}| = \Delta E_B$ where the states and the energies are defined relative to the center of momentum (CM) frame of the composite system A + B. We postulate that once the above conditions of non-separability and non-interaction have been satisfied, the final state of the composite system ($\alpha_1|A_1\rangle|B_1\rangle + \alpha_2|A_2\rangle|B_2\rangle$) will spontaneously reduce after a time of order $\frac{\hbar}{\Delta E_A}$ or $\frac{\hbar}{\Delta E_B}$ (whichever is smaller: they will be equal if energy is to be conserved in single processes) to either $|A_1\rangle|B_1\rangle$ or $|A_2\rangle|B_2\rangle$ with respective probabilities $|\alpha_1|^2$, $|\alpha_2|^2$. The 'lifetime' $\frac{\hbar}{\Delta E}$ of this non-separable state refers to the time interval following the satisfaction of the conditions for S.R.

This reduction of the state of the composite system is a process in C.M. proper time, and the criterion for its occurrence is formulated in terms of objectively defined energies of the individual systems (i.e. as opposed to ensemble properties) in their common C.M. frame. Because the futures of these two sub-systems are inextricably linked through the non-separability of their combined state, the choice of

their common C.M. frame as a basis for the definition of the energies concerned has some objective validity. In general, the choice of a reference frame is arbitrary, and this would make the criterion for S.R. both ambiguous and subjective. However, in the case of non-separable systems, their common C.M. frame is objectively and unambiguously fixed. Conceptually, we might consider such a frame to have 'objective reality' through being defined, in each case, by real, non-separable systems.

We have required that the two sub-systems be effectively non-interacting for S.R. to occur. i.e. The interaction energy, E_I , must be small compared with ΔE . This is necessary in order for the conceptual separation of the composite system into sub-systems to be objectively meaningful¹. If $\Delta E \nlessgtr E_I$, this persistent interaction would make it impossible to distinguish, objectively and unambiguously, distinct sub-systems in the non-separable composite. Hence, according to our postulates, the non-separability would persist, and no S.R. would occur.

Furthermore, ΔE must also be large compared with any energy fluctuations of A and B resulting from environmental perturbation (See §9.4): the energy states of each sub-system of the non-separable composite must be objectively different for the criterion for S.R. to be applicable. This restriction is quite appropriate in the case where B is a measurement apparatus since, as we shall see in §9.9, an apparatus whose sub-systems undergo fluctuations larger than ΔE would either spontaneously 'fire', or would be incapable of measurement on A.

In the case of a system consisting of two sub-systems with

1. Note that the systems A and B considered above in the final state need not have been initially non-interacting. The final state (which satisfies the criterion for S.R.) could have resulted from pair-anihilation, a decay, the interaction of some other systems, etc.

discrete energy spectra, we propose that all of these conditions are necessary, and together they are sufficient, for S.R. to occur as indicated. The criteria for S.R. can therefore be written formally as follows :

- 1). The state must be non-separable.

$$\alpha_1 |A_1\rangle |B_1\rangle + \alpha_2 |A_2\rangle |B_2\rangle \neq |A_3\rangle |B_3\rangle \text{ for any } |A_3\rangle \in \mathcal{H}^{(A)} \\ |B_3\rangle \in \mathcal{H}^{(B)}$$

where A_1 and A_2 are eigenstates of H_A with energy difference ΔE_A etc., defined in the C.M. frame of the composite system.

- 2). The systems must be effectively non-interacting:

$$E_I \ll \max (\Delta E_A, \Delta E_B)$$

- 3). Environmentally induced energy fluctuations of A and B must be such that $\Delta E_{IA} \ll \Delta E_A$ and/or $\Delta E_{IB} \ll \Delta E_B$.

(This does not, however, refer to non-random energy changes resulting from interaction with some background field, which may be responsible for ΔE in the first place).

Once these conditions are satisfied, the non-separable composite state will reduce to $|A_1\rangle |B_1\rangle$ or $|A_2\rangle |B_2\rangle$, with probabilities $|\alpha_1|^2$ and $|\alpha_2|^2$ respectively, after a time interval of the order of $\min(\frac{h}{\Delta E_A}, \frac{h}{\Delta E_B})$ or depending on which ΔE satisfies (3) above. S.R. can be viewed as 'caused' by the energy differences which will be operative if they can be.

In general, the C.M. frame, the time at which the above conditions are first satisfied, the energy differences etc. may themselves be 'superpositions'. However, the values superposed are objective and unambiguous, and hence can be dealt with by this method. The

event of disintegration of a compound system into two sub-systems (as in the experiment considered below) will, in general, be superposed over an interval of the order of the lifetime of the compound system, giving rise to 'spacially extended wave packets'. The reduction into separable energy states of this system will not affect this 'spacial extension', and the final states of A and B will not be position eigenstates.

