THE INTERPRETATION OF QUANTUM MECHANICS: A CASE-STUDY IN THE SOCIOLOGY OF SCIENCE

by

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DECLARATION

I declare that this thesis has been composed by myself, and that the work reported in it is my own.

William Harvey

Note Some of the findings presented in this thesis have been published. See:

(1)B. Harvey, 'The Effects of Social Context on the Process of Scientific Investigation: Experimental Tests of Quantum Mechanics' in K.D.Knorr, R.Krohn and R.D.Whitley (eds) The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook, Vol 4 (Dordrecht: Reidel, 1980), 139-63.

(2)B. Harvey, 'Plausibility and the Evaluation of Knowledge: A Case-Study of Experimental Quantum Mechanics', Social Studies of Science Vol 11 (1981), 95-130.

Contents

ABSTRACT

The aim of this study was to investigate the role of social processes in scientific investigation, and to relate the products of scientific activity to particular features of the social context in which that activity took place. The group of scientists under examination shared a common interest in the interpretation of Quantum Mechanics. This is not a typical scientific specialty; it lacks both a cohesive social structure and an agreed set of theories and practices. It is argued that social and cognitive factors interact in a complex way to bring about and maintain this fragmented state. In particular, methodological differences are identified as a major cause of disputes in this field. A sub-group of physicists performed experimental tests of Quantum Mechanics. Accounts of the behaviour of these physicists in terms of general codes of scientific conduct (norms) seem to be unsatisfactory; instead, we must refer to many specific features of their local social context. The construction and evaluation of knowledge-claims can also be best described by referring to the cultural context of this work. These conclusions support a relativist view, in which the outcome of scientific activity is not uniquely determined by empirical data, but is flexible, and can reflect social pressures from inside or outside science. However, social and cultural factors may also set limits on this flexibility. The concept of plausibility is introduced as a way of characterizing the role of the social and cultural context in the evaluation of knowledge.

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Chapter One

Introduction

The broad aim of this thesis is to investigate, for a particular area of scientific research, the effect of social processes on the generation, transmission, and reception of scientific knowledge. The topic to be examined is the recent history of attempts to criticise, reinterpret or reformulate the theory of Quantum Mechanics.

In this introductory chapter, I shall outline the main features of my theoretical framework, together with those of alternative theoretical models of science. Where there is conflict, I shall try to show that the sociology of knowledge, as I interpret it, provides a more satisfactory account of the actual processes which make up scientific research. In other chapters of the thesis, I shall draw on elements of my theoretical framework to account for my empirical findings without, for the most part, further defence of the validity of this framework. In my conclusions, I shall return to the question of conflicting theoretical frameworks, and will show how my empirical material clarifies some of the issues. In addition, I will draw on my findings to extend and develop some theoretical concepts.

Having introduced and defended my theoretical framework, the next part of this introductory chapter will consist of a discussion of methodological issues. This will include not only details of how my study was carried out, but also consideration of the limitations of interactive studies such as this one. The final part of this chapter will consist of a brief account of the contents and conclusions of the other chapters of this thesis.

Theoretical Background.

In the last few years, developments have occurred in a number of different disciplines, all of which have important implications for empirical studies of science. In this section, I shall review some of these developments in the philosophy of science, history of science, and sociological theory. In addition, I shall examine a number of empirical studies in which these important theoretical developments have been applied to particular areas of past and contemporary scientific activity.[1]

According to Collins:

"Modern philosophy of science has allowed an extra dimension - time - into descriptions of the nature of scientific knowledge. Theories are now seen as linked to each other, and to observations, not by fixed bonds of logic and correspondence, but by a network, each link of which takes time to be established as consensus emerges and each link of which is potentially revisable - given time."[2]

A number of authors have been responsible for this change in the philosophy of science. Although Popper is often cited in discussions of 'scientific method' as a supporter of the 'hypothetico-deductive method', he does not claim that the application of such a method automatically leads either to an unequivocal solution to problems of theory-choice, or to inevitable improvements in our scientific knowledge. Certainly, he claims that diligent attempts to falsify our current theories provides the best hope of progress, but at the same time he stresses the conjectural, tentative and temporary nature of our current theories.[3]

Lakatos also examined the concept of falsification, and pointed out that scientists always have a choice between developing or rejecting a theory whose predictions have been falsified.[4] It emerges from Lakatos' work that an apparently falsified theory can be 'saved' by developing auxiliary hypotheses which account for the apparent discrepancy between the theory's predictions and empirical findings. It is impossible to know in advance whether such hypotheses will prove fruitful or will turn out to be 'ad hoc'. Lakatos himself chose to adopt an evaluative approach to the history of science, by performing 'rational reconstructions', showing how historical events led up to our present knowledge.[5] Yet his findings emphasise the contingent nature of the processes by which we arrived at our current beliefs, so that it is difficult to justify the special status which Lakatos gives to these beliefs.

Other philosophers and historians, such as Polanyi, Kuhn and Hesse,[6] have stressed the interlinking of scientific concepts, techniques and theories into a self-consistent whole. Testing of hypotheses and evaluation of results involve the use of different parts of the same network. The implication is that the network is never tested against reality in any direct sense; such tests are mediated by theory-laden, and therefore conjectural, concepts and practices.

Taken as a whole, these views suggest that historical studies of science should not be concerned with showing 'where scientists went wrong' in developing theories other than our present ones. Instead, a sympathetic approach to historical contexts is likely to reveal the existence of 'good' science which differs from our own not in terms of rationality but in terms of theoretical presuppositions, technical practices, and goals.

As a methodological prescription for historians of science, this may be unexceptionable. However, this view also has epistemological implications which, for some authors, may be less palatable. According to this view, scientific knowledge no longer has a clear, direct relationship with the natural world. Scientific method is now seen as a set of <u>social</u> processes, and knowledge is generated and evaluated according to socially-determined criteria. In addition, both the knowledge and the criteria can change over time with no assurance that such changes are irreversible. As a result, it becomes more difficult to justify the view that one set of scientific beliefs is unequivocally 'better' than any other set. Wittgenstein's views on the conventional nature of knowledge have been particularly influential in providing support for this relativist position.[7]

Developments within sociological theory have progressed along similar lines. Phenomenologists, such as Berger and Luckmann[8], have examined more general features of social life, such as socialization processes. By demonstrating how an actor's perceptions, meanings, attitudes and roles are structured by his social interactions, these authors provide a useful general model which can be applied to the professional socialization of scientists. This model is, for example, remarkably similar to Kuhn's description of scientific training in which the neophyte is introduced to the world-view generated by the current paradigm.

Ethnomethodology, adopting a similar perspective, has described the processes by which individual actors 'make sense of' the world. Examples studied by ethnomethodologists include microsocial interactions and classification procedures.[9] The results indicate that, at least for 'everyday' or 'common-sense' reasoning, meanings are continually negotiated in the process of social interaction. As well as providing a mechanism for the construction of meanings, ethnomethodology also raises interesting epistemological issues. For example, Garfinkel describes an experiment in which subjects interacted with a 'psychological counsellor' who answered their questions either with 'yes' or 'no'. Although the counsellor's responses were in fact purely random, subjects were able to assimilate these responses and to construct an account of the counselling session in which the advice they received was self-consistent and indeed sensible.[10]

Although ethnomethodologists did not immediately extend their findings to scientific reasoning, some sociologists of science have now done so. Authors such as Woolgar and Latour[11] have concluded that scientific meanings and accounts are generated in very similar ways, and that any particular account produced by scientists has the same epistemological status as any particular account produced by a subject in Garfinkel's counselling experiment. In both cases, empirical data are interpreted, evaluated, reconstructed and ordered according to an internalised set of criteria, with no assurance that the account produced is either unique or more valid than any other possible account.

It is of course possible to take a position of methodological relativism without necessarily accepting the full epistemological implications of relativism. For example, Barnes and Bloor[12] have argued in favour of a symmetrical view of rival belief systems, with no a priori distinction between 'true' and 'false' beliefs, but unlike Collins[13] they have not consistently defended epistemological relativism.

At this point, let us summarize the main features of the theoretical framework described above. Scientific knowledge can be depicted as an interrelated network of concepts, observations, theories and procedures. Phenomena are interpreted and meanings are defined through processes of social interaction and negotiation in which actors draw on the resources of their culture to provide theories, models and analogies. Thus, scientific knowledge is theory-laden, context-dependent, and conjectural. We should adopt a symmetrical approach to both accepted and rejected beliefs in science.

In order to further clarify the implications of the framework, it may be useful to look at another description of science based on a different perspective.

Some authors draw a clear distinction between 'external' and 'internal' influences on the development of science. According to these authors, the intervention of external influences represents a disturbance and distortion of science; 'good' science, it is claimed, proceeds along directions which are determined purely by internal, esoteric or technical considerations.

This distinction is greatly weakened within the new framework. According to this view, scientists operate within a social and cultural context in which a whole range of concepts may be utilised as metaphors and models; in addition, scientists can construct accounts which simultaneously meet contemporary scientific criteria of adequacy while also serving some wider social interest. The latter does not invalidate the former.[14] One need not of course assume that <u>every</u> scientist is consciously concerned with achieving some political end; however, studies such as Shapin's[15] have indicated that social interests can operate even within apparently esoteric technical debates.

Another concept which conflicts with the new framework is that of 'scientific norms', as described by Merton and his supporters. The normative view begins with the observation that scientists' behaviour regularly follows certain patterns, and with the assumption that such patterns serve the function of organising science in such a way as to maximise its efficiency as a social system. For this reason, this view is often described as 'functionalist'.

Usually, supporters of the normative view adopt a specific epistemological position, namely, that the products of scientific activity constitute reliable, objective knowledge. However, it is quite possible to argue that norms serve a regulatory function in a social system without expressing any opinion on the value of the products of that system. (One can, for example, construct a normative account of the social structure of a criminal fraternity.) Thus, an epistemological critique of scientific knowledge does not necessarily weaken the normative view.

There are, however, other difficulties with this view.[16] There is, firstly, the purely empirical problem of identifying the norms.

As research has continued, a bewildering variety of norms and counter-norms has been added to Merton's original list of four. However, this problem is only a manifestation of a more fundamental difficulty. As Wittgenstein, Garfinkel and many others have pointed out, 'the meaning is the use'; that is, no rule can specify completely all possible future situations, so that rules inevitably require interpretation. These interpretive procedures are not formalized, and different actors will inevitably interpret rules in different ways. The meaning and import of the rule will depend on how an actor chooses to define and use that rule. Thus, we can never unproblematically identify a situation in which a rule has been broken; any situation can be interpreted in many different ways.

This 'negotiability' of norms would be bad enough if there was a single well-defined set of norms. Yet we know that a whole range of norms and counter-norms have been proposed. If actors can cite different (conflicting) norms in different (conflicting) ways when they describe a particular action, then it becomes difficult to sustain the view that norms in some way automatically regulate behaviour and sanction deviance.

Becker, in a more general study of deviance[17], arrived at similar conclusions. His 'labelling theory', closely linked with phenomenology, argues that the judicial system does not identify deviants: it creates them. Since the judicial system has the power to enforce its categories on actors in a very direct way, an actor's 'real' status (deviant or innocent) is virtually irrelevant. He has little choice but to accept the identity conferred on him by the courts. Although Becker's conception has been criticised by later ethnomethodologists for still being too absolutist[18], it provides useful insights for a discussion of science.

If scientific norms do not control behaviour, and yet scientists continue to cite norms in their accounts of their own and others' actions (which they do), what then is the role of norms in such accounts? It has been argued, most notably by Mulkay[19], that norms are used to characterize actions rather than to directly constrain them. Scientists do not have access to the powerful sanctions available to the judicial system. Nevertheless, an account of a rival's actions which depicts him as infringing a norm may have some rhetorical utility. Conversely, depicting one's own actions in terms of conformity to a norm may usefully serve a legitimizing function. Of course, given the diversity of norms, and the flexibility of interpretive procedures, any particular action can be described in a number of conflicting ways. For example, a decision to withhold an empirical result may be described as sensible caution or as intolerable secrecy. Thus, according to this view, norms provide a set of resources which can be actively used by scientists to generate particular accounts of actions in order to attribute particular meanings to those actions.

Let us summarize the main features of this alternative view of science, together with the major criticisms of this view. Perhaps <u>the</u> central concept in this view is the distinction between 'good' and 'bad' or 'healthy' and 'pathological' science. As discussed earlier, it is difficult, on philosophical grounds, to draw a clear distinction between successful and unsuccessful theories. However, proponents of the alternative view claim that 'bad' science is still identifiable in terms of scientists' <u>conduct</u>. For example, good science should proceed along lines determined by 'internal' technical criteria; science which is influenced by external social or political interests is condemned. However, historical studies suggest that this internal/external distinction is artificial, and does not represent scientists' own perceptions of their work.

Norms appear to provide another means of discriminating between good and bad science. However, the proliferation of norms, and the fact that norms must be interpreted in order to apply them to any particular situation, makes it difficult to argue that norms actually govern scientists' behaviour.

Thus, these attempts to discriminate between good and bad science have not been wholly successful. This provides further indirect support for the symmetrical approach to scientific beliefs advocated earlier.

In recent years, the views described earlier as the 'new theoretical framework' have been applied with a great deal of success in a number of empirical case-studies. Some have been concerned with the role of 'external' influences of science.[20] Others, perhaps less radical, have examined contemporary scientific specialties[21]. In many cases, such studies have shown that social processes within science (such as informal communication networks and the organisation of research groups) can have important effects on the ways in which scientific knowledge is generated and perceived. A number of recent studies of science have examined the construction of knowledge in contemporary science from a relativist standpoint[22]. Such studies have identified an important role for non-empirical factors in the generation and assessment of knowledge-claims.

Thus, there is already a great deal of empirical evidence in support of the theoretical framework outlined earlier. Hopefully, we have now reached the stage where there is no need to provide a comprehensive defence of these views at the beginning of every empirical case-study. Collins has expressed similar sentiments, in his introduction to a collection of empirical relativist studies:

"Authors feel that every new report must defend the relativist position anew. This collection, it is hoped, in addition to its substantive contribution, will reveal clearly the flourishing empirical programme associated with relativism and thereby obviate the necessity for further defences and re-affirmations."[23]

Certainly, further empirical evidence in support of relativism is not unwelcome, and at various points throughout this thesis I will show how my empirical findings do provide support for this theoretical framework.

However, there are also other aims. As Shapin puts it:

"An empirical sociology of scientific knowledge has to do more than demonstrate the underdetermination of scientific accounts; it has to construct its explanations by showing the historically contingent connections between knowledge and the concerns of various social groupings."[24]

and

"work is often thought to be completed when it can be concluded that 'science is not autonomous' or that 'science is an integral part of the social context'. These are not so much conclusions as starting points for much more explicit analyses of the manners in which social facts relate to scientific knowledge."[25]

Therefore, I will try to construct a comprehensive, self-consistent and empirically accurate account of the structure and development of the particular area of science being studied, in which the products of scientific activity are related explicitly and in detail to various features of the social context.

In addition, and particularly in the final chapter, I shall return to a more general consideration of theoretical issues, and I shall try to develop my theoretical framework in the light of my empirical findings.

Methodology of this Investigation.

This study began in the autumn of 1975 with a broad examination of the literature both on the sociology of science and on the interpretation of Quantum Mechanics (hereafter QM). Given my institutional location, the former was relatively unproblematic. However, access to the technical literature on QM was more difficult since (as I shall discuss later in this thesis) the study of the interpretation of QM is not a recognised specialty in physics. Consequently, there are few reviews, specialised journals or special categories in abstracting services devoted to this field. Papers were therefore initially collected on a 'snowball' basis by following up citations, and by use of the Science Citation Index to locate papers which cited key papers from previous years. Once some general review texts were found, this task became considerably easier. In addition, as my contacts with physicists developed, I began to receive a regular supply of reprints and unpublished material.

About a year after the study began, a questionnaire was constructed and sent to a number of physicists, mathematicians and philosophers. The initial criterion employed to select respondents was that they should have published at least one paper on the interpretation of QM. Initially, names were drawn from my literature survey, although additional names were obtained from early replies to questionnaires. The questionnaire has been included in this thesis as Appendix A.

In all, a total of 168 questionnaires were sent out. Reminders were sent out approximately six weeks later if no reply had been received by that time. Eventually, a total of 107 replies were received. This response rate (66%) was, I feel, artificially lowered for two reasons. First, a number of potential respondents were only known to me through papers published ten or more years previously, and in some cases their address at the present time could not be found.

In such cases, the last known address was used. Thus, either because the questionnaire did not reach them, or because their involvement with QM was long past, a low response rate from such people might well have been expected. Secondly, approximately 30 questionnaires were sent to physicists whose names were obtained from earlier replies on the grounds that they were on the mailing lists for these respondents' reprints. This is clearly a much weaker criterion than that used to select the first batch of recipients. Again, a smaller response rate is to be expected. When such people are excluded, the response rate increases.

A further problem with the questionnaire was the particular social and cognitive structure of this field. As will be discussed in Chapter Four, scientists who study the interpretation of QM have no clearly-defined social identity and no formal institutional structures. In addition, a vast range of topics are studied in this field using many different methodologies. On several occasions I discovered that although I perceived a scientist as someone who worked on these issues, the scientist himself denied this, claiming that his work was in cosmology, philosophy, or some other field.

For all these reasons, it soon became clear that little would be gained by drawing quantitative conclusions from the questionnaire. Its main purposes, then, were threefold. First, it provided me with a great many individual, often anecdotal, accounts of certain features of this activity, and at several points in this thesis I will refer to one or more replies. Second, it brought to my attention a number of people whose names had not appeared in the literature, yet who were to some extent involved in the interpretation of QM. Third, the replies enabled me to identify, and clarify, important features of this field, such as points of dispute, methods of recruitment to this work, and respondents' differing perceptions of the major events in the history of QM. I was then able to follow up these points in greater depth in correspondence and interviews.

Interviews were carried out at three different stages in the project. By a complete coincidence, John Bell, a physicist who is deeply involved in the topic being studied, gave a talk in Edinburgh early in 1976. Through him, I learned that an important conference on experimental tests of QM was to take place in Sicily a few weeks later. I was able to attend this conference and interview many of the participants.

In the first half of 1977, I interviewed most of the rather small number of physicists in Britain who are involved in the interpretation of QM.

In the autumn of 1977, thanks to the generosity of the Science Research Council, I was able to travel widely in Canada and the USA to interview a large number of physicists, mathematicians and philosophers. This field trip was particularly useful in studying experimental work on local hidden variables, and extracts from interviews with experimenters, obtained on this trip as well as in Sicily, form a major part of the evidence to be presented in chapters five and six.