9.9 Application

In order to clarify the meaning and purpose of the above scheme, we apply these ideas to a number of revealing examples; firstly some in which interference occurs and S.R. is not predicted, and secondly, some cases such as measurement where S.R. is expected.

a). Double-slit Interference

Consider again the familiar double-slit interference experiment. Upon interaction between each photon with the double slit diaphragm S, we have, in obvious notation, the following evolution¹.

$$|S_0\rangle|\psi_0\rangle \rightarrow C_1|S_1\rangle|\psi_1\rangle + C_2|S_2\rangle|\psi_2\rangle \quad 9.20$$

Now, as we showed in §9.7, interference can only be observed if the mass of S is large enough for the difference between its possible final states, $|S_1\rangle$ and $|S_2\rangle$, to be unobservable : when a normal, macroscopic double-slit is used, $|S_1\rangle$ and $|S_2\rangle$ cannot be orthogonal. Indeed, for the usual treatment (in which the state of the double-slit is omitted) to be valid, their inner product $\langle S_1 | S_2 \rangle$ must be close to unity i.e. $|S_1\rangle$ and $|S_2\rangle$ must be essentially the same state for interference to occur².

1. Here, in assuming that the state of the macroscopic slit-cover can be written separably and independently of its environment, we ignore the difficulties raised by Zeh (1970). See §9.4.
2. We are aware of a possible circularity in this argument. See §9.10.

If we therefore assume that $|S_1\rangle = |S_2\rangle = |S\rangle$, 9.20 becomes

$$|S_0\rangle|\psi_0\rangle \rightarrow C_1 |S\rangle|\psi_1\rangle + C_2 |S\rangle|\psi_2\rangle = |S\rangle(C_1|\psi_1\rangle + C_2|\psi_2\rangle) \quad 9.21$$

This state is separable giving the state for each photon and the double-slit *independently* as $C_1|\psi_1\rangle + C_2|\psi_2\rangle$ and $|S\rangle$ respectively. Since the final state in 9.21 is *not* non-separable, our criteria for S.R. are not satisfied, S.R. does not occur, and interference may be expected.

b). Interference between photons of differing frequencies

Consider two radio sources transmitting essentially monochromatic signals which differ in frequency by $\Delta\nu$. According to the first version of the reduction postulate (§9.7) these signals could not be expected to interfere at distances greater than $\frac{c}{\Delta\nu}$ from the nearest transmitted since this would involve the persistence of a superposition of states, differing in energy by $\Delta E = h\Delta\nu$ for longer than $\frac{h}{\Delta E}$. The fact that such interference (in the form of beats) does occur is a counter-example to the original formulation. Here we test the new formulation against this example.

Consider two transmitters, R and S, which are initially described by quantum states $|R_0\rangle$ and $|S_0\rangle$ respectively. If the photon vacuum state is denoted by $|\phi\rangle$, and $|\psi_R\rangle$, $|\psi_S\rangle$ are the states of photons originating from R and S, we can write the evolution of the combined system, over the time interval of interest, as follows:

$$|R_0\rangle|S_0\rangle|\phi\rangle \rightarrow C_1 |R_1\rangle|S_0\rangle|\psi_R\rangle + C_2 |R_0\rangle|S_1\rangle|\psi_S\rangle \quad 9.22$$

where $|R_1\rangle$, $|S_1\rangle$ denote the states of the transmitters when they have emitted a photon.

Now, if the emission of a photon could be detected by examination of the transmitters, no interference would be observed due to phase randomisation (cf. §9.7). Hence, by a similar argument to that in the previous example, we can write $|R_0\rangle = |R_1\rangle$ and $|S_0\rangle = |S_1\rangle$, consequently, 9.22 becomes

$$|R_0\rangle|S_0\rangle|\phi\rangle \rightarrow |R_0\rangle|S_0\rangle(C_1|\psi_R\rangle + C_2|\psi_S\rangle) \quad 9.23$$

This separable state gives the state of the photon *independently* as of those of the transmitters as $C_1|\psi_R\rangle + C_2|\psi_S\rangle$. Since the conditions for S.R. given in §9.8 are not satisfied (the state is separable), and the photon state is a superposition of different frequencies, we expect to observe beats at any distance from the transmitters. The persistence of phase-coherence in spin-rotation experiments, and, as far as we can tell, all other such cases can be treated similarly.

c. Bohm's E.P.R. experiment

We apply our criteria for S.R. to the experiment proposed by Bohm (1951) as an illustration of the E.P.R. 'paradox' which we discussed in Chapters 2 and 5. This is of interest since the experiment is conceptually equivalent to the experiments discussed in §4.5 which test the possibility of local H.V. theories. We show that our interpretation gives rise to predictions which are consistent with the provisional experimental results i.e. that the predictions of ordinary Q.M. are correct. However, we show also that, by making a slight change in the experimental set-up, we can use our criteria to predict results in contradiction with those of the physical formalism of ordinary Q.M., thus indicating the possibility of an experimental test.

Recall that, in Bohm's experiment, a spin-zero system decays into two oppositely directed fermions via a spin-conserving interaction. The quantum state for the two particle system, after decay, is given by

$$|\psi\rangle = 2^{-1/2} (|A_{+Z}\rangle|B_{-Z}\rangle - |A_{-Z}\rangle|B_{+Z}\rangle) \quad 9.24$$

where A and B refer to each fermion and the subscripts +Z and -Z indicate spins parallel and antiparallel with the Z direction, which is arbitrary.