In addition to my own interviews, some physicists agreed to answer questions, from a list supplied by me, put to them by other researchers who were able to contact them while carrying out their own fieldwork abroad. I am grateful to Andy Pickering, who interviewed John Bell, and to Harry Collins and Trevor Pinch, who performed the other interviews in this category.

In both the questionnaire and interviews, respondents were assured that, if they wished, their comments would remain anonymous in any written reports which might emerge from this study. A substantial number of respondents indicated that they had no objections to having their remarks attributed specifically to them. However, the general approach adopted in this thesis is that whenever a comment might be interpreted as 'sensitive' (for example, comments on the work of other scientists) anonymity has been preserved. The original tapes of the interviews, and transcripts, have of course been preserved.

Appendix B lists those individuals who were interviewed in the course of this study. In general, interviews lasted about 45-60 minutes and were tape-recorded. Only one interviewee objected to taping, and notes were taken during this interview.

Finally, I also corresponded with a large number of people, mainly to clarify points which they made in replying to a questionnaire. In a small number of cases, blank tapes and a list of questions were sent to respondents who dictated their replies onto the tape. This technique was used in place of interviews for people who were not directly accessible. In terms of the amount of detail supplied, this proved far superior to written correspondence.

Methodological Problems.

It is obviously vital that any claims made in this thesis should be supported by an adequate amount of empirical evidence. However, it is also necessary that this empirical evidence must itself be reliable and representative. In this section, I shall discuss the problems associated with the assessment of data.

I begin with the assessment of data gained from the published work of other researchers. By providing references in all such cases, the reader is at least at liberty to examine the original sources for himself. When quoting studies of other scientific specialties, I have restricted myself to studies which are generally held to be of an acceptable standard by the majority of workers in this field.

Most of my data on the history and social structure of my own field of study was gained either by personal contact with physicists or by reference to established texts of good reputation. In the remaining cases, I tried wherever possible to obtain 'second opinions' on my sources. For example, in Chapter Four, I shall discuss a case-study by Pinch[26] which deals with the reception of Bohm's work. Bohm himself assured me, in an interview, that Pinch had made no factual errors.[27] In addition, Pinch's study overlaps with my own to a large extent, and I was able to check his references for myself.

There are only two topics in the thesis where I was unable to check the validity of references and the accuracy with which the content of cited material is represented. Both occur in Chapter Three, and deal with the origins of QM in Germany and the reception of QM in the Soviet Union. In both cases, language difficulties were compounded by the inaccessibility of the material cited by my own secondary sources. I have therefore been forced to rely on these sources to a large extent. However, this does not seem to constitute an overwhelming problem. In the case of German QM, my main source is the work of Forman, whose study of this period has attracted a great deal of attention; to the best of my knowledge, the validity of Forman's data (as opposed to his conclusions) has not been questioned.[28] Similarly, in the case of the Soviet Union, my main source is Graham's work[29], and again his empirical findings have been available for a number of years. I also refer to two other authors who have made largely independent studies of Soviet QM.[30]

There is another sort of methodological problem which does not refer to the details of any particular study, but instead applies to any research project which involves gathering data about respondents' attitudes, opinions and reasoning. As discussed earlier in this chapter, it is a central feature of the 'new' sociology of scientific knowledge that scientists' accounts of their actions do not necessarily constitute an objective, comprehensive description of their 'real' motives. Indeed, the whole notion of 'real' motives is brought into question. Scientists' accounts, like those of laymen studied by ethnomethodologists, may invoke a wide range of explanatory structures, which may vary according to the actor's perception of the requirements of the context in which the account is presented.

This problem has been recognised by sociologists of science.[31] For example, Pinch, in a study of solar neutrino scientists[32], found (contrary to his predictions) that scientists who were closely involved with a particular research topic were not in general willing to admit to having doubts and uncertainties about their work. Pinch did not conclude that the theoretical basis for his predictions is incorrect; instead, he argued that these scientists perceived him as a representative of the public, and not as a confidant. (Solar neutrino research had recently been fairly well-publicised, making such a perception plausible.) In other words, the context of the interview, as perceived by the interviewee, influenced the sort of account which was deemed to be appropriate.

However, an alternative interpretation is possible. Perhaps Pinch's theoretical prediction is wrong, and such scientists do not have doubts about the validity of their work! Unfortunately, Pinch does not discuss this possibility or explain why he believes his interviewees took this interpretation of his status.

Gilbert,[33] on the other hand, explicitly recognises the possibility that by (perhaps unconsciously) structuring an interview in a certain way, the interviewer and interviewee produce a rather artificial picture of scientists' motives and perceptions, and that some sociological theories may derive much of their apparent empirical support from such artefacts.

Whereas Pinch claims his interviewees perceived him as a sort of journalist or public investigator, Gilbert notes that in his experience interviewees treated him rather like a student. As a result, he suggests that he

"was treated to a very particular view of the careers of these scientists, one which resulted from their initial conceptions of the roles which they assumed they and I should play during the interview....Topics which were considered to have no place in the conventional picture of science were omitted or mentioned only in passing, just as they would be when teaching students."[34]

To suggest that one particular account is 'distorted' or 'biased' may seem to imply, incorrectly, that a 'true' or 'objective' account can exist. However, although <u>every</u> account is selective, it is important at least to be aware of the ways in which a particular context influences the sort of account produced. Gilbert suggests, albeit tentatively, that models of scientific 'migration' between specialties or problem areas may be based on accounts in which scientists, perceiving and structuring the interview as a teacher - student context, presented the sociologist with a description of their careers as a succession of separate 'problems'. In another context, scientists might well have produced different accounts, in which a desire for promotion, success, or a job in a particular location may appear as the main motivating factor.

In a nutshell, then , if a scientist tells us that the reason he did X was because he thought it would lead to Y, this reason is not necessarily to be taken as a statement of some unique objective truth, but as an account, given in the artificial context of an interview, and no doubt framed within the requirements of that context. It may be interpreted as a legitimation, a rationalisation, or a rewriting of history by hindsight. The same question asked by a colleague, in an informal context, or asked by the original interviewer a few years later, may well yield a different answer. How are we to cope with this problem and justify our interpretations of interview extracts?

One possible solution is simply to concede that there are no 'real' reasons, that any one interpretation is just as valid as any other, and that sociologists are concerned with generating accounts according to their own criteria of reasonableness.[35]

Another possibility is to obtain feedback from interviewees, which will at least help to ensure that the researcher does not consciously or unconsciously select extracts which do not accurately reflect interviewees's opinions on the matter being discussed. To this end, an unpublished paper[36], with an analysis similar to that of Chapters Five and Six, was sent to all the physicists who were quoted within it. Comments were invited, and although few people chose to reply, no-one claimed to have been misrepresented, and no-one claimed that any factual errors had been made, either in my descriptions of the physical principles involved or the events which occurred in the field. Of course, this solution does not fully come to grips with the problem of the physicist's perception of his interaction with the researcher; if the researcher is perceived as a student during the interview, this perception may well persist in later correspondence.

Another possible solution might be to define the interview context in some other way, not in the hope of removing <u>all</u> structuring, but with a view to avoiding the most obvious (and most confining) constructions, such as student - teacher, or scientist - journalist. This approach would presumably mean providing the interviewee with information about what sociologists do, and the sociological hypotheses being tested. The danger is, of course, that either the interview becomes sidetracked into a discussion about sociology (so that the student - teacher relationship is simply reversed!) or the scientist, in a spirit of helpfulness, may actively shape his account to meet the sociologist's expectations. In my own fieldwork, the first possibility did occur on several occasions. It is, by definition, difficult to be sure about the second possibility.

Another way of assessing the validity of accounts is to try, wherever possible, to get a number of points of view on any particular issue. In this way, we may hope that individual idiosyncracies will be identified. To illustrate this point, I shall discuss two examples from my own case-study.

Both concern an experimental test of QM carried out by Richard Holt at Harvard University. Holt's results were in conflict with QM; this was a very surprising result, and Holt spent more than a year checking his apparatus for sources of error. Although none was found, Holt did not conclude that his result was correct. (This episode will be discussed in detail in Chapters Five and Six.)

The only published support for Holt's result came in a series of papers by Paul Werbos of the University of Maryland.[37] Werbos was interviewed on my behalf by Harry Collins, and he reiterated his view that Holt's result was correct. He cited Holt's time-consuming careful checks of his apparatus, and added that, as a student at Harvard, he had observed at first hand the "tremendous psychological pressures" on Holt to encourage him to reject his own result.

In Chapter Five, I shall argue that it was indeed in Holt's best interests to check his apparatus carefully. However, Holt himself denies that he was under any pressure to reject his result prematurely or without justification. Neither Holt, nor either of the other two Harvard physicists to whom I spoke, claimed to have heard of Werbos.

There is other relevant evidence which helps us to resolve this apparent problem of interpretation. It turns out that Werbos works in the Department of Political Science at the University of Maryland. (This information was not provided in Werbos' published papers on Holt's experiment.) He was indeed a student at Harvard, but not a physics major. He is in fact an amateur physicist. (He is also a Rosicrucian, and has published papers on cosmology and physics in the Rosicrucian Digest.[38] These papers contain extremely unorthodox views.) Given a choice between accepting Werbos' account, or of postulating an elaborate cover-up by the Harvard physics department, it is not difficult to decide to give little credence to Werbos' version of events.[39]

The second example concerns an anecdote which was related to me both by Holt and by another physicist, who had heard it from Holt at an earlier date. It again deals with the reception of Holt's result by his colleagues at Harvard. According to the other physicist, Holt was under pressure not to publish his results, and a senior member of the department said to him, "you're not going to publish that, are you?". Holt recites the same story, claiming that this comment was a joke. Holt used this anecdote to illustrate the amusement with which his rather embarrassing result was received.

Other evidence is available to help us choose between these accounts. For example, Holt did not attempt to 'cover-up' his result. An unpublished manuscript was widely circulated among the group of physicists who were involved with these tests of QM. The decision not to publish was not immediate or easy, as we shall see in Chapter Five. In addition, it is difficult to avoid making subjective judgements about interviewees' demeanour. I was impressed by the relaxed, amicable frankness with which Holt discussed his anomalous result.

These examples illustrate the processes by which conflicting accounts can be evaluated. Similar processes of cross-checking and gauging interviewees' credibility are of course advisable in all cases, even when there is no conflict. If our interviewees are unanimous in making a particular claim, we may feel it is a 'safe bet' to accept the claim, but we cannot be sure that this feeling is justified.[40]

There is, then, no complete solution to the problem of interpreting accounts. However, the above discussion at least reminds us of the problems, and perhaps encourages us to approach scientists' statements (and PhD theses!) with a critical awareness. In the last analysis, of course, it is the reader who must judge the plausibility of my own account.

Thesis Structure.

In the remainder of this thesis, I shall apply the theoretical framework described above to the area of scientific activity concerned with the interpretation of QM. This will involved detailed discussion of the similarities and differences between a number of alternative interpretations, and (particularly in Chapters Five and Six) some rather technical details about experimental tests of QM. Some of these details will be presented as and when they are required in the thesis. However, it seems desirable to deal with more general aspects of the physics in a single location. This is

the function of Chapter Two.
Chapter Two will not attempt to provide a comprehensive account of <u>all</u> known interpretations; such a task, as many authors have discovered, is virtually impossible. Instead, I shall simply outline the main features of a few specific interpretations. These will be chosen on two grounds; first, to provide as strong a contrast as possible; second, to provide sufficient information for the reader to follow the technical aspects of the discussion in later chapters. Chapter Two is therefore fairly long. However, many of the philosophical issues in the interpretation of QM are subtle, but also interesting and accessible to the general reader. A full discussion is therefore justifiable.

In Chapters Three, Four, Five and Six I deal with the relationship between physicists' work and their 'social context'. The term 'social context' is rather vague; it may be taken to refer to 'culture' in its widest sense, or the 'scientific community', or (at a smaller level) the 'research group'. Each chapter will deal with social context on a different scale.

Chapter Three is concerned with the relationship between science and its <u>global</u> social context; that is, the entire society, or other subsections of society outwith science. This chapter has two main sections. The first examines the evolution of QM in two rather different cultural contexts: Weimar Germany in the 1920's and the Soviet Union from the 1920's to the 1960's. I will try to show that in both contexts the development and presentation of QM was strongly influenced by pressures originating <u>outside</u> the physics community. While most observers would agree that such influences can and do occur, many would argue that they constitute <u>distortions</u> of the 'normal' scientific processes. Such an argument is difficult to maintain here, since the Weimar Republic produced the <u>accepted</u> theory of QM. Other authors have argued that the Soviet context was particularly harmful to physics. I shall criticise this argument, and conclude that a distinction between 'good' and 'bad' external influences is unhelpful.

The second part of Chapter Three examines what one might call the other side of the coin: namely, the influence of QM on other subcultures. QM has been cited as relevant for such areas as biology, theology and parapsychology. Authors have pointed to features of QM and used them to argue in favour of beliefs in these other contexts. I shall argue that QM's usefulness in such debates is as a rhetorical resource. It does not compel adherence to any particular opinion about parapsychology, theology and so on. A whole range of conflicting accounts can be generated from QM. I shall examine the concepts of 'valid' and 'invalid' uses of QM, and shall try to show that these concepts are not useful.

In Chapter Four I will examine the personnel who study the interpretation of QM, and I will investigate the social relationships between these people. In this chapter, 'social context' refers to the particular scientific subgroup. I will show that this field is not a typical scientific specialty, either in cognitive or in sociological terms. The cognitive and sociological status of this field are not independent; each influences the other.

The concept of methodological differences will emerge from this analysis as a central feature of an adequate description of this field. Such differences help to explain not only the lack of consensus and coherent social structure, but also the way in which disputes are conducted. In order to develop this point, I shall examine an earlier study of a dispute in QM, in which the author adopts a slightly different position from my own. I shall defend my own account, and then apply the concept of methodological differences to another dispute in this field.

The general implication of Chapter Four is that a cognitive dispute in science, which may apparently centre over the status of a theoretical concept or the validity of empirical data, can often be attributed to more fundamental general differences between the rival parties. Different scientists may use a particular term to mean quite different things. Such differences in meaning, arising from differences in usage, can create and perpetuate disputes.

Chapters Five and Six deal amost exclusively with experimental tests of QM carried out by a small group of physicists in the last twelve years. The social context is thus scaled down even further to the 'local' group of people working on a specific set of problems. These two chapters are concerned with the microsociology of interactions between scientists who are, by and large, personally acquainted with each other. The general argument will be that this local context plays a highly significant part in determining the way in which the process of scientific investigation is carried out.

Many authors would concede that 'internal' factors can influence certain features of science, such as the direction of research, the presentation of results, and so on. However, many would claim that such social factors cannot affect the content of science, and this argument would be expressed particularly forcefully when applied (as here) to empirical science.

Although I shall reject this distinction, it does at least provide a convenient way of subdividing my findings. Chapter Five will therefore deal with the behaviour of the physicists involved: their decision to become involved in the experiments, the way they presented their results, the presentation and reception of anomalous results, attitudes to non-empirical work on QM, and so on. In each case, I shall try to show the influence of the local social context. Such influences are widespread and powerful; yet they do not lead to noticeably aberrant or 'unscientific' behaviour. In fact, far from being the cause of distortions of the normal scientific process, I shall argue that such social influences are central features of this process, which must be taken into account when constructing an explanation of this process.

In Chapter Six, I shall examine the content of scientific beliefs. By discussing the validity of assumptions, the epistemological status of anomalous experimental results, and the status of untested hypotheses, I shall argue that scientists' evaluation of such knowledge-claims does not rely solely on generalized procedures or 'scientific method'. Instead, the validity of 'facts' is heavily dependent on 'tacit knowledge' - that is, on the cultural background which physicists bring with them when they tackle a new problem. Not only do physicists find it difficult to articulate this set of beliefs, but even a full articulation would not logically justify the way in which scientists evaluate knowledge-claims. The conceptual framework which is employed in the evaluation of knowledge is empirically adequate rather than uniquely valid.

I shall use the term 'plausibility structure' to refer to this conceptual framework.[41] The plausibility of an idea or procedure is a function not only of empirical evidence but also of the extent to which all observers share the same cultural background or plausibility structure. This has two important implications which I shall investigate. First, the attribution of plausibility is not an unproblematic classification but an active interpretive process. It can be influenced by a number of factors other than empirical evidence; I shall try to demonstrate how the plausibility of one particular hypothesis was altered purely as a result of one physicist's manipulation of that hypothesis, without the intervention of any empirical data. The second implication is that empirical evidence cannot enforce consensus if actors do not share the same plausibility structure; I shall examine several features of the experimental tests of QM about which consensus was not obtained, and I shall relate this to the social location of the actors involved.

The final chapter of this thesis will attempt to draw together the results of the preceding chapters, and to derive some more general conclusions. Many of the findings of this case-study are in close agreement with other empirical studies of science, and provide much supporting evidence for the general theoretical framework outlined earlier in this chapter. The empirical findings of this study will also be used to develop and extend some aspects of this theoretical framework.

Chapter Two

Interpretations of Quantum Mechanics

Introduction

The aim of this chapter is to provide a technical background to the theories, interpretations, and experiments with which the rest of the thesis is concerned. Much of the physics involved is dealt with in standard textbooks and review articles on Quantum Mechanics (hereafter QM) and its interpretation.[1] Accordingly, other bibliographic references in this chapter are mainly restricted to cases where the work of an individual author is particularly relevant, or where the point being made is not dealt with in the standard texts cited in footnote 1.

The major figures involved in the development of QM, such as Bohr, Born, Schrödinger, Heisenberg and Einstein, are well-known. In addition, the majority of this thesis deals with fairly recent critiques of QM in which these physicists played no part. For these reasons, I shall not provide detailed biographical information on such figures. However, I shall discuss a number of physicists, such as Bohm and Bell, who are less well-known, yet who have made major contributions to the interpretation of QM. Biographical information on such authors will be provided in footnotes. Other physicists will be cited in this chapter not because their work is of major importance but because they have written useful reviews, or because they provide good illustrations of general trends. While all such publications are fully referenced, biographical information seems unnecessary in such cases.

In order to grasp the significance of the proposals described in this thesis, it is important that we draw a strong distinction between the mathematical formalism of QM and the interpretation of that formalism. Such a distinction would be virtually meaningless for many branches of 'classical' physics, since most of the symbols which appear in classical physics refer in a well-defined way to real observable quantities.[2] QM can be expressed in terms of symbols, but the identification of symbols with elements of reality, or even with observable quantities, is less clear-cut.

For example, the symbol ψ (the Greek letter psi) occurs in

Schrödinger's formulation of QM, notably in his famous wave equation, which for a single particle (such as an electron in a hydrogen atom) can be written as $\nabla^2\psi + \frac{8\pi^2 m}{h^2}(E-U)\psi = 0$.

ψ is a function (the 'wave function') whose variation in space and time can be well-defined for specified conditions.[3] Nevertheless, according to the orthodox interpretation of QM (the 'Born interpretation of ψ'), ψ does not represent an observable physical quantity. Instead, it refers to the probability that a given observable will have a particular value at a specific point in space and time.

Schrödinger himself did not construct or even accept this interpretation of his wave function. Taking as an example a single electron, Schrödinger's original idea was that ψ represented the spatial distribution of charge at any given time. This led to serious problems of interpretation in situations where ψ had non-zero values over a large region of space (implying that the electron's charge was 'smeared out' over this volume), yet where the electron could be shown experimentally to be localized at essentially a single point. It was Born who proposed that the value of ψ^4 at a point represented the probability that the electron would be found at that point. Schrödinger strongly opposed this interpretation, but was unable to construct a consistent alternative.[5]

Used as an operational rule for generating predictions about the values of observables, the Born interpretation is unexceptionable. What is more, the predictions yielded are remarkably consistent with the results of actual measurements. Nevertheless, many critics of orthodox QM, including Schrödinger, wished to give greater ontological significance to the wave function. They argued that ψ represents a real wave, whereas according to the Born interpretation ψ simply represents our knowledge about the values of observables.
Attitudes towards the wave function suggest that the debate over the interpretation of QM can be characterised in terms of a simplistic but helpful dichotomy. Many supporters of orthodox QM adopted a positivistic or operationalist approach, arguing that any talk of 'reality', which is not rooted firmly in the procedures by which we actually measure things in experiments, is vacuous. To them, the Born interpretation is a procedure which generates accurate empirically - testable predictions, and is therefore quite acceptable. Critics, on

the other hand, argue that the aim of physics is to describe reality, and that the formalism of QM is unsatisfactory because it bears only a very indirect relationship with reality. Most critics agree that the formalism yields accurate predictions, but they reject any operationalist interpretation of this formalism. In general, critics want something more; however, as we shall see, there is little agreement over what this 'something more' should be. The aim of this chapter is to provide a brief, and by no means comprehensive, survey of some proposed interpretations of QM.

Rather than simply provide a list of interpretations, I have tried to impose what I hope is a useful classification system. I have identified a number of aspects of QM which have served as major areas of activity in debates over interpretation. In each case, I shall discuss the implications of orthodox QM, together with several reinterpretations of QM. However, it must be conceded that this classification is an artificial one, and that a few alternatives to orthodox QM, such as hidden-variable theories, have been developed in a number of different directions so that these theories will be discussed under more than one heading. Nevertheless, I would argue that the classification scheme is still a useful way of getting to grips with what often seems a bewilderingly diverse field.[6]

Measurement Theory

The concept of measurement plays an important part in QM. Although every physical theory is concerned with observations and predictions, QM is fairly unique because of the rather strange relationship between the elements of the formalism and observable quantities. As discussed in the introduction, there is no direct connection between ψ for (say) an electron, and the position of that electron. Instead, the values of ψ in different parts of space represent the probability of locating the electron in a particular part of space at that time. Many authors have tried to analyse in detail the relationship betweeen ψ and observables, and a large number of mathematical, physical, and even psycho-physical mechanisms have been postulated in an attempt to justify and explain the undeniable, but rather obscure, link between ψ and real physical systems.

In many cases, conceptual difficulties such as those encountered in the analysis of measurement are best illustrated by reference to specific examples. However, if the point in question is to be made clear, it is often necessary to discuss rather idealized situations. For example, we wish to discuss the behaviour of a single photon of light, while ignoring the technical difficulties of generating and observing single photons. In other words, 'thought-experiments' (Gedankenexperimente) are used. Such thought-experiments have been common in discussions of QM from the time of the early debates between Einstein and Bohr in the 1920's.

As a way of introducing the problem of measurement, consider the following thought-experiment. Light is directed towards a semi-silvered mirror. The metallic backing of the mirror is made sufficiently thick to ensure that exactly half the light reaching it is transmitted, and half is reflected. (Since this is a thought-experiment, we are at liberty to assume that no light is absorbed.) According to QM, light exhibits both wave and particle properties. Rather than saying that the reflected and transmitted light waves have equal intensity, we can just as correctly say that the numbers of reflected and transmitted photons are equal.

If the intensity of light is reduced, we arrive eventually at a situation where only one photon is in flight at any one time. We must then describe the semi-silvered mirror by saying that it assigns equal probabilities to transmission and to reflection. That is, an incident photon has a 50-50 chance of being reflected or of being transmitted. There is no doubt that the individual photon follows only one of these options; if detectors are placed on either side of the mirror, one (and only one) will detect the photon.

However, QM provides us with no way of predicting which way a photon will go. The photon's wave function, ψ , behaves like an ordinary wave and on reaching the mirror it splits up into two components of equal magnitude. Obviously, these components will grow further and further apart as time goes on, but their amplitudes remain equal. According to QM, this is equivalent to saying that no matter how long we delay the detection process, the two outcomes remain equally probable. But this is all that QM provides us with.

This is apparently quite different from the classical treatment of equally probable events, such as the likelihood of getting heads or tails when tossing a coin. Because there are many factors affecting the outcome of any particular toss (for example, the angular velocity of the coin, its mass distribution, air currents) it is very difficult in practice to predict the outcome with certainty. Normally, only the probabilistic prediction of 50% heads and 50% tails is practical, and this prediction will only be accurate for a large number of trials. Nevertheless, according to the classical view it is possible in principle to provide a complete description of all the relevant variables and so to provide an accurate prediction for each individual attempt. What is more, there is no doubt that, in the classical picture, the outcome of an event (heads or tails) is definite and real even if we fail to observe it. In contrast, the QM prediction for the photon is fundamentally probabilistic; we can never predict with certainty the outcome of a single event. In addition, according to the Schrödinger equation, the two halves of the wave function spread apart for an indefinite amount of time unless and until the photon interacts with a detector so that we can definitely assign the photon to one side or the other of the mirror. This point is a subtle one, and a further example may help to clarify it.

Before moving on to this example, it must be conceded that the distinction between a classical and a quantum picture is seldom quite so clear-cut in practice. In the first place, the 'perfect determinism' of classical physics is itself an idealization.[7] In the second place, statistical predictions, such as those provided by QM, are often the only sort of prediction which can be meaningfully tested. For instance, to check the QM prediction for the photon in the example just quoted (that is, 50% chance of transmission and 50% chance of reflection) it is necessary to observe a large number of photons and compare the numbers detected on each side of the mirror. Otherwise, we could not differentiate the QM prediction from other predictions such as 60% transmission and 40% reflection. In a similar way, the only practical way to tell if coins or dice are 'loaded' is by throwing them many times and seeing if all possible outcomes occur with equal frequency. Thus in both classical and quantum models, predicted outcomes for individual events have little relevance.

Nevertheless, a distinction between classical and quantum models can still be drawn, even though the distinction is more philosophical than practical. Another thought-experiment is often used by critics of

QM to highlight this distinction. This experiment was first discussed by Schrödinger, and is commonly referred to as 'Schrödinger's Cat'.

Schrödinger described a 'Höllenmaschine' (which can be translated either as 'infernal machine' or as 'time bomb'), which basically consists of a closed steel chamber containing a cat and an atom of radioactive material. The half-life of the radioactive material is one hour. If the atom decays, and emits radiation, a Geiger counter is activated and this, in turn, via a relay, releases a hammer which breaks a flask of cyanide gas. The cat is then killed by the gas.[8]

When the box is closed, the observer is aware that the radioactive atom has not yet decayed. One hour later, there is a 50% chance that the atom will have decayed and that the cat will have been killed, though the observer cannot know what has happened if the box remains closed. The wave function describing the atom evolves in time, beginning with a certain value (corresponding to an undecayed atom) and thereafter becoming a mixture (technically, a superposition) of two states, $\Psi_{\text{undecayed}}$ and Ψ_{decayed}. The relative magnitudes of these two components change with time, and after exactly one hour their coefficients are equal. In principle, the component $\Psi_{\text{undecayed}}$ becomes smaller and smaller (as it becomes more likely that the atom will have decayed) but the value of its coefficient reaches zero only after an infinite amount of time. As in the case of the photon and the mirror, QM cannot tell us what has actually happened, but only what is likely to happen.

There is nothing to prevent us from constructing a similar description of the apparatus as a whole, including the cat. The obstacles to this in practice are immense, because of the enormous number of interacting atoms in a macroscopic object such as a Geiger counter, let alone a complex living organism such as a cat. However, there is no theoretical limit of this sort to the scope of QM, so that the assumption is valid within a thought-experiment.

Then, at any time, the global wave-function of the box and its contents is a superposition of $\Psi_{\text{undecayed atom, live cat}}$ and $\Psi_{\text{decayed atom, dead cat}}$. According to QM, this superposition is the most detailed description possible, yet we are not accustomed to thinking of cats as being partly alive and partly dead. The difficulty is similar to that of the photon and mirror. However, because Schrödinger's thought-experiment deals with macroscopic objects, the apparently paradoxical nature of this twilight state of being is displayed much more dramatically.

As with the photon, it would be possible to argue that ψ merely yields statistical predictions for a large number of identically-prepared systems. That is, what QM tells us is that if one hundred such boxes were prepared, then on opening the boxes after an hour approximately fifty cats would be dead and fifty would be alive. However, many people are more reluctant to ignore the fate of an individual cat than of an individual photon. Schrödinger's Cat is therefore widely cited as an argument in favour of a more detailed description of <u>individual</u> physical systems.

It must be stressed that a purely operationalist interpretation of QM is perfectly self-consistent. If the formalism is treated purely as a means of generating empirical predictions, which are then tested empirically on a large number of systems, then no paradox exists. One is not obliged to discuss the fate of individual systems, and it is perfectly rational to take the positivist view that the fate of the cat <u>before</u> the box is opened is a meaningless question because it is, by definition, not an empirically-testable issue. Critics of 'orthodox' QM often claim that this positivist view is part of the orthodox interpretation, and it is certainly true that some of the physicists who developed QM made positivist statements. However, there is no clear consensus as to what constitutes orthodoxy.

Niels Bohr is often credited with being the spokesman of orthodoxy. Indeed, many physicists use the term 'the Copenhagen interpretation' as a description of orthodoxy. (Bohr worked in Copenhagen.) Bohr's own views on QM are rather vague, dsspite the profusion of his writings on this subject. Indeed, the generation of coherent (but often conflicting) 'Copenhagen' interpretations from Bohr's written statements is a thriving industry.

Bohr did not try to construct a formal (mathematical) representation of measurement. Instead, he stressed forcefully the logical distinction between microsystems (describable only by QM) and macroscopic systems such as measuring instruments.Bohr claimed that the latter must be described classically. Insofar as this view can be called a measurement theory, it is one which emphasises the transition between micro and macro levels. This provides a solution of sorts to the problem of the 'schizophrenic cat', which seemed to be in a mixture of two different states. Bohr would argue that the paradox arises because we have extended a quantum-mechanical description into an area where such a description is inappropriate.

According to some critics, one problem with this view is that the exact nature of the micro-macro transition is left unexplained. Even Jammer, a fairly neutral commentator, describes the transition as "a somewhat questionable or at least obscure feature of Bohr's conception of quantum-mechanical measurement."[9]

In the area of measurement theory, the use of the term 'Copenhagen Interpretation' as a label for orthodoxy is rather misleading, because when most critics of QM (and indeed a good many textbook authors) refer to the orthodox view they are describing a theory proposed by von Neumann, not by Bohr or his colleagues in Copenhagen. Von Neumann proposed a formal treatment of the transition from an unobserved (mixed) state to an observed (precise or pure) state. In a sense, this proposal constitutes a formalised version, and hence perhaps a justification, of Bohr's more qualitative account. However, some authors would argue that von Neumann's description is a distortion of what Bohr had in mind. Rather than dwell any longer on such disputes, let us examine von Neumann's proposal in more detail.

A major feature of this theory is that the wave function is explicitly attributed to an individual microsystem, and not merely to a statistical ensemble of such systems. For example, in the case of Schrödinger's cat, ψ takes the form of a superposition of two components, ψ_{dead} and ψ_{alive}. When a measurement is made, ψ 'collapses' or 'reduces' to one of these components. The probability that any particular outcome will occur is given by the size of the coefficient of ψ for that outcome. This in turn is affected by the physical circumstances. For example, the probability of the transition $\psi \rightarrow \psi_{dead}$ is 0.5 after one hour, 0.75 after two hours, and so on.

This view, then, claims that ψ is affected by the physical act of measurement. It can certainly be argued that von Neumann's version of 'Copenhagen' is very similar to the (usually unarticulated) viewpoint of the average working physicist. Von Neumann's view makes it possible to think in terms of single photons or atoms being observed individually, and the resulting outcomes being obtained quite naturally in the proportions predicted by QM.

However, by giving a clear formal role to the act of measurement, this approach also raises a number of problems. In particular, it differentiates between measuring processes and all other physical interactions. The behaviour of ψ is normally governed by the Schrödinger equation, in which ψ is defined as a function of space and time, and in which, for any particular physical conditions, the value of ψ at any point evolves in a completely causal manner. There is nothing in the Schrödinger equation which could account for the reduction of ψ from a superposition to a single component. Therefore, von Neumann's proposal involves a new addition to the mathematical formalism of QM; this additional element is known as the projection postulate, since ψ is 'projected' onto a single component. (In an analogous way, the shadow of a three-dimensional object can be projected onto any of a number of different possible planes, giving rise to a variety of two-dimensional images.)

The projection postulate comes into play whenever a measurement is made. But a measurement is a physical interaction, and as such it ought to be describable by means of Schrödinger's equation. For instance, the fate of the cat in the steel box could be recorded by means of a cine camera inside the box. Alternatively, the cat itself may be considered to observe its state (alive or dead) at any moment. But the interactions involved in these observations, whether with a photographic film or with feline brain cells, are presumably physical interactions (albeit very complex ones). They can therefore be incorporated in the Schrödinger equation, at least in principle, so that the 'global' wave function can be thought of as a superposition of

ψ(atom undecayed, film showing live cat, cat feeling alive) and
ψ(atom decayed, film showing dead cat, cat no longer feeling alive).

Thus reduction may not take place at all. For if we use more sophisticated observation techniques, such as heartbeat monitors attached to the cat and leading to a tape recorder outside the box, we are simply extending (and complicating) the chain of physical interactions, and hence enlarging the scope of the global wave function. But as long as these interactions are physical, they could be incorporated into a Schrödinger equation, and the superposition would never be reduced to a single component.

The 'orthodox' response to this apparent problem is to deny that there is a problem. The difficulty, it is argued, stems from trying to think of ψ as a physically real quantity, and from trying to decide what exactly a measurement 'does' to ψ . Instead, according to this view, ψ should be treated as a mathematical tool, so that reduction becomes an operational procedure rather than a physical event. There is then no need to worry about what is 'really happening' to ψ - indeed, the question becomes meaningless. An analogy is provided by the use of complex numbers, which involve $\sqrt{-1}$, usually symbolised as i.[10] i is clearly an imaginary quantity, yet it is very useful as an aid to calculation in a number of fields.

This view of reduction is perfectly self-consistent yet, as stated earlier, it is objectionable to a number of critics who feel that the symbols used in equations should relate in some way to reality. Let us now examine some other responses to the 'measurement problem'.

Some authors, such as Einstein and Ballentine,[11] argue that QM yields only statistical information. (As we have seen, there are many contexts in which the predictions of QM can only be checked by measuring the distribution of outcomes among a large number of events.) This 'Statistical Interpretation' allows its supporters to believe in the real existence of individual microsystems while arguing that QM does not fully describe such systems. ψ provides a (correct) statistical description of an ensemble of such systems, and 'reduction' is merely a way of saying that a single measurement constitutes a selection of <u>one</u> microsystem from this ensemble. This interpretation leaves open the question of whether a more detailed account of each individual system is possible.

Other groups of critics lay more emphasis on the detailed physical events which go to make up a measurement. In other words, such critics accept the fundamental distinction between measurement (where von Neumann's projection postulate comes into play) and other physical processes (where it does not).

For example, Daneri, Loinger and Prosperi (DLP) argue that in the interaction between a microsystem and a macroscopic measuring apparatus, an irreversible change takes place in ψ as a result of the shift in scale from micro to macro[12]. As with many interpretations of QM, there is much disagreement over the mathematical validity of this theory[13], and over whether it is or is not consistent with the Copenhagen interpretation. Since DLP, like Bohr, stress the distinction between micro and macro systems, it has been claimed both by DLP and by Rosenfeld (a former co-worker and staunch supporter of Bohr) that DLP's view is consistent with Bohr's more qualitative theory of measurement. Indeed, since it provides a formal mathematical treatment of the transition, DLP claim that their theory constitutes

"an indispensable completion and a natural crowning of the basic structure of present-day QM."[14]

However, Bub disagrees, claiming that because DLP continue to use QM once they have reached the macro level, they are "basically opposed to Bohr's ideas."[15]

Bohr, according to Bub, argued that micro and macro levels require quite different conceptual frameworks, the former being quantum-mechanical and the latter classical.

DLP's proposal has not gained a great deal of support from other physicists. However, it provides a good illustration both of a particular approach to the measurement problem (examining the transition between micro and macro levels) and of the sort of doctrinal disputes which accompany much of the activity on the interpretation of QM.

Other authors, such as Bohm and Pearle, have suggested that the conflict between the Schrödinger equation and the projection postulate can be resolved by modifying the Schrödinger equation.[16] They claim that by adding a non-linear term to this equation, a superposition will spontaneously (and causally) reduce to a single component on measurement. This additional term is deemed to refer to an underlying set of 'hidden variables' (HVs). Conventional QM provides only an approximate or statistical view by averaging over a whole range of such HVs. Since these theories imply that reduction is a physical process which occurs over a finite (non-zero) period of time, they raise the possibility of experimental tests. If a system could be examined part-way through the process of reduction, for example by performing two measurements separated by a very short time interval, we might observe behaviour which is inconsistent with QM. Such an experiment has been carried out, and the details will be discussed later in this chapter.