Now the expression on the r.h.s. of 9.24 represents a non-separable state for two systems which have ceased to interact. However, in the usual case, prior to any measurement on the system,

$$\Delta E = |E_{A+Z} - E_{A-Z}| = |E_{B+Z} - E_{B-Z}| = 0 \quad 9.25$$

and S.R. is not expected, according to our criteria.

Thus, we can expect interference between the superposed states to occur, as long as they have a definite phase relationship. In this case, the interference between the superposed two-particle states in 9.24 results in 9.24 having the same form, irrespective of the direction in which the spin-components are considered. i.e. The anti-correlation between the spin-component directions of A and B will be observed independently of the direction in which they are measured.

Suppose that we modify the experiment by allowing the fermions to pass through a *uniform* magnetic field in the Z-direction. Note that this interaction will have the same effect on the motion of the particles irrespective of the direction of their spin components and hence does not constitute what would normally be called a *measurement* of spin-component.¹ Suppose the magnetic field is produced by a large magnet described initially by quantum state $|M\rangle$ (in as far as it can be given independently of its environment). The evolution of 9.24 can now be described as follows:

$$|M\rangle|\psi\rangle \rightarrow 2^{-1/2}(|M_1\rangle|A'_{Z+}\rangle|B'_{Z-}\rangle - |M_2\rangle|A'_{Z-}\rangle|B'_{Z+}\rangle) \quad 9.26$$

1. This is in contrast to the case where the magnetic field is non-uniform, as in a Stern-Gerlach machine, where the trajectories of particles with different spin components in the direction of the magnetic field are separated.

Now, since the magnet is a macroscopic element, we may suppose that the remarks above (relating to the double-slit assembly) apply, and $|M_1\rangle \approx |M_2\rangle$. Thus, 9.26 is separable and the state of the magnet alone is given by $|M_1\rangle = |M_2\rangle$. The state of the two-particle system is also given independently as

$$|\psi'\rangle = 2^{-1/2}(|A'_{Z+}\rangle|B'_{Z-}\rangle - |A'_{Z-}\rangle|B'_{Z+}\rangle) \quad 9.27$$

Now, 9.27 differs from 9.24 in that

$$\Delta E = |E_{A'Z+} - E_{A'Z-}| = |E_{B'Z+} - E_{B'Z-}| = 2\mu B \quad 9.28$$

where μ is the magnetic moment of A and B (assumed equal) and B is the magnetic field.

Therefore, 9.27 satisfies the criteria for S.R. which will take place after a time of the order of $\Delta t \sim \frac{h}{\Delta E} = \frac{h}{2\mu B}$. If the particles are subjected to the magnetic field for this period or longer, S.R. will occur, according to our criteria, and the state of the system given in 9.27 will transform spontaneously to

$$|\psi''\rangle = |A'_{Z+}\rangle|B'_{Z-}\rangle \quad \text{or} \quad |A'_{Z-}\rangle|B'_{Z+}\rangle \quad 9.29$$

with a probability of $\frac{1}{2}$ for either possibility.

The S.R. will not be apparent in subsequent spin-component measurements in the Z-direction because the components will be anti-correlated in either of the possibilities given in 9.29. However, when the spin-components are measured in any other direction, there will be some cases in which they will not be anti-correlated. This effect will be most pronounced for measurements at right angles to the Z-direction (in the x-direction, say).

This can be seen by substituting the following relations into 9.29.

$$\begin{aligned}
 |A_{+Z}\rangle &= 2^{-\frac{1}{2}}(|A_{+X}\rangle + |A_{-X}\rangle) & |B_{+Z}\rangle &= 2^{-\frac{1}{2}}(|B_{+X}\rangle + |B_{-X}\rangle) \\
 |A_{-Z}\rangle &= 2^{-\frac{1}{2}}(|A_{+X}\rangle - |A_{-X}\rangle) & |B_{-Z}\rangle &= 2^{-\frac{1}{2}}(|B_{+X}\rangle - |B_{-X}\rangle)
 \end{aligned}
 \tag{9.30}$$

This gives

$$\begin{aligned}
 |\psi'\rangle &= \frac{1}{2}(|A_{+X}\rangle|B_{+X}\rangle - |A_{-X}\rangle|B_{-X}\rangle + |A_{-X}\rangle|B_{+X}\rangle - |A_{+X}\rangle|B_{-X}\rangle) \\
 \text{or} \quad &\frac{1}{2}(|A_{+X}\rangle|B_{+X}\rangle - |A_{-X}\rangle|B_{-X}\rangle - |A_{-X}\rangle|B_{+X}\rangle + |A_{+X}\rangle|B_{-X}\rangle)
 \end{aligned}
 \tag{9.31}$$

In either of these possibilities, a measurement of spin-component in the x-direction would give results consistent with the system being in one of the four component two-particle eigenstates, with a probability of $\frac{1}{4}$ for each.