Another sort of interpretation also invokes non-linear elements in the Schrödinger equation, but argues that these reflect not physical but psychological processes. It has been claimed that von Neumann himself was sympathetic to this view but its main protagonists have been London and Bauer, and, more recently, Wigner[17]. One of the clearest arguments in favour of this view has become known as the argument of 'Wigner's friend'.

Recall that, with the photon and mirror, and the Schrödinger cat, even a <u>series</u> of detectors, observing the system and each other, would not produce a reduction of ψ because they themselves could be included in the superposition. However, we know that if we ourselves were to look at the system, it would appear to be clearly in one or other of its possible states - for example, the cat would <u>either</u> be dead <u>or</u> alive. According to London and Bauer, human observers have special properties which <u>can</u> cause reduction to occur:

"The observer...has <u>with himself</u> relations of a very peculiar character; he has at his disposal a characteristic and quite familiar faculty which we call the 'faculty of introspection'. For he can immediately give an account of his own state...[and] create for himself his own objectivity."[18]

That is, although we may be able to imagine photons, cats and chains of detectors as inhabiting a shadow world of 'potentialities', it is clear that we ourselves do not. As one reviewer put it, "The buck stops here."[19]

Wigner takes the argument further, claiming that if a friend makes the observation and then reports his findings to me, it is unnatural to suppose that it is <u>my</u> perception of his statement which causes the reduction, and that until I had heard what he said my friend was in a mixture of two states. Thus unless we accept a solipsist position, we must concluded that <u>all</u> conscious observers can reduce wave functions.

Here again, the possibility of an experimental test is raised. For if such psychophysical interactions exist, it is possible that we may be able to detect or control them. As before, I shall postpone discussion of such experiments until later in this chapter.

I shall turn now to the last measurement theory which I wish to discuss. Like the various non-linear theories, this interpretation can be called 'realist', since it takes ψ to represent a real physical entity. However, the reality implied by this theory is a rather strange one. The theory in question is the 'Many Worlds Interpretation' (MWI), first proposed by Everett, and later popularised by DeWitt and Graham[20].

MWI claims to take the formalism of QM at its face value. Since the projection postulate is not part of the original formalism, it is rejected. Therefore, reduction does not occur and hence all the elements in a superposition are equally real; consequently, all possible outcomes of a measurement are equally real. Since we only see one particular outcome for any given measurement, it is concluded that when a measurement is made the universe splits into a number[21] of near-identical branches, one for each possible outcome, and the only difference between these universes is the outcome observed for this particular interaction. We are unaware of other branches (which include copies of ourselves) because QM provides no way of crossing from one branch to another.

Physical interactions are occurring all the time all over the universe, and these are effectively measurements. The rather bizarre conclusion reached is that

"[the] universe is constantly splitting into a stupendous number of branches, all resulting from the measurementlike interactions betweeen its myriads of components. Moreover, every quantum transition taking place on every star, in every galaxy, in every remote corner of the universe is splitting our local world on earth into myriads of copies of itself."[22]

This completes my brief survey of measurement theories. In Chapter Four, I shall discuss the construction and reception of such interpretations in more detail. The purpose of the present chapter is simply to provide some technical background for this discussion. Table 1 provides a summary of the central distinguishing features of the various interpretations dealt with here. I must stress that I have dealt with only a few of the many versions of measurement theory which have been proposed. However, those selected are representative of the main theoretical and philosophical divisions within the field, and provide a useful basis for characterising and

Table 1

Interpretation	Status of wave-function ψ	Status of reduction of ψ
Bohr ('Copenhagen')	A mathematical tool which accurately describes microsystems. Macro-systems require classical concepts and formalism	A qualitative (and perhaps undesirable) way of referring to the conceptual division between microsystems and macrosystems
Von Neumann ('Copenhagen'?)	A mathematical tool which accurately describes microsystems and can, in principle, also describe macrosystems	A mathematical description, possibly with physical correlate, of the transition between micro and macro systems, and of the corresponding change which occurs in ψ
Bohm-Bub, Pearle, etc ('Hidden Variables')	ψ provides a statistical approximation of the equilibrium values of the HVs. HVs describe individual physical microsystems	A real physical process of finite duration. Results from new non-linear elements in the (causal) Schrodinger equation
London-Bauer, Wigner ('Consciousness')	ψ is real but does not provide a full account of the psychological state of human observers	A real physical process brought about by pscho-physical interactions, probably located within the central nervous system of human observers
Ballentine, Einstein? ('Statistical')	ψ provides a statistical description of ensembles of microsystems; an individualistic description of microsystems would require HVs, but these may not exist	Not a physical process, merely a way of expressing the fact that a measurement on an ensemble of systems which yields a certain value constitutes a selection from an ensemble whose systems have a whole range of values for the observed parameter

analysing an otherwise very complex scene.

Any attempt to provide a complete account of measurement in QM would require at least a separate thesis. Jammer, in an encyclopaedic work on the interpretation of QM, devotes an entire chapter of fifty pages to measurement theory, and concludes by noting

"the immense diversity of opinions, and the endless variety of theories concerning quantum measurements"

and he lists 24 authors "to name only a few" whose theories could not be discussed due to lack of space[23].

The Incompleteness of QM: (1)The Heisenberg Uncertainty Principle.

In this section, I shall turn to another area of disagreement within QM. We have already seen how critics claim that QM provides an inadequate description of measurement. Here we deal with QM's limitations on the results of experiments; this is most easily summarised by referring to the Heisenberg Uncertainty Principle (HUP)[24]. In a sense, this is a more severe limitation on our knowledge. Measurement theories are concerned with the description of changes in ψ during measurement but most such theories accept that the results of measurements are correctly predicted by QM. HUP tells us that we can only gain a limited knowledge about the properties of physical systems no matter how we choose to describe the process of measurement.

To be more specific, HUP claims that the values of pairs of certain physical observables cannot both be simultaneously known with complete precision. Pairs of observables to which this restriction applies are known as incompatible. The product of the minimum uncertainties of such pairs is of the order of Planck's constant, a small but non-zero number. To quote some examples, for position and momentum in a given direction, $\Delta p \, \Delta x \gtrsim h$

and for energy and time $\Delta E \, \Delta t \gtrsim h$.

(In these equations, ΔE refers to the uncertainty in the value of E, and so on; h is Planck's constant, which has the value 6×10^{-34}Js.)

We can discover an electron's position or momentum to as perfect a degree of precision as we wish, but we cannot know both simultaneously without introducing an uncertainty in our measured values. This represents a serious withdrawal from the classical ideal, in which precision is limited only by the accuracy of our instruments, and infinite precision is a meaningful, if idealized, goal. According to QM, this goal is only meaningful in certain cases such as single measurements.

Heisenberg produced a thought-experiment, the gamma-ray microscope[25], which provides a good illustration of the uncertainty principle. This microscope is used to find the position of an electron. The resolution possible in any microscope is limited by the wavelength of light which is used. Gamma rays have a very small wavelength, and by using high-energy gammas, the wavelength can be reduced without limit, since (according to QM) energy is inversely proportional to wavelength. Thus the electron's position can be obtained with arbitrary precision.

However, in order for the observer to 'see' the electron, gamma rays must strike it and be deflected into the microscope's lens system. On colliding with an electron, an unpredictable amount of momentum will be transferred from the gamma ray to the electron. The size of this discontinuous momentum change is limited by the energy of the gamma ray. Thus, as the position of the electron is specified more precisely (by increasing the energy of the gamma ray) the momentum of the electron becomes more uncertain. Hence HUP is seen to hold in this thought-experiment.

Heisenberg himself adopted an operationalist approach to measurement. He argued that we can only discuss or describe those things which we can actually measure; our measurements are necessarily imprecise, hence our description of microsystems is also imprecise. This viewpoint rejects the notion that we can describe microsystems at times when we are not observing them. As a result, it avoids the very difficult problems which arise if we do accept the 'realist' notion. For a realist is forced to wonder whether the 'fuzziness' of observables follows simply from the physical facts of any measurement process, or whether it represents a more fundamental 'fuzziness' in nature. In other words, even if it is impossible to obtain simultaneous precise values of the position and momentum of an electron, must we conclude that the electron does not simultaneously possess a well-defined position and momentum at each moment in time?

In fact, according to QM, we are not even permitted the luxury of imagining that these quantities are well-defined, even if we accepted that their values are unknowable with complete precision. Not only do positivist supporters of QM reject this whole line of thinking as meaningless, but the physical implications of the theory raise serious doubts about the validity of such imaginings. The best way to illustrate this is by reference to another thought-experiment, the two-slit interference experiment.

The Incompleteness of QM: (2)The Two-Slit Interference Experiment.

According to classical wave theory, if a coherent set of waves is diffracted by a pair of slits as shown in Figure 1, an interference pattern is set up which (in the case of light waves) appears as a set of alternating bright and dark bands on a screen. These bands represent constructive and destructive interference[26].

The classical explanation of this phenomenon is that a part of each wave front passes through each slit, and the two parts recombine at the screen. Phase differences between the two components cause the interference pattern.

A second classical example consists of a gun firing bullets at a pair of slits (Figure 2). The bullets which pass through the slits are embedded in a target and their distribution is plotted as shown. No interference pattern is seen. Each bullet which arrives at the target has either passed through the top slit or the bottom slit.

Now consider the results of firing electrons or light at a two-slit system, taking into account the implications of QM. According to QM, electrons have wave-like properties and hence can exhibit diffraction and interference. Conversely, light, which we normally think of as a wave, is emitted and absorbed only in discrete quanta (photons), and these photons can be thought of as particles of light. Thus neither of the above classical models is wholly appropriate for such microsystems.

QM predicts (and experiments confirm) that if both slits are open an interference pattern is obtained, while if only one slit is open a 'bullet-type' distribution is produced (Figure 3). What is more, these predictions continue to hold even when the frequency of 'firing' is reduced to such an extent that only one electron or photon is in flight at any one time. (in this case, the individual

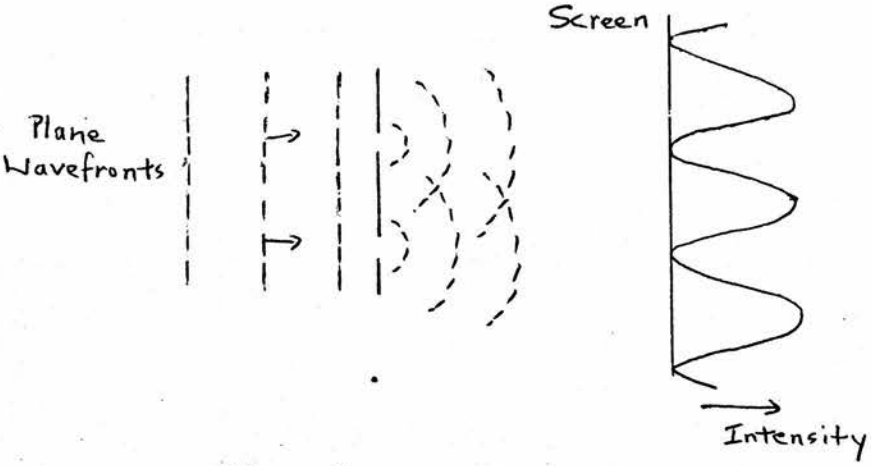


Figure 1

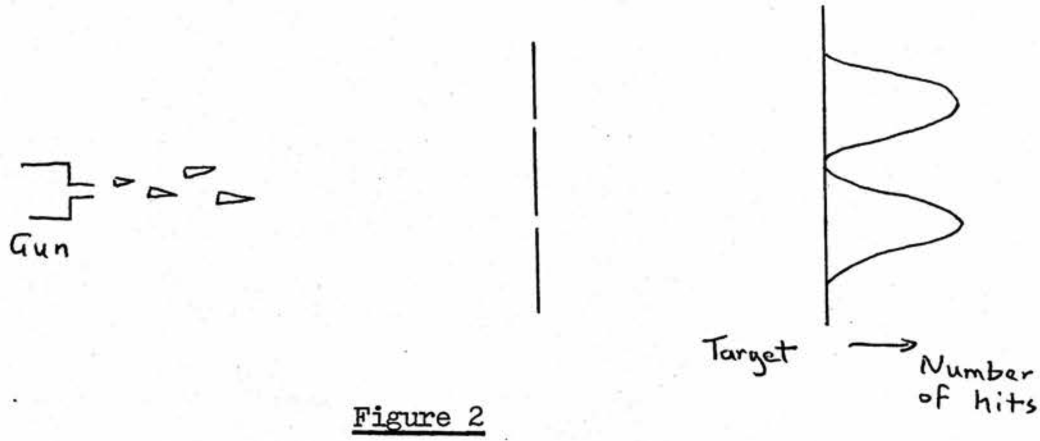
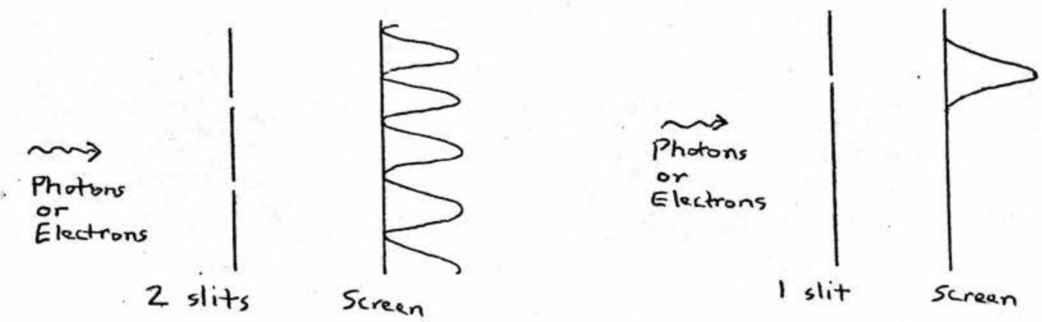


Figure 2



(a) (b)

Figure 3

particles are detected as single events in a detector and the interference pattern is the summation of many such events.)

This suggests that an electron or photon can interfere with itself. A particle model would suggest that each particle travels only through one slit, since it cannot be in two places at the same time, yet in the two-slit case, the presence of the other slit (through which the particle presumably did not pass) alters the outcome so that the interference pattern occurs. Nevertheless, the arrival of a particle at the detector is a single event, even in the two-slit case.

Further, if a detector is placed at each slit, then for each electron entering the system, a detector will either register the electron or it will not. We never get half a 'click' from a detector. (However, any such measurement disturbs the electron's motion and destroys the interference pattern.)

Another major difficulty with a conventional particle model has been pointed out by Bohm. There are certain regions on the target where (in the two-slit case) an interference minimum occurs, so that few or no electrons arrive at these regions. However, in the one-slit case, some of these regions may receive a large number of electrons. As Bohm puts it

"How can the opening of a second slit prevent the electron from reaching certain points that it could reach if this slit were closed?"[27]

QM provides a consistent account of all these phenomena. The electron's behaviour is governed by the wave function ψ. In the two-slit case, ψ causes interference just like any other wave, so that the amplitude of ψ at the screen is alternately large and small in adjacent regions. The magnitude of ψ at any point represents the probability that the electron will be located at that point. Points on the screen where ψ is large will receive a large percentage of the electrons. As we fire more electrons at the slits, the electron distribution tends to copy the spatial variation in the amplitude of ψ. Thus QM provides statistical information about an ensemble of electrons, but since we need to observe a large number of events to see what pattern actually does emerge, QM provides an adequate description. (Similarly, in the one-slit case, QM predicts a distribution corresponding to single-aperture diffraction, and for a large number of electrons such a distribution is obtained.)

However, although QM accurately predicts the experimental results, it has now become extremely difficult to talk about 'the position' of an electron, because the electrons behave partly like waves (being affected by both slits) and partly like particles (arriving at specific points on the target). Thus not only are our measurements restricted (in the way HUP describes) but our ability to describe the behaviour of physical systems, even when no measurement is occurring, is also hampered. It goes without saying that this does not constitute a problem for a positivist, for whom description without observation is meaningless. Nevertheless, many physicists have been concerned by this difficulty, and a number of solutions have been proposed.

Bohr took this restriction very seriously, claiming that our ordinary (classical) physical concepts were simply inadequate for describing microsystems. He argued that any attempt to choose between concepts like 'particle' and 'wave' would inevitably lead to ambiguity and confusion. He introduced the term <u>complementarity</u> to describe this view. The principle of complementarity implies that wave and particle models are both applicable to a certain extent; they are mutually exclusive, and neither constitutes a complete description of reality.

Bohr stressed the connection between the use of any particular mode of description (wave or particle) and the specific experimental procedure being employed to examine the system. Experiments which involve detecting electrons as they pass through the slits necessarily give rise to particle behaviour (no interference), while experiments using two unobstructed slits gave rise to wavelike properties (interference patterns).

Bohr extended his complementarity principle to other aspects of QM, and indeed to other sciences. For example, he argued that 'causality' and 'space-time description' were a pair of complementary modes of description in QM. <u>Either</u> we describe systems using the Schrödinger equation (where the system evolves causally provided it is not disturbed) <u>or</u> we interact with a system (and thus alter its behaviour in an uncontrollable way) in order to measure its physical state. We can have either causal evolution or quantitative measurements, but not both at the same time[28].

The extension of complementarity into other areas such as biology, psychology and even theology will be discussed in Chapter Three. I quote here a single example. Bohr argued that biological experiments in vitro were complementary to experiments in vivo. For instance, we can describe an organ either in terms of its function for the living organism, or in terms of the organ's internal structure Experiments which study the internal structure of an organ are, according to Bohr, necessarily fatal to the host organism, so that the two modes of description, and the procedures used to obtain these descriptions, are mutually exclusive.

Critics of Bohr's 'complementarity principle' have argued that the use of this term reifies and exaggerates the philosophical significance of what may well be a temporary limitation on our experimental techniques and on our knowledge. As we shall see in Chapter Three, it certainly seems to be true that some authors have invoked complementarity to defend vitalism in biology.

A detailed analysis of complementarity is hampered by Bohr's rather vague and ambiguous use of the term. As Jammer[29] points out, Einstein could not comprehend Bohr's usage, and even supporters of Bohr, such as von Weizsäcker, apparently did not fully appreciate Bohr's meaning.

Having discussed the limitations which QM imposes on our descriptions of reality, let us now examine some critiques of QM which argue that more detailed descriptions of reality are possible. I shall discuss two such critiques, both of which have been highly influential, and both of which will be referred to again later in this thesis. These alternative arguments are Bohm's hidden variable theory (HVT) and the Einstein, Podolsky and Rosen thought-experiment.

As discussed earlier, HVTs can allow a more detailed hypothetical picture of what occurs when a measurement is made. This greater detail can also be obtained even when measurements are not being made. For example, in Bohm's (1952) HVT, it is possible to refer to precise values of (say) the position and momentum of an electron at all times[30]. In addition, the behaviour of the electron is completely causal. Causal interactions take place between hidden variables at a 'sub-quantum level'. Our existing measuring instruments do not give us access to this level, but only to the 'higher' level at which QM operates. There is no empirical difference between this HVT and QM because the HVs 'average out' to the values predicted by QM. In particular, the uncertainty principle is retained as a practical limitation on our knowledge. However, there is an important conceptual difference between the theories, namely that within Bohm's HVT it makes good sense to refer to observables such as position and momentum simultaneously, even if we choose not to, or are at present unable to, measure them simultaneously.

To illustrate this difference, consider the two-slit electron interference experiment. Within Bohm's HVT,

"this experiment is described causally and continuously in terms of a single precisely defined conceptual model."[31]

Here, the electron is a real particle, and ψ represents a real physical field which sets up forces which act on the electron. The ψ wave is diffracted by the slits and sets up an interference pattern, while the electron, like a classical particle, passes through one or other of the slits. Because the amplitude of ψ varies markedly on the far side of the slits, the electron experiences complex forces as it travels towards the target. The electron's trajectory is therefore difficult to describe, and in practice only statistical predictions about its final position are possible. These predictions turn out to be identical with those of QM.[32] The conceptual distinction between the theories is that in the HVT the electron follows a definite trajectory, about which we can meaningfully talk even if in practice it is not possible to measure it.

Einstein's response to the apparent incompleteness of QM was quite different. As one might suppose from his well-known statements about God not playing dice, Einstein was unhappy with the idea of renouncing causality. Initially, he tried to show that the uncertainty principle was not fundamental, and he produced a number of thought-experiments which claimed to provide simultaneous arbitrarily accurate values for apparently incompatible observables. However, in each case, Bohr managed to show that Einstein had neglected some feature of the experiment and that the observed system was necessarily disturbed by the measurement, so that uncertainty remained.[33]

Although Einstein was apparently forced to concede that QM was consistent, he refused to accept that it was a complete description of reality. His position was most strongly expressed in a paper which he co-authored with Podolsky and Rosen in 1935 (hereafter referred to as EPR)[34]. EPR argue that incompatible quantities could still have precise values, although we may not know what these values are.

This paper has exerted a great influence on later work on the interpretation of QM; in particular, it forms the background for the experimental tests of local hidden variables. However, EPR's discussion is rather difficult to follow. In a textbook written in 1951, Bohm[35] recast the EPR argument into a clearer form, and virtually all subsequent references to EPR make use of Bohm's version. I shall adopt the same procedure.

EPR first state their philosophical premises. First, every element of physical reality must have a counterpart in any physical theory which claims to be a complete description of reality. Second, if, without in any way disturbing a system, we can predict with certainty the value of a physical quantity, then there exists an element of reality corresponding to this physical quantity.

Within these premises are other implicit assumptions, and it will be useful for the argument which follows to point them out. Implicit in the first premise is the assumption that the world can be analyzed into distinct, separable 'elements of reality'. Implicit in the second premise is the assumption that a prediction of the value of a quantity which is sure to be correct is in every way equivalent to an actual measurement of that quantity.

EPR then describe a particular thought-experiment. Consider a molecule for which the total spin angular momentum is zero[36]. The molecule consists of two atoms, each with non-zero spin, and these spins are oppositely directed so that their vector sum is zero. We can define spin components along any direction. Let us choose three mutually-perpendicular directions x, y, and z, and denote the spin components along these directions by σ_x, σ_y, and σ_z. Because the total spin is zero, the spin components of the two atoms along any of these three axes are equal and opposite; that is,

$$\sigma_{x_1} = -\sigma_{x_2},\ \sigma_{y_1} = -\sigma_{y_2},\ \text{and}\ \sigma_{z_1} = -\sigma_{z_2}.$$

An additional feature which is unique to QM and follows directly from the uncertainty principle is that, for any particle, no two perpendicular spin components can both be known with complete precision. For example, $\Delta\sigma_{x_1}\Delta\sigma_{y_1} > h$, $\Delta\sigma_{y_2}\Delta\sigma_{z_2} > h$, and so on.

If the molecule dissociates into its constituent atoms and these atoms then fly apart, the total spin of the pair of atoms remains constant (at zero) and the above relationships between the spin components continue to hold. Therefore, if we measure, say, σ_{x_1}, we can predict the value of σ_{x_2} with complete certainty, since $\sigma_{x_2} = -\sigma_{x_1}$.

According to EPR's definition, σ_{x_2} is an element of reality. But we have not physically interacted with the second atom in any way, which suggests that σ_{x_2} was an element of reality even before we measured σ_{x_1}, because this measurement could not have had any effect on the second atom. (Recall that the argument is independent of the distance between the two atoms so that they could be millions of miles apart.) However, we could instead have measured σ_{y_1}, and hence predicted σ_{y_2} with certainty. Thus, according to EPR, σ_{y_2} is also an element of reality. By extension, we could reorient our measuring apparatus while the atoms were in flight, and so conclude that <u>all</u> possible components of the second atom's spin are, simultaneously, precisely defined and real. This contradicts the uncertainty principle so that QM does not completely describe reality.

As is typical in the interpretation of QM, the thought-experiment can be interpreted in several ways. For example, Bohr emphasised the fact that different experimental arrangements are required to measure different spin components, and that these arrangements are mutually exclusive. According to Bohr, the concept of a physical property cannot be considered in isolation from the measurement procedures by which a value for the property is obtained. Since no two spin components can be measured at the same time, such spin components are not simultaneously real.

Thus, Bohr resolves the EPR problem by rejecting both of their premises. First, it is not the case in QM that there is a one-to-one correspondence between elements of reality and elements of the mathematical formalism. QM yields only statistical predictions of outcomes, and many classical concepts, such as particle and wave, are simply inappropriate for describing microsystems.

Secondly, whereas EPR claim that a physical quantity is real if its value can be predicted with certainty, Bohr attributes reality only to quantities which are actually measured experimentally. This is quite consistent with the earlier discussion of Bohr's account of the two-slit experiment, where the choice of model (wave or particle) is inextricably linked with the experimental procedure being used.

EPR does not only raise questions about QM. Any alternative 'realist' theory should also be able to account satisfactorily for EPR. For example, Bohm's HVT reproduces the results of QM, including the uncertainty principle. Let us examine how this theory copes with EPR.

Bohm suggested two alternative solutions. The first applies if QM's predictions are actually verified by experiments, and the second raises the possibility that QM might be empirically falsified. Let us begin with Bohm's first possibility.

The problem is to reconcile the predictions of QM, which are retained in Bohm's HVT, with the additional assumption that all the spin components are real and precisely defined, as required in a realist HVT. Recall that if σ_{x_1} is measured then σ_{x_2} is known precisely while σ_{y_2} and σ_{z_2} are not. Thus if σ_{y_2} were to be measured after σ_{x_1} was measured, the value obtained for σ_{y_2} would be random. If, on the other hand, we had measured σ_{y_1} followed by σ_{y_2}, the value obtained for σ_{y_2} would now be fixed, and set equal to $-\sigma_{y_1}$, while the other spin components of the second atom (σ_{x_2} and σ_{z_2}) would be random.

Because of the nature of Bohm's theory, we cannot evade the responsibility of providing a causal explanation for the behaviour of the second atom. In other words, we have to ask: how does the second atom 'know' which spin components should be random and which should not, and how can this knowledge be 'updated' instantaneously should we decide (after the atoms are in flight) to measure a different spin component of the first atom?

For Bohr, of course, none of these problems exist, because until a measurement is performed, none of the spin components are meaningful. However, Bohm's HVT can only agree with QM if the HVs are allowed to carry 'messages' from the first to the second atom, 'telling' the second atom which spin components it should randomise. These signals, moreover, have to be able to travel at an infinitely fast speed in order to reproduce QM's prediction of an instantaneous change in the second atom should we decide to measure a new component of the first atom's spin. The necessity of such faster-than-light signalling over arbitrarily large distances is, to many people, a major difficulty for any HVT which attempts to reproduce the predictions of QM.[37]

We turn now to Bohm's second possible solution. Bohm pointed out that no existing experiment has actually tested the predictions of QM under the extreme conditions of the EPR thought-experiment. All the experiments which have examined correlations between pairs of particles are static, so that (for example) spin directions are fixed throughout the experiment. Under these circumstances, there is no need to postulate infinitely-rapid signals passing between the atoms because much slower signals would still have time to transmit the information about the spin direction which was going to be measured. This raises the possibility that if the spin measuring devices were to be reoriented while the atoms were in flight, then the correlations predicted by QM (and by Bohm's HVT) might not be found. Bohm's discussion of this possibility in 1957 and again in 1962 [38] anticipates the much later attempt by Aspect to construct an experiment to test this possibility. This experiment will be discussed below.

Bell's Theorem and Local Hidden Variables.

The EPR argument was originally constructed to support the argument that QM does not provide the most complete possible description of reality. However, EPR did not suggest that the predictions of QM were empirically incorrect. Nevertheless, some thirty years later, EPR was revived by John Bell and used to make such a suggestion[39].

Bell used the same basic physical situation as EPR; that is, a coupled system dissociates into two parts, and QM predicts that the properties of the two parts are not independent, even if they become widely separated. In other words, long-range correlations exist between the properties of the subsystems. Bell constructed an alternative hidden-variable theory in which such correlations were absent. According to this theory, the subunits behave quite independently of one another from the moment they separate. This theory is therefore a local hidden-variable theory (LHVT) whereas QM is clearly non-local.

Some authors[40] have remarked that most physicists, including many supporters of QM, intuitively think of the world in 'local' terms. Although most physicists may recognise the existence of coupled systems which (according to QM) can only be described in terms of their combined properties, there is nevertheless a tendency to drop this approach when the correlated subsystems become separated. It is tempting to think, for example, that each member of a photon pair (produced by electron-positron annihilation) has a well-defined set of properties which are manifested quite independently of what we do to the other member of the pair.

The great significance of Bell's work is that he was the first person to point out explicitly that such local mental pictures are quite inconsistent with the predictions of QM. This seems a rather startling finding, and in retrospect we may be surprised at the lack of interest in locality in earlier years. Even Bohm's attempt to provide a realist underpinning of QM, which explicitly mentioned the need for instantaneous nonlocal interactions in any such attempt, did not arouse much widespread interest in the essentially non-local character of QM. The later experimental tests of Bell's postulated LHVT seem to suggest that the world is really non-local, and (as we shall see) many of the experimenters have hailed this as an important finding. However, in a way it is strange to find this sort of reaction to the experiments, since they merely appear to confirm the predictions of a theory which has been generally accepted for over fifty years.

Certain aspects of QM's nonlocality, such as the Pauli exclusion principle[41], were recognised at a very early stage, and even nonlocality on a macroscopic scale was not unknown. For example, the accepted account of superconductivity assumes coherent behaviour of electrons throughout a sample of macroscopic dimensions. Yet as far as I can tell, Bell was the first person to introduce explicitly the concept of locality in connection with the EPR thought-experiment. Perhaps the reason for the surprise with which Bell's work was greeted in some quarters was that he was the first person to construct an explicit local formulation of a physical process like the EPR experiment and to show that such a formulation was incompatible with QM. In addition, it is very easy in the context of EPR to show that QM's nonlocal predictions can extend not only over macroscopic, but also over astronomical distances. As one author put it, "[Bell's theorem] shows that our ordinary ideas about the world are somehow profoundly deficient even on the macroscopic level."[42]

Let us examine the distinction between QM and LHVT in more detail, for a particular experimental situation. The most commonly discussed case is the correlation between the polarizations of a pair of photons created in such a way that (according to QM) they constitute a coupled system. Thus, according to QM, the wave function of each photon contains elements which relate to the state of the other photon. In contrast, LHVT claims that the state of an individual photon is governed by a set of hidden variables and that this state function contains no reference to the other member of the photon pair. Thus (according to LHVT) localization occurs as soon as the photon pair is emitted.

This distinction between QM and LHVT, although conceptually obvious, is not at all marked at the experimental level. For example, both theories would predict that if (say) the polarization of one photon is found to be parallel to a certain axis, then that of the other photon will, if measured, be found to be antiparallel to that axis. However, the ways in which the two theories arrive at these predictions are rather different. According to QM, the photons remain coupled until one is measured, and this act then fixes the parameters of the other photon[43]. According to LHVT, the parameters of the photons are set, once and for all, on emission, but their values are such that the same complementary relationship holds. The difference is a subtle one, and it is difficult to imagine an experiment which could discriminate between these two theories.

In his 1964 paper[44], Bell describes an experimental procedure which could, in principle, provide such a test. What is more, Bell's reasoning is remarkably general, and discriminates between QM and all possible LHV theories, irrespective of the details of their structure. Since 1964, a large number of modifications of 'Bell's Theorem' have been proposed, and some of the later versions bear very little resemblance to the original[45]. Nevertheless, all such theorems conclude that although QM and LHVT are indistinguishable for single pairs of measurements, it is possible to discriminate between the theories by comparing the results of a series of measurements taken at different orientations. In the case of photon polarization correlation experiments, an empirical quantity (usually denoted by λ) is defined as a sum of coincidence counting rates[46]. Such rates count the frequency with which both members of a pair successfully pass through polarization analysers, and these rates depend upon the orientations of the pair of analysers. Rates for different orientations are added to give a value for λ. 'Bell's Inequality' states that, for any LHVT, λ is less than zero. In contrast, according to QM λ can be a positive quantity if the analyser orientations are chosen correctly. In this way, a series of measurements should allow a clear distinction between QM and LHVT. The difficulty arises in matching this theoretical prediction to the real (and therefore imperfect) conditions found in actual experiments.

Experimental Tests of Hidden Variable Theories.

Until fairly recently (1967), discussion of the interpretation of QM, and of the validity of possible alternatives, was confined to theoretical work involving philosophical or mathematical analysis and reference to thought-experiments. Since 1967, a number of real experiments have been performed which attempt to discriminate in a more clear-cut way between various alternative theories. A major theme of this thesis is that the switch to experimental investigation did not mean the end of theoretical and philosophical wrangling. However, before discussing this point, it may be useful to describe the basic features of the various experiments which have been performed.

(1) Papaliolios' test of the Bohm-Bub theory.[47]

Earlier, it was pointed out that Bohm and Bub added a nonlinear term to the Schrödinger equation and so implied that reduction of the wave function was a physical process which took a certain amount of time to complete. In hidden-variable terms, measurements disturb the HVs and it takes time for the equilibrium distribution (which averages out to QM) to be restored. The time required for this process is often referred to as the relaxation time.

Bohm and Bub suggested, as a tentative estimate, that the relaxation time was of the order of 10^{-13} seconds. As we shall see in Chapter Four, this value seems to have been rather arbitrary, and this had important consequences for the reception of Papaliolios' experiment. Nevertheless, this value appears explicitly in their paper,[48] and raised the possibility that an experiment could either corroborate or falsify their theory. If two consecutive measurements are performed on a physical system and less than 10^{-13} seconds passes between the first and second measurements, then the HVs will not have had time to return to their equilibrium values and the results of the second measurement will not be in accordance with the predictions of QM.

Papaliolios performed polarization measurements on photons, using polaroid filters with axes oriented as in Figure 4. The arrows indicate the polarization axes of the polarizers. Light which passes through A is polarized along the direction of A's axis. Only a fraction of this light can then pass through B; the amount depends on the angle between the axes of A and B. Since these axes are chosen to be almost perpendicular, very little light will emerge from B. Thus any photons leaving B are in a precisely-defined state and (according to Bohm and Bub's theory) the values of their HVs are also well-defined. This makes it much easier to produce conflicting predictions for the two theories.

In travelling from A to B, the HVs have plenty of time to return to their equilibrium values, but this is not the case between B and C because of the shorter distance involved. Therefore, the QM predictions for the intensity of light emerging from C[49] may not hold. Figure 5 shows the conflicting predictions for the two theories. The actual experimental results agree with QM to within 1%. Thus either the Bohm-Bub theory is incorrect, or the relaxation time is much smaller than the lower limit set by Papaliolios' experiment. This lower limit was about 2×10^{-14} seconds, roughly one-fifth of Bohm and Bub's original estimate. The interpretation of these findings will be discussed in Chapter Four.

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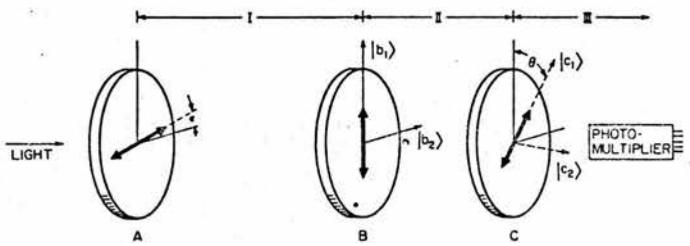
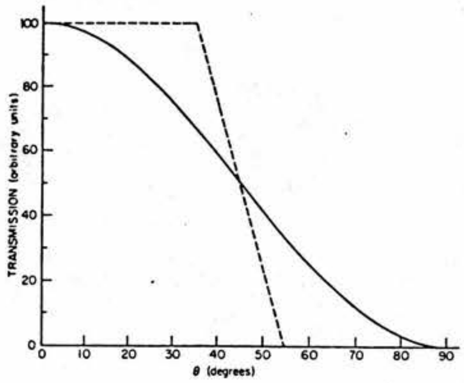


Experimental arrangement of linear polarizers A, B, and C. The heavy arrows indicate the direction of polarization transmitted by each polarizer. The arrows labeled $|b_1\rangle$ and $|b_2\rangle$ indicate the direction of polarization for the eigenstates of polarizer B, and $|c_1\rangle$, $|c_2\rangle$ for the eigenstates of polarizer C. Angle $\epsilon = 10°$.

Figure 4



The solid curve indicates transmission versus θ according to quantum mechanics and is proportional to $\cos^2\theta$. The dotted curve is that predicted by the Bohm-Bub theory for $\epsilon = 10°$, assuming no relaxation of the hidden variables. The data, taken at a relaxation time of 7.5×10^{-14} sec, agree with the quantum-theory curve to within 1%.

Figure 5

(2) Experimental test of Wigner's 'Consciousness' interpretation.