This means that, when the experiment is performed repeatedly, the spin-components of the two particles will be found to be parallel in about one half of the cases. This is in contradiction with ordinary Q.M. which predicts that the spin-components of the two particles will *always* be anti-parallel¹.

This experiment, or its equivalent, therefore provides an empirical test of our criteria for S.R., as well as the whole concept of spontaneous S.R.². Unfortunately such an experiment, if it is feasible, is beyond our means and abilities. We can only hope that it will be performed shortly. We note that it is not equivalent to the tests of Q.M. discussed in §4.5 since, in these experiments, there was no suitable energy difference induced prior to measurement.

1. Note that this does not imply a violation of the principle of conservation of angular momentum because the microsystems have interacted with a (massive) macrosystem (the magnet).
2. Any spontaneous S.R. will, in general, give rise to results in disagreement with Q.M. In fact, inequalities analogous to that derived by Bell (1964) have been derived for reduction theories by Fortunato (1976).

Finally, note also that, by varying the period during which the particles interact with the magnetic field and/or the magnetic field strength, it should be possible to test the specific formulation of our criteria for S.R., provided that S.R. occurs at all.

d). Measurement

The fundamental motivation behind the introduction of S.R. in the first place was, of course, to account for the occurrence of single unambiguous measurement results in measurements on single systems which are *not* described by an eigenstate of the operator corresponding to the measured observable. Thus, the 'acid test' of any criteria for S.R. is that they should be fulfilled whenever such measurements occur.

Consider again the double-slit interference experiment. Roughly speaking each incident system (photon or electron, say) must, in some sense, pass through *both* slits if interference effects are to be observed. If, however, we place a detector (e.g. photographic film) close behind each slit, each incident system is detected *either* behind one slit *or* the other. Hence, the superposition of states 'having passed through slit' and 'having passed through slit 2' must be reduced in this second experiment.

As in equation 9.21 above, the state of the incident system, after it has interacted with the double-slit diaphragm (and been transmitted) is given by itself as $|\psi_1\rangle + |\psi_2\rangle$. Let us suppose that detection systems P and Q (grain centres on a photographic plate, say) are placed behind slits 1 and 2 respectively. Then, if the initial states of P and Q are $|P_0\rangle$ and $|Q_0\rangle$,¹ and their excited states (corresponding to a positive reading) are $|P_1\rangle$ and $|Q_1\rangle$, the evolution of the combined system over the time interval during which interaction takes place is given as

1. Here, again, we assume that the states of these systems can be given independently of their environments.

$$|P_0\rangle|Q_0\rangle(C_1|\psi_1\rangle + C_2|\psi_2\rangle) \rightarrow C_1|P_1\rangle|Q_0\rangle|\psi'_1\rangle + C_2|P_0\rangle|Q_1\rangle|\psi'_2\rangle$$

9.32

If the incident systems are photons, $|\psi'_1\rangle = |\psi'_2\rangle = |\phi\rangle$, the photon vacuum. In this case, the state given on the r.h.s. of 9.32 is separable, but the state of the combined detection system, $C_1|P_1\rangle|Q_0\rangle + C_2|P_0\rangle|Q_1\rangle$, is not. If the incident systems are rest-massive, $|\psi'_1\rangle \neq |\psi'_2\rangle$ and the state of the combined object and detection system is non-separable. In either case, provided that P and Q are viable measurement systems, we contend that the non-separable state satisfies our criteria for S.R.

Since our criteria for S.R. are formulated in terms of *pairs* of non-separable systems, we deal with the case of photon interference first. Thereafter we extend our criteria to apply to cases where more systems are involved. In the former case, the state of the measurement apparatus is given as $C_1|P_1\rangle|Q_0\rangle + C_2|P_0\rangle|Q_1\rangle$. The energy difference

$$\Delta E = E_{P_1} - E_{P_0} = h\nu \quad 9.33$$

will be large compared with the environmental fluctuations (gravitational, thermal, etc.) in the energy of P in the C.M. frame of P and Q. If this were not so, P would be useless as a measurement apparatus: the environmental fluctuations would be sufficient to cause P to be triggered spontaneously, giving spurious readings. Furthermore, the interaction between P and Q is much less than ΔE for, say, a visible photon and a gravitational interaction. Once again, if this were not so, we would expect *this* interaction to give rise to spurious results. Thus, according to our criteria, the state of the combined measurement apparatus will reduce to a single term, $|P_1\rangle|Q_0\rangle$ or $|P_0\rangle|Q_1\rangle$, in a time of the order of $\frac{1}{\Delta\nu}$, and with probabilities $|C_1|^2$ and $|C_2|^2$ respectively.

For a visible photon, this implies that S.R. would occur very soon after the measurement interaction and certainly prior to any interaction between the measurement apparatus and a conscious observer.

In the case where the incident systems are rest-massive, the state of the system after interaction is given by

$$c_1 |P_1\rangle |Q_0\rangle |\psi'_1\rangle + c_2 |P_0\rangle |Q_1\rangle |\psi'_2\rangle$$

The same arguments as above apply to the states of P and Q except that here, the energy difference is of the order of the initial kinetic energy of the incident system in the C.M. frame of P and Q. This energy difference will, in general, be greater than that between $|\psi'_1\rangle$ and $|\psi'_2\rangle$ (supposing that these are energy eigenstates). However, we can extend our schema to more than two systems by assuming that the energy differences *induce* S.R. whenever the criteria are fulfilled. Thus, S.R. will occur in a time determined by the greatest energy difference between states satisfying the other criteria. Hence, in a time of the order of $\frac{1}{\Delta E}$, S.R. will occur, yielding a final state for the system which is *either* $|P_1\rangle |Q_0\rangle |\psi_1\rangle$ or $|P_0\rangle |Q_1\rangle |\psi'_2\rangle$.

This schema can clearly be generalized to other measurement situations since any interaction capable of yielding satisfactory (distinguishable) measurement results will result in the system evolving into a state which satisfies the criteria for S.R. i.e. If the measurement systems 'distinguishes' between the states of interest, and it does not give rise to spurious readings as a result of interactions either between its different parts or between it and its environment, then the deterministic evolution of the system as given by ordinary Q.M. (the Schrödinger equation) will result in a non-separable state which satisfies our criteria for S.R.

In contrast, let us consider the case where the detecting system placed far away from the slits, and interference is observed. Now, the superposition of states 'having passed through slit 1' and

'having passed through slit 2' should not be reduced. This follows from our criteria since the measurement apparatus does not distinguish between these superposed states. Any grain of the film can be excited by any system incident on the screen, irrespective of which slit it traversed: the final state of the combined object and detection system does not contain non-separable terms relating to particles passing through either slit. Nevertheless, we expect S.R. to occur when this type of measurement is performed: each incident system arrives at the detection screen in a state containing terms corresponding to all possible positions on the screen. This state should be reduced to one referring to exactly one such position.

Since this measurement *does* distinguish between different positions on the screen (otherwise it would not constitute a satisfactory measurement), the final state given by deterministic quantum mechanical evolution will contain non-separable terms, each one corresponding to the excitation of a different grain-centre. Since the grains are essentially non-interacting with each other or their environment (for the reasons discussed above) and the non-separable states differ in energy (in the sense defined in our criteria) by the photon energy, or the kinetic energy of a rest-massive system, we expect S.R. to occur in a very short time. The state after S.R. will correspond to the excitation of exactly one grain, consistent with the occurrence of an unambiguous measurement result viz. a dot with a definite position on the photographic plate. This final state does not, however, correspond to the passage of the incident system through exactly one slit, but contains terms relating to its passage through *both* slits: the probability that the unreduced state will reduce to a state corresponding with a given position depends on the relative phases of the states 'having passed through slit 1' and 'having passed through slit 2' at that point. Thus, if the experiment is repeated many times under the same conditions (i.e. with identical or equivalent initial states for the incident particles, the double-slit diaphragm, and the detection system) the distribution of dots (excited grains) will form a double-slit interference pattern.

In both the 'Schrödinger's cat' and our 'Interfering Schrödinger's cat',¹ the detection of the triggering system will bring the combined system into a state satisfying the criteria for S.R. Thus, Schrödinger's cat is *either* killed *or* left alive, and the slit-cover in our experiment *either* covers one slit *or* the other, according to this interpretation. i.e. We not only do not expect to observe interference, either between the states 'cat alive' and 'cat dead' or, in our case, between the photons in the secondary interference system, but we can also expect to observe the cat to be either alive or dead or to see one single-slit pattern on the screen in our experiment.

9.10 Criticisms

As far as we can tell, then, our criteria for S.R. are objective and unambiguous. They can be used to predict S.R. in every case where measurement occurs, but not when interference is expected. This is achieved without resorting to a dualist conception of physical reality, but rather by asserting that the *evolution* of physical systems is dualistic: on the one hand, we have the normal, deterministic evolution of a system as given by the Schrödinger equation and on the other, we have the spontaneous non-deterministic occurrence of S.R. whenever the criteria are fulfilled. Q.M., including now the postulates concerning S.R. provides a *complete*² description of a non-deterministic microphysical reality.

We contend that our criteria are subject only to an experimental test of the kind outlined above. However, we are aware of some difficulties which have not proved amenable to a simple solution.

Possibly the least important of these is the fact that our formulation can be seen to imply that the final state of any system after S.R. should be an energy eigenstate: a stationary state. It is clearly not our

1. See Chapter 8 and Bedford and Wang (1976a, 1976b).
2. We use this term in the specific sense outlined in §2.3.

intention to imply that systems cannot be localized at all after S.R.! We note that this difficulty is not trivial in that it has its origins in the application of classical ideas of energy exchange (as, for example, in the analysis in §9.6) to a quantum mechanical situation. However, since measurements do not, in general, distinguish between the different momentum (and hence energy) states which must be superposed to give spacial localization to, say, the measurement apparatus, it may be possible to formulate our criteria for S.R. in terms of the difference between the 'average' energies of the states of interest.