An ingenious test of this interpretation was performed by Shimony and some of his students in 1976[50]. This group reasoned that if an observer is responsible for reducing a wave function by some psychophysical interaction, then it might be possible to discriminate, by introspecting on one's mental sensations, between situations in which one has, or has not, reduced a wave function. The authors concede that this possibility is not demanded by the consciousness interpretation, and that it constitutes an additional hypothesis. Failure to make such discriminations might then have little to do with the basic hypothesis that conscious observers reduce wave functions. Nevertheless, the experiment is interesting since it represents as yet the only attempt to test the consciousness interpretation by empirical means.

In this experiment a radioactive substance is placed near a Geiger tube which is connected to an amplifier and a pair of counters. The count rate was approximately one event every thirty seconds. The counters were observed by two observers, A and B, situated in separate rooms. By using delay lines, it was ensured that any event (radioactive decay, tube activation and counter increment) would be observed in A's room before the signal reached B's room. When the event was observed, the wave function of the combined system (radioactive atom plus apparatus) was reduced from a superposition (a mixture of 'atom not decayed/counter not activated' and 'atom decayed/ counter activated') to a pure state ('atom decayed/ counter activated').

Normally, observer A, being the first to see the event, would be responsible for the reduction, so that the wave function which reached B would by then already be reduced. However, it was arranged that, at random intervals, A would stop looking at his counter, so that B (who looked all the time) would, in such cases, be the first to observe the event and so would initiate reduction. The aim of the experiment was to see if B's sensations as he observed an event differed according to whether he was or was not responsible for the reduction.

In order to help observer B to make such a judgement, he was 'trained' by observing a number of events which A had already seen and a number which A had not seen. It is of course possible that the training period was too short, or (as suggested earlier) that the two types of observation do not give rise to noticeably different sensations. The result of this experiment was that B's estimates of whether he was responsible for reduction were correct exactly 50% of the time, a result which (with remarkable accuracy) fits the hypothesis that B was simply guessing.

This experiment clearly has many limitations and is far from being conclusive. Indeed, one might wonder why it was ever performed, given the methodological objections which can be raised against it. I shall discuss this issue at some length in Chapter Four.

(3) Experimental Tests of Local Hidden Variable Theory.

A large part of this thesis is devoted to LHV experiments, and to the ways in which their results were obtained, received and interpreted. Therefore, it will be necessary to refer to many detailed aspects of the experiments in the context of this later discussion, and there seems little point in providing a comprehensive account of the experiments at this stage. However, some general comments on their aims and procedures may be useful here.

The aim of all such experiments is to measure correlations between the properties of pairs of particles which fit the basic demands of the EPR thought-experiment. One experiment looked at the spin correlations of protons. All the other experiments used the polarization correlation of photons, and I shall restrict the present discussion to the latter case.[51]

Correlated photon pairs can be produced in a number of ways, and the polarizations of the pair may (according to QM) be either antiparallel or mutually perpendicular, depending on the mode of production.[52] In either case, EPR-type correlations are predicted. It is not possible to perform a direct measure of polarization. Instead, the photons enter polarization analyzers (often referred to as 'polarizers'). Ordinary sheets of polaroid could be used, though more efficient systems are available. Analyzers have a preferred axis of polarization, and the transmission rate (between 0 and 100%) depends on the difference in direction between the analyzer's axis and the photon's polarization. (Complete alignment between these directions gives 100% transmission, while no light will emerge from the polarizer if these directions are perpendicular.) Detectors are then used to measure the transmission rate. Figure 6 shows a schematic diagram of the process.

There are many practical difficulties associated with such experiments. For instance, only a small fraction of the photon pairs will travel in the required directions and encounter the measuring apparatus. Most of the pairs will be emitted in different directions and so will not be detected. The count rates are therefore small, and long runs (up to several hours in the early experiments) are needed before significant amounts of data are accumulated.

A more serious difficulty is that the photons may not be emitted in exactly opposite directions as shown in Figure 6. One method of photon production uses atomic cascades[53], and the emitting atom may recoil, carrying off some momentum and destroying the symmetry of the arrangement. A photon may therefore avoid detection by 'missing' the apparatus while its partner on the other side passes straight through the other set of apparatus and is detected. An additional problem with the use of cascades is that the photons produced are of low energy and cannot be detected with great efficiency.

Both of these problems are serious ones, because they distort the results. Thus the detection of a <u>single</u> photon may lead us to make incorrect inferences about the polarization of its partner, simply because the partner flew off at an angle or failed to register when it arrived at a detector. Both problems can be avoided by using high-energy photons produced by electron-positron annihilation[54], but it is very difficult to measure the polarization of such photons.

In addition, there is a considerable amount of 'noise' in all photon experiments, produced partly by the presence of stray light from outside the system, and partly by random triggering of the detectors even when no photon is present, due to background radiation and thermal 'dark current'. In all the experiments, electronic coincidence circuitry was used to minimise noise. To illustrate this, Figure 7 shows data from an early correlation experiment in which the polarizers' axes were set either parallel or perpendicular.We would then expect either 100% or zero coincidence. An artificial delay is introduced between the registration of signals from the first and second detectors. If the coincidence is spurious, and due to random

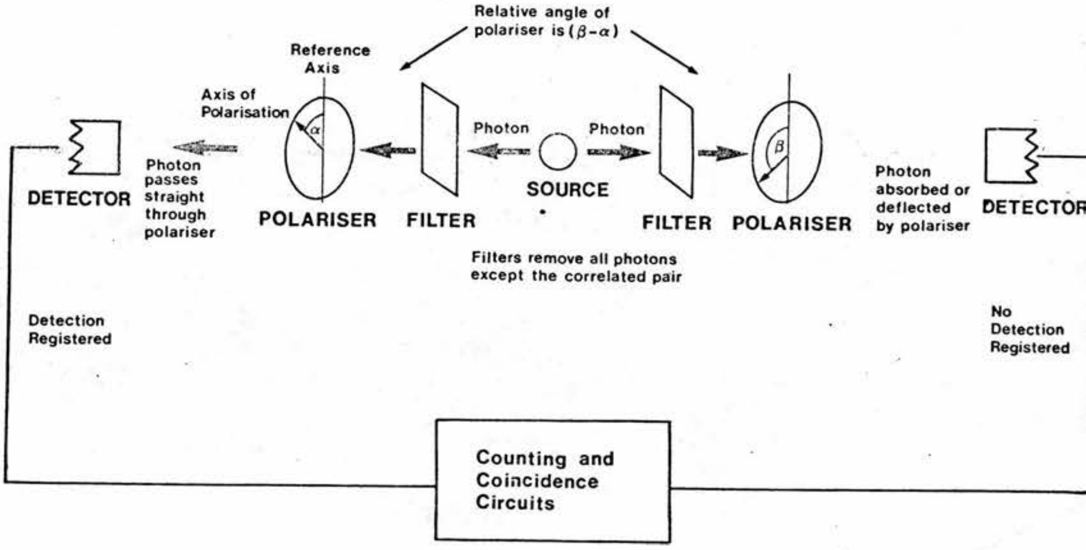
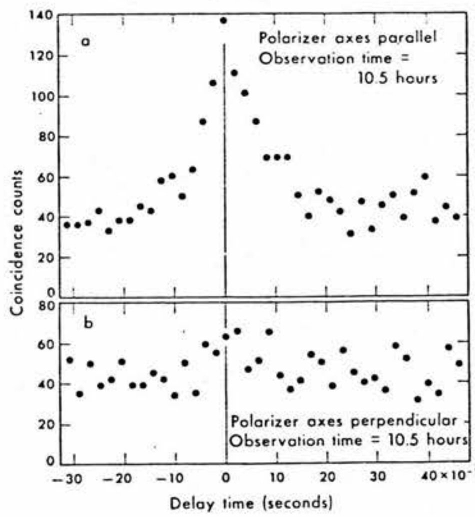


Figure 6



Coincidence counts, as a function of relative time delay, showing polarization correlation. Each point represents a sum over three analyzer channels. Very slight peak in (b) is probably due to 6% transmission of crossed polarizers.

Figure 7

effects, it should be independent of the delay time. If genuine, it should only show up when the delay time is zero. The graphs in Figure 7 show the presence of genuine coincidences together with a large amount of noise.

Let me summarize the results of these correlation experiments. The proton spin experiment found that the QM predictions were accurate. Four experiments using high-energy photons have been performed. Three gave results in agreement with QM while the fourth was ambiguous; its results lay on the limit of Bell's Inequality, since the quantity λ was calculated to be approximately zero.

Four experiments using low-energy photons have also been performed. Three gave good agreement with QM while the fourth came out clearly in favour of LHVT. The reception of this anomalous experiment is especially interesting and will be discussed in detail later. At the risk of pre-empting this discussion, however, it would be accurate to say that this experiment is not now accepted by the LHVT group as a valid result.

<u>Note</u> Apart from Chapter Three, the rest of this thesis is concerned almost exclusively with <u>critical</u> approaches to QM. That is, the work to be examined is concerned with reinterpreting or modifying the theory in order to remove perceived difficulties either with the conventional interpretation or with the theory itself. Such work is quite different from 'routine' or 'orthodox' extensions of QM, in which the theory is applied in new contexts. Rather than make this distinction repeatedly in the text, I shall adopt physicists' own usage, by referring to such <u>critical</u> work as work dealing with '<u>Foundations of QM</u>' (FQM).

Chapter Three

Quantum Mechanics and Society

Introduction

The general theme of this thesis is the relationship between scientific knowledge and the social context in which that knowledge is generated, transmitted and interpreted. As pointed out in Chapter One, the term 'social context' can be defined in a number of ways. In the present chapter, it is employed in its widest sense, to represent society as a whole; that is, the social, cultural and political milieu in which the activity of physicists is embedded.

In Chapter One, I argued that the content of scientific beliefs is not uniquely determined by the application of a single methodology or by interaction with the natural world. It is therefore possible to inquire whether scientific beliefs reflect the context in which they originated or were developed. I also argued that knowledge should be seen as a resource to be actively manipulated and employed in the pursuit of personal or social interests, rather than as a set of facts to be accepted at face value. This implies that the same set of scientific beliefs can be interpreted and employed in different ways by different groups.

Both these issues will be discussed here. In the first part of the chapter, I shall discuss two quite different social contexts (Weimar Germany, and the Soviet Union during and after the reign of Stalin) and I shall examine the development of QM in these contexts. Much of the empirical data is taken from the work of Forman[1] and Graham[2], though the approach taken here, and the conclusions reached, differ to some extent from those of the original authors.

In the second part of this chapter, I shall examine the wide variety of inferences which have been drawn from QM by various groups of physicists and non-physicists. We have already seen in Chapter Two that QM can be interpreted in many different ways; here we shall deal with the implications which QM is claimed to have for other areas of belief.

This chapter will serve several purposes. It will provide evidence in favour of the view that even modern physics is not immune from external cultural influences. Since the remainder of the thesis will concentrate on influences within the scientific community, this evidence will be useful in demonstrating that there is no sharp discontinuity between 'internal' and 'external' influences.

Second, an examination of the treatment of concepts such as causality and complementarity by scientists in Germany and the USSR will illustrate the negotiability, or flexibility, of meanings within science. The second part of the chapter will demonstrate that meanings are even more flexible outside physics, and that it is very difficult, even in principle, to discriminate between 'valid' and 'invalid' applications of terms such as complementarity. In Chapter Four, I shall use this finding to criticise another case-study, whose author makes unproblematic identifications of 'causal' and 'dialectical' theories, and I shall go on to propose a model of controversies which is consistent with this finding.

Third, the evidence in the present chapter will introduce us both to the constraining effects of the cultural context (to which physicists respond) and also to the active employment by physicists of the resources provided by this culture. In Chapters Five and Six I shall return to this dichotomy, dealing in detail with both these effects in a much smaller area of scientific activity.

QM and the Weimar Republic: Forman's Case-Study.

Forman's case-study of the origins of QM in Germany in the 1920's is often cited in texts on the sociology of scientific knowledge, though it is usually described as 'speculative' or 'controversial'.[3] The reason for this is that it is possible to read his paper as claiming that cultural factors led physicists to alter the content of physics. If accepted, such a claim would provide strong evidence in favour of the sociology of knowledge.[4] Unfortunately, Forman's arguments are often ambiguous, and at many points in his paper he makes much weaker claims for the role of cultural factors in the development of QM. It is therefore important to examine his data and his arguments in some detail.

QM was developed in the 1920's mainly by German-speaking Central European physicists. It was (and still is) represented as an acausal theory, and was thus seen to constitute a break with the traditions of 'classical' physics. At the same time as QM was being developed, the dominant cultural movement in Germany was anti-scientific, irrationalist, pessimistic and anti-determinist - the so-called 'Lebensphilosophie', characterised by Spengler's highly influential book 'The Decline of the West'. Forman asks whether the development of an acausal science in such a context was purely coincidental. He concludes that it was not, but it is difficult to decide just how strong he thinks the connection is.

Certainly, many physicists participated in the general cultural movement, reading Spengler and adopting a philosophical dislike for causality. By quoting physicists' speeches before lay audiences, Forman persuasively argues that they were eager, even before the development of the 'new' quantum theory[5], to present physics in such a way that it did not seem to be in conflict with the general cultural trend. For example, Reichenbach argued that complete determinism was an unnecessary and unjustified assumption which, in the interests of economy, should be rejected.[6] Schrödinger claimed that even in classical physics, what we actually observe are statistical laws, which do not require causal determination of individual events.[7]

Forman cites a number of similar cases, noting that the arguments against causality were often less than rigorous:

"What is most striking is [Nernst's] resolve to sink the law of causality by hook or by crook"[8]

and

"the most striking features of the manifesto are, on the one hand, the quasi-moral terms in which causality....is repudiated [by Schrödinger] and, on the other hand, the frivolousness with which the objections to dispensing with causality are dismissed."[9]

A large number of physicists made virtually simultaneous 'public conversions' to acausality. Since, as he points out

"there were at just this moment no specific developments in physics which could plausibly be regarded as the source of such acausal convictions"[10]

it seems a plausible conclusion that

"what we are dealing with is, essentially, a capitulation to.... intellectual currents in the German academic world."[11]

However, the extent of this capitulation is not clear. Most of Forman's evidence in favour of this apparent change in physicists' attitudes consists of extracts from speeches made by physicists before general bodies of academics. He concedes that such statements may be part of a 'public relations' exercise, rather than expressions of physicists' own convictions:

"When one recalls that the audiences for most of these renunciations of causality were, in the first instance, the whole body of the university assembled on a ceremonial occasion, then I think it reasonable to construe such renunciations as attempts to alter, or at least receive a special dispensation from, an unbearably opprobrious public image of the theoretical physicist as a 'hard-boiled determinist'".[12]

This claim may be uncontentious. However, Forman apparently wishes to go further, by making the charitable assumption that such public declarations represented a real desire among physicists to dispense with rigorous causality:

"There was in fact a strong tendency among German physicists and mathematicians to reshape their own ideology towards congruence with the values and mood or their environment."[13]

If we reject this argument, we are forced to conclude that the public statements were "mere image projection"[14] and that "physicists and mathematicians were engaged in a cynical manipulation of their image."[15]

What evidence is there that physicists were not simply engaged in image manipulation? The most convincing sign that physicists took the rhetoric seriously is their attitudes to 'crises'. One of the central tenets of Spenglerian philosophy was that the whole Western world was experiencing a series of crises. As one would expect from the 'image management' argument, physicists were eager

"to serve themselves with the crisis rhetoric when addressing a general academic audience"[16].

However, Forman goes further than this, arguing that physicists were impelled to look for, and to create, crises in their own discipline. In particular, over a very short time, a crisis was perceived (or constructed) in the 'old' quantum theory. Forman claims that this perception was not justified on technical grounds:

"While it is undoubtedly true that the internal developments in atomic physics were important in precipitating the widespread sense of crisis....nevertheless it now seems evident to me that these internal developments were not in themselves sufficient conditions. The possibility of the crisis in the old quantum theory was, I think, dependent on the physicists' own craving for crises, arising from participation in, and adaptation to, the Weimar intellectual milieu ....the crisis of the old quantum theory, far from being forced upon the German physicists, was more than welcome to them."[17]

This, then, is a rather stronger claim, namely that social pressures altered physicists' perceptions of their own discipline, and, precipitating the conviction that something was seriously wrong with the current theory, led them to attempt to modify it. In other words, external social pressures altered the direction of physics.

Forman also claims that these social pressures also affected the reception given to proposed alternatives to the old theory. Proposals were judged not only on their technical merits, but also on the extent to which they renounced causality. There are three pieces of evidence to support this claim. First, Forman discusses a paper by Bohr, Kramers and Slater, which was speculative yet also involved what Slater called 'discarding rational causation'. Forman writes:

"It is, I think, only by reference to the widespread acausal sentiment that one can understand the immediate and widespread assent which the theory received in Germany, even though it was in fact hardly a theory at all, but rather a vague suggestion."[18]

Secondly, there was a small number of physicists, including Einstein, who had not publicly repudiated causality, and who felt that

"fellow physicists were rushing to embrace a failure of causality without having made any serious attempt to explore the possibilities of a causal solution."[19]

Thirdly, Forman discusses the reception of the 'new' quantum theory. Not only was it rapidly accepted by German physicists, but it was also widely publicised by physicists who were eager to "carry the good news to the educated public"[20]. Articles about the new theory were published in the popular press even before the major technical papers appeared in print.

In a later paper,[21] Forman examines the reception of QM in Britain, where there was no cultural movement against causality. His results are consistent with the claims made above, since British physicists not only showed less interest in the 'crisis' of the old quantum theory, but also laid far less emphasis on the acausal aspects of the new theory.

Thus, Forman is clearly arguing not only that physicists made rhetorical statements about causality for purposes of 'image management', but also that they actively tried to incorporate acausal concepts into their theories, and eagerly welcomed an acausal theory, stressing the fact that it was acausal. We should note, however, that this account is far from being a full-blown 'externalist' or 'relativist' model of the development of QM. It implies, for example, that QM is indisputably acausal; it also implies that although external factors may affect the direction of science by providing specific cultural resources, focussing attention on specific problems, and setting specific goals, internal technical considerations are still responsible for generating and evaluating new theoretical developments.