Alternatively, we could extrapolate our criteria thus: if S.R. is predicted for each of the states which must be superposed to form localized 'wave packets' then it will occur for the superposition as well. In any event, we do not consider this difficulty to be particularly damning unless it is a symptom of a graver misinterpretation on our part.

A second and more serious problem is our treatment of large (macroscopic) systems in this interpretation. As we have repeatedly pointed out (having first discussed the problem in §6.5) it is not strictly correct to ascribe a quantum state to a large system, independently of its environment. According to Zeh's (1970) observation, such systems, having extremely dense energy spectra, will interact continually with their environment. Thus, strictly, a macroscopic system can only be described by a non-separable state which includes its environment. Indeed, the problem of non-separability runs very deep, since it relates back to the concepts of atomism, and the even more fundamental notion that systems can be analysed into parts. We have largely ignored the role of non-separability when ascribing states to macroscopic systems. Nevertheless, we also assume that environmental interaction occurs in order to describe macroscopic states which are *indistinguishable* as *identical*. This assumption is not only inconsistent, but may also involve some sort of circularity: we have been unable to discover the significance of the assumption that

indistinguishable macroscopic states are identical and it may be that, although it is in line with common practice, we are in some way assuming what we are setting out to prove.

On the other hand, we feel that there are powerful reasons for assuming that the macroscopic aspects of the universe, at least, can be analysed independently of each other. Our point-of-view, amounts to this: when environmental interactions are negligible from a classical viewpoint, they are also negligible from the quantum mechanical viewpoint, at least in so far as large systems are concerned. We feel that it is more reasonable to view such perturbations as a second-order effect rather than assume, as does Belinfante (1975) that they are the reason that unique unambiguous measurement results occur¹.

A further related problem concerns the division of a given non-separable system into subsystems: some notion of the elementarity of the subsystems involved is necessary for our criterion to be unambiguously applicable. This was clearly indicated by Ballentine (1976) who pointed out that, under certain circumstances, our criteria might lead us to expect S.R. to occur between states describing different degrees of freedom of a single elementary system! We must rely on our intuitive understanding of the concept of a 'single system' to exclude such a possibility.

9.11 Conclusion

Given that the difficulties mentioned above lend themselves to amenable solution (or else can be ignored!) we consider our criteria to be objective and unambiguous². They can be used to predict S.R. in every case where measurement occurs, but not when interference is expected. This is achieved without resorting to a dualistic conception

1. See §6.5.

2. It is our hope and belief that this statement does not reduce to a tautology!

of physical reality, but rather by asserting that the *evolution* of physical systems is dualistic: on the one hand, we have the normal, deterministic evolution of a system as described by the Schrödinger equation or its equivalent, and on the other, we have the spontaneous non-deterministic occurrence of S.R. whenever our criteria are fulfilled. Q.M., including now the postulates concerning spontaneous S.R., provides a *complete* description of a non-deterministic microphysical reality.

Finally, let us dispel a misconception which seems to arise regarding the status of our interpretation: it is *not* a hidden variables theory. This can be seen in two ways. Firstly, since we assume that Q.M. is a *complete* description of reality, that reality cannot be deterministic. This is borne out in our interpretation, in which the evolution of quantum states and physical reality is overtly non-deterministic. Hence our interpretation cannot be a hidden variables theory as defined in Chapter 4. Secondly, we have taken pains to provide criteria for S.R. which relate only to factors which are defined by the explicit form of the quantum states involved; no further variables are postulated: indeed we assume that any specification of a physical situation which is additional to the relevant quantum state (i.e. a 'hidden' variable) is superfluous: Q.M. is complete.

Chapter 10

CONCLUSION

10.1 Summary

Let us suppose that the test of our criteria for S.R. proposed in §9.9 or an equivalent experiment is satisfactorily performed, and the results turn out to be positive: i.e. results are obtained which contradict the predictions of ordinary Q.M., as expected in terms of our interpretation. This will provide powerful evidence in favour of the notion of spontaneous S.R. in general, and our criteria for S.R. in particular. The opposite result, which somehow seems more likely in the face of the general predictive successes of the Q.M. algorithm, will result in our particular criteria being dismissed on empirical grounds. However, it will not exclude the possibility of an interpretation involving spontaneous S.R.

Thus, it seems that we may have found a solution to the problems of interpretation and measurement in Q.M. Physical systems are seen as being completely specified by quantum states. These states, and hence the systems themselves, usually evolve in a deterministic fashion, as described by the Schrödinger equation or its equivalent. Under certain circumstances, a given physical system will evolve into a situation described by a quantum state which satisfies our criteria for S.R. (an S-state, say). An S-state denotes a physical situation in which a superposition of certain states cannot be maintained. This situation is recognizable from the *explicit* form of the state describing the system, so no *hidden* variables need be involved.

Further, a physical system can evolve into the situation described by an S-state without being involved in the affairs of conscious beings, whether by interaction with their associated biological systems or by being involved in what they may choose to call 'measurement' or 'measurement-like' interactions. (See Chapter 8 and §9.2). The connection between measurement and the occurrence of S-states lies in the

fact that interaction between a system not described by an eigenstate of the operator corresponding to the measured observable and a system which can be employed in a successful measurement (see §9.9) of that observable will necessarily result in the combined system evolving into a situation described by an S-state. i.e. Situations described by S-states (and hence S.R.) may occur independently of measurement, but during measurement, an S-state must necessarily occur, if the measurement is to be successful. In such cases, S.R. brings about a transition from a state which contains all possible measurement results to a state which corresponds to exactly one measurement result being obtained.

Also, although this point is not entirely clear (see the discussion in §9.10) it seems that none of the systems involved in the evolution of an S-state need to be distinguished as either macroscopic or microscopic: no dualism in the nature of physical reality needs to be introduced. Indeed, a classical dualism, between 'wave' and 'particle' indicated by the failure to detect an aether in, say, the Michelson-Morley experiment, is resolved in our interpretation of Q.M. Systems are completely described by quantum states, which may be 'wave-like' or 'particle-like' with respect to certain observables, depending on the circumstances.

When a system evolves (deterministically) into an S-state, which is maintained for a time of the order of $\frac{h}{\Delta E}$, S.R. occurs. The system transforms spontaneously into a situation described by one of the superposed terms (of different energy) of the S-state. This transformation is non-deterministic in that it is not possible to predict what the state of the system will be after S.R. We contend that S.R. is an absolutely random transformation, governed only by the probability amplitudes in the S-state. Since there is no way of predicting which of the superposed states will describe the system after S.R., hidden variables constructed for this purpose are superfluous. The information as to which substate will emerge simply *does not exist* prior to S.R. This gives the probability amplitudes which occur in

quantum states an *objective* meaning (see §5.5). Given the S-state, the probabilities for different outcomes of S.R. are objective and absolute: they cannot be altered by obtaining more information since *there is no more information to be obtained!*

The transition described by S.R. is also non-local¹: by this we mean, roughly, that it involves the transmission of information over a space-like interval². However, as we noted in §5.3, this information must be *virtual* in that it cannot be used to transmit intent or, equivalently, a message. This sort of non-local relationship, which we have dubbed relative causality, cannot lead to violations of our concepts of free-will and the certainty of the past, even if the special relativistic concept of the relativity of simultaneity is empirically correct. Consider, for example, Bohm's (1951) illustration of the E.P.R. argument³. When one of the decay products, P say, interacts with a Stern-Gerlach machine oriented in the Z-direction, the combined state of *both* particles and the measurement apparatus evolves into an S-state, and S.R. occurs, resulting in the following transition:

$$2^{-1/2}(|P_{+Z}\rangle|Q_{-Z}\rangle|S_{+}\rangle - |P_{-Z}\rangle|Q_{+Z}\rangle|S_{-}\rangle) \rightarrow$$

$$\text{either } |P_{+Z}\rangle|Q_{-Z}\rangle|S_{+}\rangle \quad \text{or} \quad |P_{-Z}\rangle|Q_{+Z}\rangle|S_{-}\rangle \quad 10.1$$

where $|S_{+}\rangle$, $|S_{-}\rangle$ represent states of the measurement apparatus corresponding to spin parallel and anti-parallel to the Z-direction. This final state is separable, giving the state of particle Q alone as either $|Q_{-Z}\rangle$ or $|Q_{+Z}\rangle$, depending on the result observed at P.

Now, the state of particle Q has changed instantaneously from part of a non-separable state involving *both* $|Q_{+Z}\rangle$ and $|Q_{-Z}\rangle$ to a separable state of *either* $|Q_{+Z}\rangle$ or $|Q_{-Z}\rangle$. i.e. The possibility of finding the spin-component of Q parallel (or anti-parallel, depending on the measurement result at P) with the Z-direction disappears immediately as a result of an interaction which occurs as far away from

1. See §5.2.
2. This term is used in the sense of the special theory of relativity.
3. See §9.9.

Q (in as far as the position of Q can be specified independently, prior to measurement) as we like. The information that S.R. has occurred, and one of the possible measurement results on Q has 'disappeared' is what is transmitted non-locally in this case. A similar situation applies whenever S.R. occurs: if, in the double-slit interference experiment, a 'particle' is found behind one of the slits, the possibility that it will be found behind the other immediately disappears. Note that, in our interpretation, we cannot ascribe this change to a change in our subjective information, since a change in the complete quantum state described by S.R. indicates a change in the actual physical situation.

However, the important point, which makes this sort of non-locality preferable to that involved in hidden variables theories (see §5.3), is that no information in the form of a message or signal can be transmitted by this interaction. Observers performing measurements on P and Q at widely separated positions cannot infer, from local measurements, whether S.R. was brought about by the measurement on P or by the measurement on Q. It is only by finding out the *time ordering* of the measurements that they can determine which one *caused* S.R. to occur.