In some of his writings, Forman seems to be going much further than this, arguing that physicists chose to construct an acausal theory, either partly or wholly as a result of social pressures. Consider the following statements:

"If the physicist were to improve his public image, he had first and foremost to dispense with causality....And this, of course, turned out to be precisely what was required for the solution of those problems in atomic physics which were then at the focus of the physicists' interest."[22]

and even more explicitly

"physicists were impelled to alter their ideology and even the content of their science in order to recover a favourable public image."[23]

The implication of this stronger argument is that acausality might not have been a necessary requirement for the solution of the technical problems which physicists faced. Of course, technical considerations meant that any new theory which physicists came up with would have to be able to account for experimental data, but acausality might simply have been a 'gloss' which physicists chose to place on this theory. As time passed, this acausal gloss may have become reified, and the theory may have come to be seen as inherently acausal.

It seems clear that the controversial status of Forman's paper follows from the fact that it can be read in this way. However, Forman does not clearly come out in favour of this strong account. In his later paper, on the reception of QM in Britain, he describes his earlier work as a study of the reception, and not the construction, of QM in Germany[24]. He also claims, simply as a matter of fact, that QM is acausal:

"the epistemic bearing of the new theory was simply overlooked"

and

"The Solvay Congress....opened the eyes of the participating British physicists to the inescapably acausal character of the quantum mechanics."[25]

Evidently, if QM is inescapably acausal, then neither the Weimar context nor any other events outside the technical domain could have affected the form which the theory took, although of course the popularity and rhetorical usefulness of this form may indeed have been dependent on its cultural context.

In a recent critique of Forman's study, Hendry also rejects the 'strong' account which claims that the acausality of QM was solely a response to the milieu, although he feels that

"Forman has succeeded in demonstrating an influence of the milieu upon physicists' attitudes to causality, and....he could even assert quite reasonably that the attitudes were in some (weak) sense 'caused' by the milieu."[26]

Hendry concludes that, by underestimating the effect of purely internal technical developments in physics, Forman has exaggerated the role of the milieu, at least in his strongest statements.

Given the weakness of the case in favour of the 'strong' reading of Forman, must we conclude that QM is inherently acausal, for purely technical reasons, and that such an acausal theory would have emerged in any cultural context? I would argue that it is possible to reject these conclusions, provided we are careful about what we mean by 'an acausal theory'.

It is possible that, in a different context, a completely different theory could have arisen. For example, Einstein claimed[27] that no serious attempts were made to develop a causal theory; he clearly believed that such a theory might have been found if German physicists had seriously looked for it.

A second possibility is that, given the actual solution to the technical problems - namely, the formalism of QM - a causal interpretation of this theory could have been constructed. We have already seen that most British physicists did not emphasise the supposedly acausal character of QM to anything like the same extent as the Germans did. Moreover, 'causality' is a notoriously vague concept, as Hendry points out:

"Two physicists might agree completely as to the definition of causality, but differ completely as to whether it had been rejected in a theory. In particular the same situation might be seen by one as a rejection of causality and by the other as a temporary absence of a causal theory, and to make matters worse these two views might be expressed in exactly the same words."[28]

In 1952, David Bohm produced a 'causal' interpretation of QM[29], which, although limited in its scope, seems to contain no factual errors, and yields the same (correct) experimental predictions as do more orthodox interpretations of QM. In a sense, then, QM 'is' neither causal or acausal; its perceived character differs from one interpretation to another, and individuals do not always agree as to which interpretation is the best one.

Many factors, other than technical issues, may affect an individual's choice of interpretation, and the reception given to different interpretations by the physics community. This will be discussed in detail in Chapters Four and Six. As things turned out, Bohm's work in the 1950's was not favourably received. We can only speculate about what might have happened if Bohm had produced his 'causal' interpretation in Weimar Germany in the 1920's.

In fact, hidden variables were discussed at that time, for example by Born and de Broglie. de Broglie actually developed a detailed hidden-variable theory as early as 1926, and he abandoned it not because it was found to be in error, but because it "found little favourable acceptance among other physicists"[30]. Born also discussed "inner eigenstates", for example in a passage quoted by Hendry:

"We have no grounds to believe that there are inner eigenstates of the atom that stipulate a determined collision path. Should we hope to discover such eigenstates later (such as phases of internal atomic motion), and to determine them for the individual case? Or should we agree that the agreement of theory and experiment on the impossibility of giving a stipulation of the causal lapse is a pre-established harmony, which rests on the non-existence of such stipulations? My own inclination is that determinism is abandoned in the atomic world. But that is a philosophical question, for the physical arguments are not conclusive."[31]

Born later came out much more clearly in favour of an acausal, probabilistic interpretation of QM which became incorporated in the orthodox 'Copenhagen' view. Yet the above passage makes clear the choice which had to be made to reject hidden variables which would restore at least the appearance of causality. Hendry also recognises the implications of hidden variables, when he notes, after discussing Born's arguments in favour of acausality, "The existence of hidden microscopic parameters could have changed this, of course"[32].

I certainly do not wish to imply that German physicists in the 1920's were foolish to ignore the possibility of hidden-variable interpretations of QM. There were very good technical reasons for the actual form which QM took in this context. In Chapter Four, and more especially in Chapter Six, I will discuss other cases in which theories and interpretations were evaluated in terms of 'internal', technical criteria (that is, in terms of the culture of physics), yet I will argue that the outcome of this evaluation process remained at least partially arbitrary, conventionalised, and problematic.

The implication is that, if QM had arisen in a context in which causality was a respected idea, alternative theories, and causal interpretations of QM, would have been more favourably received; if more people had tried to develop approaches like Bohm's and de Broglie's it is possible that a more complete alternative to 'orthodox' QM would have emerged.

Obviously, this does not constitute a proof that an alternative theory was possible in Weimar Germany. We are entitled to ask whether Bohm, transplanted back into the 1920's, would have been able to construct a causal interpretation without the advantage of having an acausal version to start with. Even if we accept the possibility of alternative outcomes to the actual one, it is by no means the case that physicists can always come up with a technically and philosophically respectable theory on demand. Of course, it is a matter of individual preference whether we ascribe this to the limited ingenuity of physicists or to the intractability of nature.[33]

Quantum Mechanics in the Soviet Union

In this section, I shall examine the effects of the changing political context in the Soviet Union, between the 1920's and the 1960's, on the sorts of interpretations of QM which were held by Soviet physicists and philosophers. As is well-known, Soviet science was often influenced by political developments during this period, the most obvious example being Lysenkoism[34]. The development of QM in the Soviet Union has also been well documented[35].

In many ways, the case of Soviet QM is quite different from the case of Lysenkoism. As has been discussed in Chapter Two, it is quite possible, and indeed normal, for physicists to use the formalism of QM to solve practical problems without having to grapple with the philosophical problems associated with the interpretation of the formalism. Most Soviet physicists, from the 1920's onwards, have followed this procedure, as have most Western physicists. There was no organised attempt by the Soviet state to cast doubt on the validity of the QM formalism. Indeed, physicists were actively encouraged to apply their science to economically and militarily important goals, most notably in the development of the atomic bomb. Thus the influence of the state on the development of modern physics in the USSR was, in this sense, similar to the influence exerted in Western countries. In both cases, technical development in specific (military) directions were encouraged, while at the same time, individual scientists could become politically suspect.[36]

However, there is one important difference between the Soviet case and that of Western countries. The <u>interpretation</u> of QM in the USSR became a subject of intense political concern. The vehicle by which this concern was expressed was the relationship between the the interpretation of QM and that of dialectical materialism (hereafter DM), the 'official philosophy' of the Communist Party.

Prior to World War Two, there was little official criticism of any interpretation of QM. According to Graham:

"Before World War II the views of Soviet physicists on QM were quite similar to those of advanced scientists everywhere. Russian physics was in many ways an extension of central and west European physics. The work of such men as Bohr and Heisenberg influenced scientists in the Soviet Union as it did everywhere. Indeed, such physicists spoke of the 'Russian branch' of the Copenhagen school."[37]

The first serious critique of the Copenhagen interpretation to be made in a Soviet physics journal was by Nikolskii in 1936[38]. He called this interpretation 'idealistic' and 'Machist'. However, this was an isolated incident, and other Soviet physicists, notably Fock, Blokhintsev, and Omelianovskii, continued to publish views which were very similar to those of the Copenhagen school throughout the period up to the war.

Some Western authors have argued that there are clear philosophical contradictions between QM (by which they mean the Copenhagen interpretation of QM) and DM. (As we shall see later in this chapter, some Western physicists have gone on to argue that DM, and therefore Marxism, has thereby been refuted.) Such authors find it difficult to account for the apparently peaceful coexistence of QM and DM in the pre-war period. For example, Brush writes:

As one might have expected, the subjectivist flavour of [the Copenhagen Interpretation] aroused the suspicions of dogmatic Marxists; what is surprising is not that the Copenhagen Interpretation was criticized as being contrary to dialectical materialism but that Soviet physicists were allowed to accept it without much political interference in the 1930's."[39]

In fact, there were a number of good reasons for this lack of political interference. In the first place, just as it is possible to disagree over the interpretation of QM, so there were also disputes among dialectical materialists, both over the proper role of DM within science, and also over the content of the officially-approved version of DM. These issues were not settled until the early 1930's.[40]

Secondly, especially in the early years after the revolution, the Party was reluctant to exert pressure on highly-qualified scientific and technical personnel in order to convert them to Marxism. They were too useful in their technical capacities to be hampered by ideological investigation. Even as late as 1931, Stalin was counselling Party officials in these terms:

"It would be stupid and senseless now to regard just about every specialist and engineer of the old school as an undetected criminal wrecker. We have always considered hostility to specialists a harmful and shameful thing and we still do."[41]

Indeed, even by 1932, less than 2% of Soviet physicists (excluding graduate students) were members of the Communist Party.

A possible third reason for the lack of political interference is the complete irrelevance of interpretive issues for the application of QM to practical problems. As in the West, few physicists took any active interest in interpretation. Even at the height of the Cold War, relatively few physicists were attacked because of their views on QM. In the pre-war period, most of the physicists who did comment on the relationship between QM and DM were content to argue that there need be no contradiction between adhering to a causal philosophy (DM) while using an apparently acausal theory (QM), because QM could be treated as a temporary first step towards a causal, non-statistical theory. Comparing this argument to more elaborate defences of QM, Joravsky notes

" This argument, a much simpler method of having the cake of QM without paying the penny of causality, was and remains very popular among Soviet Marxists."[42]

The period after the Second World War was largely dominated by the politics of the Cold War, which led, on both sides of the Iron Curtain, to rigorous attempts to 'purify' intellectual activity by making it consistent with the dominant ideology. In the Soviet Union, this process gradually abated after the death of Stalin in 1953. It is possible to trace the effects of these internal policy changes on Soviet physicists' interpretations of QM.

The 'ideological purge' which began in the late 1940's is sometimes known as the Zhdanovshchina, after A.A.Zhdanov, who was appointed Minister of Culture by Stalin in 1947. In a speech made in June, 1947, Zhdanov had this to say about QM:

"The Kantian vagaries of modern bourgeois atomic physicists lead them to inferences about the electron's possessing 'free will', to attempts to describe matter as only a certain conjunction of waves, and to other devilish tricks. What nation other than ours, the country of victorious Marxism, and our philosophers, should have the privilege to lead the fight against the corrupt and vile bourgeois ideology? Who else than we should deliver to it the fatal blows?"[43]

Despite this strong hint of a tightening-up of official attitudes towards the interpretation of QM, M.A.Markov wrote a paper in 1948 which was strongly in favour of the Copenhagen interpretation. The paper was published in the second issue of a new journal, Voprosy Filosofii (Problems of Philosophy). Soon after, a polemical attack on Markov's position was made by A.A.Maksimov. A number of physicists, together with B.M.Kedrov, the editor of Voprosy Filosofii, gave their support to Markov's position, and pointed out that Maksimov's critique contained a number of technical errors. The technical issues were, however, quickly overshadowed by the dismissal of Kedrov, together with five members of Voprosy's editorial board, at the Party's behest.[44]

Kedrov made a public apology:

"The root of my errors is that I violated the Leninist principle of party spirit in philosophy and deviated towards bourgeois objectivism and apoliticism."[45]

As Graham's account makes clear, the 'Markov affair' had as much to do with a power struggle among professional philosophers as with the interpretation of QM, but it certainly left physicists in no doubt that certain views would not be tolerated. As Zhdanov himself put it:

"Bohr's point of view is a product which is not viable in the least. It is refuse which, as Lenin said, must be thrown down the drain."[46]

The Markov affair constituted the most extreme case of political interference in the interpretation of QM. It soon became clear that the professional ideologists, such as Maksimov, were not competent enough in physics to produce alternative interpretations which were officially acceptable yet also technically self-consistent. The task of providing an approved interpretation therefore reverted back to the physicists and philosophers of science who were now much more aware of the demands of 'party spirit'. It may therefore come as no surprise to find that their views after the Markov affair were often different from their earlier positions on QM.

A common view among Western critics of the Soviet Union is that scientists' attempts to conform to DM during the years of the 'ideological purge' were hypocritical, if understandable, moves designed to ensure their professional survival. Scientists' 'true' beliefs, which could only be expressed after the liberalization which followed the death of Stalin, were claimed to be more in line with the 'politically neutral' beliefs of Western physicists. For example, Born said:

"I venture to presume that the opinions of leading Soviet physicists are quite similar to ours.... they merely make the concession to official philosophy by describing the new ideas as a dialectical development of materialism."[47]

and Heisenberg said:

"It can scarcely be avoided that the narrowness of the doctrines of DM is felt by those who have really understood modern physics and its philosophical meaning."[48]

There is certainly <u>some</u> evidence in favour of this claim. Take, for example, the physicist V.A.Fock. He was a member of the pre-war 'Russian branch' of the Copenhagen school, and defended Bohr's viewpoint throughout the 1930's, arguing that complementarity was an 'integral part of QM' and 'a firmly established objectively existing law of nature.'[49] After the war, complementarity became officially unacceptable, yet as late as 1949 Fock continued to defend it. In that year, he came under strong attack, notably in a book edited by Maksimov, which contained a paper by Omelianovskii which stated:

"Unfortunately, several of our scientists....have not yet drawn the necessary conclusions from the criticism to which Soviet science subjected the Copenhagen school. For example, V.A.Fock in his earlier works did not essentially distinguish the uncertainty relationship from Bohr's principle of complementarity."[50]

Following this criticism, Fock temporarily ceased to advocate complementarity. He criticised Bohr's tendency to extend this principle into other areas such as biology, noting that

"to the extent that the term 'principle of complementarity' has lost its original meaning....it would be better to abandon it."[51]

Fock also made some substantive changes to his interpretation of QM. For example, he changed his definition of the wave function ψ from being a measure of our information about microsystems to being an objective description of microsystems.

In the late 1950's, as political pressures eased, Fock began to discuss complementarity once more. He also visited Bohr in Copenhagen in 1957. Fock claimed, though this is not universally accepted,[52] that Bohr modified his interpretation during this visit. On his return to the Soviet Union, Fock attended a conference at which he drew a strong distinction between the 'physically true' part of Bohr's original ideas, and Bohr's original 'erroneous' philosophical position, namely, positivism. Now that Bohr had adopted a more objective philosophy, Fock claimed,

"it is therefore possible to agree completely with Bohr after this correction of his formulation, and the term 'Copenhagen School' should no longer be used in a pejorative sense."[53]

Fock's position soon became the dominant one in Soviet physics. Bohr's 'rehabilitation' was finalised when he was invited to the Soviet Union in 1961. Soon after his death in 1962, a commemorative issue of a leading journal was dedicated to him. His work was highly praised. Fock's account of Bohr's change of view was now apparently officially accepted, since the leading article noted the existence of a historical struggle between materialistic and idealistic interpretations of Bohr's work, concluding that the work of Soviet physicists like Fock had helped to make the idealistic interpretation untenable.[54]

This account of Fock's changes in position seems to support the view of Western critics that DM exerted a wholly negative role in the post-war period, and that as soon as this external pressure was removed, Soviet physicists fell back more or less into line with the views of Western physicists. Similar changes in position by Omelianovskii and Blokhintsev are well documented.[55]

However, it is by no means certain that these responses were nothing but necessary acts of obeisance towards a stultifying monopolistic ideology. Certainly, Soviet physicists thought it wise to include references to DM in their work. But DM is a complex philosophy, and different physicists were able to select different elements of the official version of DM in order to justify different interpretations of QM. Blokhintsev, for example, developed a statistical interpretation during the 'purge' which differed markedly from the views of Fock. Bentley summarizes the flexibility of DM in the following terms:

"Adherence to the Stalinist frame of reference does not ....lead to identical interpretations of experimental results and thus identical new theories. In fact, the doctrines [of DM] are not necessarily always mutually consistent. Blokhintsev's first interpretation neglected Stalin's law of universal connections, and his attempt to rectify this resulted in [his neglecting] Lenin's doctrine of objective reality. Fock's later interpretation stretched the meaning of this doctrine, and the Terletsky school (the Soviet equivalent of the Bohm school) neglected the concept of levels. DM in Soviet science is thus not a kind of 'mechanical' replacement for creativity as a number of Western writers seem to assume."[56]

DM, then, as part of the cultural context, provided a range of models, principles, and other resources which physicists could select and adapt while constructing their interpretations. Given the importance of DM in the cultural context, it is not surprising to find that it is also used critically; for example, one of the ways in which Fock criticised Blokhintsev's interpretation was to describe it as undialectical:

"A purely statistical point of view is incorrect in a philosophic sense. In contrast with what DM teaches us, the statistical point of view issues not from the objects of nature but from observations ....This draws it toward the positivist view of Bohr."[57]

Blokhintsev's response was to criticise Fock's own definition of the wave function by claiming it was not consistent with a realist or materialist view.[58]

Making the claim that an opponent's views are in conflict with the tenets of DM is not simply making a statement of fact. The former involves a process of selection, and the active construction of an account which is designed to be rhetorically useful. In a sense, such claims and counter-claims are very similar to the rather routine rhetorical exchanges found in any scientific dispute. Western interpreters of QM engaged in similar exchanges, though here the key terms were not 'undialectical' or 'positivist' but 'contrary to good scientific methodology', 'unreasonable', and so on. [59] Of course, in the Soviet case, the consequences of losing the battle for the 'official' definition of reality could be more serious, although with the exception of Kedrov physicists do not seem to have suffered greatly for their stance on the interpretation of QM.

Thus, it may have been necessary for Soviet physicists to make some response to DM during the 'purge'; however, since this response was an active, and indeed a creative, process, it certainly did not lead to uniform conformity. Yet critics might argue that all the interpretations produced during the 'purge' were similarly tainted and distorted by having to incorporate an alien philosophy which was irrelevant for the proper development of science. In other words, it could be argued that 'good methodology', and even 'reasonableness' are concepts with some heuristic value, whereas the concepts of DM have none. Yet some Soviet physicists have continued to defend DM and its role in their work, even at times when there was little pressure for such defences from ideologists.