If the reader is concerned about 'what happens' to the states which cease to exist (in our universe!) after S.R., he or she should consult the Many Universes Interpretation¹ for a possible explanation. This interpretation is in no way incompatible with our 'spontaneous S.R.' interpretation, and, if it seems implausible, it is only because *all* theories dealing with occurrences in universes other than our own (in terms of the past, at least) must be implausible. Our interpretation simply augments their criterion for universe 'branching' or 'splitting'. This is given by the protagonists of Many Universes Interpretation as the occurrence of a 'measurement-like interaction'. We identify universe splitting with S.R., and make the criteria for it (the occurrence of an S-state) objective and unambiguous. Since the states which

1. See §7.7.

disappear after S.R. (i.e. appear in different universes) can never affect our branch again (unless history is ambiguous in fact, as well as in interpretation) we consider the Many Universes Interpretation to be of passing interest only.

10.2 Prognosis

Given the above schema, we feel that there is no need to go to the extremes of rejecting the concepts of realism or materialism (more specifically, what we have called unirealism) as a basis for physics, for all the problems they present, both in general, and in the particular context of the interpretation of Q.M. Furthermore, it is not only unnecessary to discard or neglect the 'second purpose of science' - to *understand* our physical environment - but vitally important that we should retain it.

However, even supposing that the difficulties described in §9.10 can be satisfactorily dealt with, there are yet aspects of our interpretation which we find most unsettling. Two of these sources of disquiet relate to regulative principles which we have adopted, albeit on what we consider to be reasonable grounds, and a third concerns the overall form which our interpretation assumes.

While we accept the conclusion that some classical regulative principles must be discarded in the interpretation of Q.M. (see e.g. §3.5) we still find it difficult to accept the existence of any form of non-local interaction and the implications of non-separability in a Q.M. which is assumed to provide a complete description of physical reality.

In a recent paper¹ we show that, provided the results of more sophisticated experiments like those discussed in §4.5 turn out as expected (i.e. to confirm the predictions of Q.M.), we can choose

1. Bedford and Wang (1977)

between the assumptions that (i) physical processes are absolutely deterministic with no possibility of 'free-will' or (ii) physical processes are non-deterministic and must involve the concomitant non-local transfers of virtual information. Although we prefer the second alternative, we are yet unsettled that such an undesirable feature as non-locality should occur in our interpretation.

Secondly, if Q.M. is assumed to be complete, the existence of non-separable states calls into question our concept of atomism (see §1.5) and, with it, the notion that the universe can be analyzed into parts without undue approximation and oversimplification. One paradigm common to all 'scientific methods' is that *analysis* is a valid method for investigating material of interest. In physics, this amounts to the assumption that physical systems can be described independently of their environments. In accepting the completeness of the quantum mechanical description we fear that we may be rushing headlong onto dangerous ground by invalidating the very methods we have used to come to that acceptance.

Finally, we come to the formal issue, which involves the 'ad hoc' nature of the whole concept of state reduction. It is an amendment to the quantum mechanical formalism which is made with the specific purpose of getting us out of the difficulties into which this formalism has lead us. What we have done or attempted to do here, is to show that this amendment can be formulated in an objective and unambiguous manner. However, this does not really make the process of achieving an explanation and an understanding of physical reality on the basis of the ad hoc 'patching up' of an unacceptable theory any more acceptable. On the other hand, the problems of interpretation of Q.M. relate back to no more complicated considerations than the principle of superposition and the linearity of the evolution in time of quantum states, and we consider that S.R. would be necessary in any realistic description embodying these concepts. Furthermore, it seems that the use of the superposition principle and linear time development are directly related to empirical phenomena: the occurrence of interference in systems which are detected discretely, as particles rather than as waves.

These objections indicate some of the reasons as to why our interpretation is unlikely to gain ascendancy over its rivals, and become accepted. More accurately, since we realise that scientific acceptance is an essentially sociological phenomenon, they indicate the reasons why we ourselves cannot wholeheartedly accept our interpretation and crusade for its acceptance. We advocate instead that more fundamental research be done into achieving a generally acceptable interpretation of Q.M., with the hope that our work may prove to be useful in such research.

To this extent, we feel rather like some self-conscious pre-Galilean mechanist attempting to describe projectile motion in terms of an inadequate theory of forced and natural notions : by adjusting certain concepts and parameters we have been able to formulate a description which, as far as we can tell, is neither logically nor empirically incorrect. Furthermore, it is testable. Nevertheless, our description lacks the conceptual coherence and simplicity which is the hall-mark of a successful and acceptable interpretation. It may take somebody with Galileo's genius for working from first principles and, in view of the controversy associated with this field, for convincing the world that he is correct, to produce such an interpretation. The *simple* solutions which have eluded us may even lie in Bohm's (1971) attempts to formulate a new, non-classical language, or in Ludwig's (1971) to produce a new logic.

"What is Fate?" Nasrudin was asked by a scholar. "An endless succession of intertwined events, each influencing the other."
"That is hardly a satisfactory answer. I believe in cause and effect."
"Very well," said the Mulla, "look at that." He pointed to a procession passing in the street. "That man is being taken to be hanged. Is that because someone gave him a silver piece and enabled him to buy the knife with which he committed the murder; or because someone saw him do it; or because nobody stopped him?"

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