For example, Blokhintsev continued to defend his statistical interpretation as late as 1966[60], at the same time retaining the association between his interpretation of QM and his reading of DM, quoting Engels in his support. Blokhintsev also attacked Bohr's philosophy, which

"has been the origin of the far-reaching conclusion that the current mechanics of the atom cannot be compatible with materialism."[61]

Fock also produced a defence of DM, long after the time when such statements could be considered compulsory:

"Dialectics plays an essential role in obtaining new outlooks on the external world and the appropriate ways of description."[62]

It is not only in the case of QM that we find Soviet scientists defending the use of DM as a heuristic device. The mathematician A.D. Aleksandrov has claimed that Marxist philosophy strongly influenced his work in mathematics.[63]

Graham argues that since the use of DM in science is now optional, we should take seriously the arguments of those Soviet scientists who continue to use it. He concludes:

"I am convinced ....that quite a few prominent Soviet scientists believe that DM is a helpful approach to the study of nature. They have examined many of the same problems of the interpretation of nature that philosophers and scientists in other countries and periods have also examined, and they have slowly developed and refined a philosophy of nature that would almost certainly continue to survive and evolve even if it were no longer propped up by the Communist Party. It is a philosophy of nature that is tied very closely to science itself, and it now depends much more on science for sustenance than on Party ideology."[64]

Of course, we are not obliged to accept this conclusion, since it is possible that, even in relatively liberal periods, it can do a Soviet scientist no harm to continue to be seen to support DM, and it may even be to his advantage.[65] We should also bear in mind the general methodological problem of interpreting actors' accounts of their actions.[66] Nevertheless, since not all Soviet scientists continue to refer to DM, there does seem to be an element of choice, which is hard to reconcile with the view that DM was and is nothing but a means of enforcing conformity.

It is certainly not the case that all Soviet physicists are 'really' in favour of the Copenhagen view, despite the claims to this effect by Born and Heisenberg, quoted earlier. Since the 1950's, Soviet physicists, like those in the West, have examined a number of alternative interpretations, including statistical interpretations, hidden-variable theories, and unified field theories.[67] In retrospect, it seems clear that Western supporters of the Copenhagen Interpretation were being just as monopolistic as Soviet dialectical materialists in claiming that their own point of view was uniquely valid.[68]

Although authors such as Born were highly critical of attempts to incorporate philosophical elements such as DM into physics, they were not at all reluctant to draw inferences from QM and then apply them to a critique of DM. This will be the subject of the next section.

Complementarity and Marxism.

In an essay written in 1951, Max Born concluded, from a consideration of QM, that Marxist doctrines were incorrect. The argument involves complementarity, which, Born implies, is an empirically established principle:

"Science is not only the basis of technology but also the material for a sound philosophy. And the development of modern physics has enriched our thinking by a new principle of fundamental importance, the idea of complementarity."[69]

Born stresses the novelty of this idea, noting that

"Marxian philosophy, which is a hundred years old, knows of course nothing of this new principle."[70]

Because Marxism does not contain a complementarity principle, Bohr seems to be saying that it must be an incorrect view of reality. He claims that one of Marxism's central predictions - the eventual overthrow of capitalism - is false. For, by applying the principle of complementarity to the conflict between capitalism and communism, "one would expect a synthesis of some kind, instead of the Marxian doctrine of the complete and permanent victory of communism."[71]

This argument can be criticised in several different ways. To some critics, the fact that Born was using a physical theory to attack a political ideology is proof that Born's account is invalid; this is an illegitimate use of science which simply demonstrates Born's political bias. For example, the Soviet philosopher of science Omelianovskii criticised "modern reactionary bourgeois physicists....who, invoking Bohr and Heisenberg, 'liquidated materialism'."[72]

A similar case is made by two Italian physicists, Garuccio and Selleri[73] who write:

"QM could certainly not be neutral with respect to the greatest political conflict of those (and our) times, the struggle between Socialism and Capitalism. At least two of the main creators of QM, Born and Heisenberg, in fact saw in their theory a powerful tool to fight Marxism and to help the widening of the capitalist world."[74]

Nevertheless, although Born's motives may be political and therefore suspect, this does not prove that his arguments are invalid. However, we are not forced to conclude that Marxism has been disproved; Born's arguments can be, and have been criticised on substantive grounds.

For example, while Born argued that complementarity is a new principle, other supporters of the Copenhagen view have adopted a quite different position:

"This mode of cognition....was in fact perceived thousands of years ago by the philosophers of ancient India and China. In modern science, Niels Bohr has given it the name of complementarity."[75]

In Chapter Two, we saw that not all physicists accept the complementarity principle as an essential part of QM; instead of seeing it as a logical consequence of the formalism, they claim it is simply part of one interpretation of that formalism. Seen in this way, complementarity is far less threatening to Marxism than Born implies.

What is more, some physicists were able to accept both Marxism and complementarity. The most notable example is Rosenfeld, who worked in Copenhagen with Bohr. Although he was a Marxist, he felt that complementarity was a well-established principle:

"The conception of complementarity forces itself upon us with logical necessity. It is not some fanciful speculation which we could at will accept or reject according to whether we find it conformable to some philosophical criterion or other."[76]

Unlike some of the Soviet physicists discussed above, Rosenfeld claimed that the Copenhagen view was quite compatible with Marxism, though (as we have seen with interpretations of QM and of DM) there are different interpretations of what is meant by 'the Copenhagen view':

"It is a pity that the creators of the conception of complementarity have....sometimes expressed themselves in ambiguous or even frankly idealistic terms. But this ought not to disconcert us. Are we going to lay complementarity under an interdict because Heisenberg is an idealist? We might just as well condemn the Principia because Newton dishes up his dialectics in the guise of Puritan theology."[77]

Rosenfeld points out that Bohr himself did not use complementarity to criticise Marxism. Indeed, Rosenfeld argues that Bohr was, albeit unconsciously, something of a dialectical materialist himself:[78]

"Bohr is too subtle a dialectician to fall into the same inconsistency as Heisenberg....he experiences the dialectical movement of nature as a living reality with which he has completely identified his thought and his feelings. Naturally enough, dialectical relations take in his mind the shape of complementarity relations".[79]

Thus, the relationship between complementarity and Marxism, let alone the empirical status of these two concepts, is by no means obvious. There is disagreement over whether complementarity is an essential feature of QM; if this is rejected, the empirical success of QM says nothing about the validity of the complementarity principle. As we saw in the previous section, there is also disagreement over the detailed structure of Marxist philosophy. Finally, because of the vagueness of these concepts, it is by no means obvious that there is any necessary contradiction between the two. In the next few sections, I will discuss other cases where inferences drawn from QM have been applied to fields other than physics; in each case, a wide range of opinions can be found.

QM and Biology.

Although Bohr avoided political issues, he did try to extend complementarity into other areas, particularly biology. For example, he argued that in vitro and in vivo experiments were complementary: experiments which involved killing an organism, he claimed, necessarily neglected the special characteristics of complete, living organisms. In particular, they neglected the concept of purpose.

In a biographical article on Bohr, Rosenfeld points out that Bohr's father was a physiologist who had resisted Darwinian 'mechanistic materialism' and had defended the use of teleological arguments in biology.[80] Bohr himself also made statements in favour of teleology and vitalism, such as the following:

"Owing to this essential feature of complementarity, the concept of purpose, which is foreign to mechanical analysis, finds a certain field of application in biology."[81]

and elsewhere

"the existence of life must be considered as an elementary fact that cannot be explained, but must be taken as a starting point in biology, in a similar way as the quantum of action, which appears as an irrational element from the point of view of classical mechanical physics, forms the foundation of atomic physics."[82]

If life is to be made exempt from causal mechanical explanation, then, a fortiori, so is human consciousness and free will. Bohr writes:

"the freedom of the will is to be considered as a feature of conscious life which corresponds to functions of the organism that not only evade a causal mechanical description but resist even a physical analysis."[83]

Born agrees with this view:

"If even in inanimate nature the physicist comes up against limits at which strict causal connection ceases and must be replaced by statistics, we shall be prepared, in the realm of living things, and emphatically so in the processes connected with consciousness and will, to meet insurmountable barriers, where mechanistic explanation, the goal of the older natural philosophy, becomes entirely meaningless."[84]

These arguments have been criticised along the same lines as those we encountered in the last section. To Marxist critics, statements like the above are clearly ideologically motivated, and therefore invalid:

"Born joined Bohr and Heisenberg in showing how important the quantum mechanical philosophy could be for granting space in other sciences to idealistic conceptions."[85]

Levy-Leblond, a French Marxist physicist, makes a similar point:

"The alleged 'crisis of determinism' which quantum physics brought about (in fact it was a modification of the forms of physical causality) opened the gates to a flood of philosophical, ideological and even political lucubrations....In brief, one has witnessed a true ideological exploitation of modern physics."[86]

In addition, we can once again find a wide variety of views both on the interpretation of QM and on the implications of QM for biology and psychology. Rosenfeld, for example, accepted the validity of complementarity but felt that

"it would be premature to assert that we shall be able to fit the dialectics of life or consciousness to such a framework."[87]

Wigner, whose 'consciousness' interpretation of QM was discussed in Chapter Two, clearly shares the view of Bohr and Born that human consciousness is a unique feature in nature. However, whereas Bohr and Born go on to argue that consciousness cannot be described in causal or even rational terms, Wigner claims that we should modify the formalism of QM in order to produce a precise physical description of the interaction between the observer's mind and the external world.[88]

To quote a final example, the Nobel prize-winning biologist Monod has applied QM to biology in order to argue <u>against</u> vitalism. In his book, 'Chance and Necessity', Monod points to the essentially random nature of genetic mutation:

"Pure chance, absolutely free but blind, [lies] at the very root of the stupendous edifice of evolution....there is no scientific position in any of the sciences more destructive of anthropomorphism than this one."[89]

QM, according to Monod, provides a supplementary argument in favour of this view:

"Finally, on the microscopic level, there exists a source of even more radical uncertainty, embedded in the quantum structure of matter. A mutation is in itself....a quantum event, to which the principle of uncertainty consequently applies."[90]

Monod has a rather sophisticated attitude towards the use of QM in such arguments. For example, he notes that there is some difference of opinion over the interpretation of QM:

"The principle of uncertainty was never entirely accepted by some of the greatest modern physicists, including Einstein....Certain schools have....denied it the standing of an essential concept."[91]

Monod therefore dissociates his general arguments about randomness from his particular use of QM:

"It must be stressed that, even were the principle of uncertainty some day abandoned, it would remain true that [in examining genetic mutations] one could still see nothing but an 'absolute coincidence'."[92]

Thus, in Monod's case, QM is used to provide an illustration, or a supplementary argument, rather than a 'proof' of his case. Levy-Leblond takes a less charitable view; in a critique of Monod's book, he writes:

"How can one admit more clearly that quantum physics has intrinsically nothing to do [with] the discussion and is an authority cited merely to buttress an argument?"[93]

A study of the extension of QM into biology allows us to extend the findings of previous sections. Not only are we faced with conflicting interpretations of QM, but even within a single interpretation (the Copenhagen view discussed by Bohr, Born and Monod) we find there are separate features which are, in a sense, 'complementary'. Thus, the complementarity principle, as employed by Bohr and Born, is used to point to one conclusion, while the uncertainty principle is used by Monod to point to a diametrically opposite conclusion. Wigner's position makes it clear that 'complementary' is an adjective which can be bandied about rather freely to describe any pair of concepts which seem to describe the same thing in different ways; this is very far from a proof that the two modes of description are mutually exclusive. Complementarity has been applied in several other contexts in this rather superficial way.[94] However, it should not be supposed that these differences of opinion simply reflect the vagueness of the concept of complementarity. Other features of QM, notably the loss of determinism, have also generated conflicting views. The next section deals with such an example.

God and Determinism.

It can be argued that the loss of complete causality is incompatible with the idea of an omniscient, omnipotent God. For some authors, the great advantage of a deterministic hidden-variable interpretation of QM is that it would restore a role for such a God. This viewpoint has been expressed by Rietdijk:

"I feel that the only possibility that [the] universe in general, and human life in particular, might have a deeper sense of purpose, consists in that the entire history of the universe, of mankind and of every individual, is proceeding according to a great design, however opaque it may be for us. An indispensable condition for the possibility that such a design might exist is that, indeed, Einstein was right in saying 'Der Herrgott würfelt nicht' (God doesn't gamble). If God did, if indeterminism and chance did exist as current QM maintains, life and the world would be a lottery. Therefore, our only hope of survival, in the deepest meaning of the word, the only hope of the truly religious man, has to be set on determinism, on hidden variables."[95]

Belinfante, on the other hand, points to the completely opposite view:

"there are those who for religious reasons do not want to believe in determinism. For them it is not a priori obvious that all happenings in the universe should be ultimately explainable by human reasoning. Anything happening in nature may in one way or another be a manifestation of God acting in this world, but especially those happenings that defy explanations can be 'understood' merely as acts of God. Thus the indeterminacy in atomic happenings as predicted by the quantum theory is a manifestation of the omnipresence and omnipotence of God. The lack of determinism in quantum theory, to these people, is not only acceptable, but is a reassurance of their religious beliefs. If nature were fully deterministic, there would be no place in it for a God with any freedom to act; the 'equations of motion' would determine everything."[96]

It is hardly surprising that the concepts of 'God' and 'determinism' should be complex enough to sustain contradictory 'deductions' about the implications of QM for religion. Partly because of this ambiguity, some authors have argued that we should refrain from drawing any strong theological conclusions from the apparent demise of determinism brought about by QM.

Mackay, for example, follows Monod's example, by pointing out that some physicists do not agree that indeterminism necessarily follows from QM:

"Although most physicists today speak of atomic events as 'indeterminate', there have been those, including the great Einstein himself, who refused to concede that they were anything more than unpredictable-by-us."[97]

Mackay also points to the flexibility of the concept of indeterminism, drawing a distinction between the apparent indeterminism of microsystems with the deterministic behaviour of many macrosystems:

"Despite Heisenberg's revolutionary principle, we all know that clocks keep quite good time, the sun continues to rise relatively predictably, and other things that we depend on like boiling kettles continue to be reliable."[98]

This view is of course quite consistent with QM, as Rosenfeld points out:

"Determinism....is perfectly adapted to the description of phenomena on the macroscopic scale; its validity in that field is of course not in question: there determinism reigns as supreme as ever."[99]

For these reasons, Mackay urges Christians who wish to defend the concept of free will

"not to waste their ammunition on <u>physical</u> determinism, even of the 'softer' variety which has come in with the Heisenberg uncertainty principle."[100]

Barbour, in his comprehensive study of the relationship between science and religion, is similarly sceptical about the implications of QM for the concept of freedom. First, he points out the wide variety of opinions among physicists concerning the nature of indeterminism in QM. Next, he argues that randomness in the behaviour of individual atoms, even in the brain, has no obvious causal links with the consciousness of human beings. Finally, he draws a clear distinction between 'randomness' and 'freedom'.[101]

The evidence presented in the last few sections strongly suggests that QM does not contain a set of clear, unequivocal implications for other areas of belief. Instead, QM provides a set of concepts, models, and other cognitive resources, which can be employed by an actor in support of his views on biology, politics, and so on. Moreover, the resources provided by QM are sufficiently rich and varied that they can be used to 'support' a wide variety of views - indeed, diametrically opposite views.

QM is by no means unique in being used to support conflicting views in a wider context. For example, Rosenberg found that the 19th century theory of the hereditary nature of intelligence and morality was used, in different contexts, to support arguments both for environmental improvement and compulsory sterilisation of the 'unfit':

"Ostensibly scientific formulations have found quite different social roles in different national contexts....social thinkers.... had selected those scientific plausibilities which fitted most conveniently into their social need and presuppositions."[102]

Existing studies of the rhetorical use of science have not only drawn attention to the flexibility with which theories can be interpreted, but also to the perceived high status of science within our culture, which presumably leads to the widespread use of science as a rhetorical resource.[103] The evidence presented in this chapter corroborates these conclusions, and may be particularly useful in pointing out that modern physics, despite its esoteric and highly technical nature, is by no means immune from this process.[104]

The Validity of Rhetoric: QM and Parapsychology.

Having examined a number of cases in which QM was applied to other fields, what conclusions can we draw about the 'validity' of such applications? Marxist critics, such as Levy-Leblond, Garuccio and Selleri, argue that, because these applications are motivated by ideological or philosophical prejudices, they are <u>all</u> invalid. As we shall see in Chapter Four, scientists who study the interpretation of QM often admit that they use philosophical or other non-empirical criteria in their assessment of ideas. Yet they do not feel that their work is invalid because of this.

One alternative is to adopt a symmetrical view to all applications of QM, describing them all as examples of the use of cultural resources for rhetorical purposes. Later, I shall argue in favour of this view.

There is, however, another possibility, namely that some applications are more valid than others, and that there are criteria of validity by which such applications can be assessed. Before arguing in favour of the 'symmetrical' view, I shall first argue that this third possibility is not viable.

The best way to investigate this question is to examine an extreme case, where a particular application of QM seems especially suspect. Parapsychology provides us with several such examples, in which QM has been used in what seems an 'illegitimate' way.

In a series of well-known books, Carlos Castaneda has described the beliefs of the Yaqui Indian sorcerer Don Juan.[105] Don Juan claims that there are many realities, and that a skilled sorcerer can jump from one to another. In his book 'Psi and the Consciousness Explosion', Holroyd uses the Everett 'many-worlds' interpretation of QM to support this model of reality. Holroyd writes:

"Modern theoretical physicists are telling us that there may exist - and they mean really, not figuratively - many worlds. Considered in the light of the theories of....DeWitt and Graham....don Juan's statement that 'your car was not in that world' is simply a statement of fact."[106]

Holroyd may be forgiven for failing to point out that the many-worlds view is not 'orthodox' physics. However, what is more difficult to ignore is the fact that, according to the authors of the many-worlds view, transitions between worlds are impossible, so that the theory cannot account for the appearance or disappearance of sorcerers or cars. DeWitt is very clear on this point:

"The laws of QM do not allow us to find ourselves split....We, who inhabit only one of these worlds....have no access to the other worlds."[107]

Indeed, the inaccessibility of all the other universes, and therefore the lack of any possible empirical evidence for their existence, is seen by many critics to be one of the main flaws of the many-worlds theory.

Thus, Holroyd is using this theory to support a hypothesis which the authors of this theory claim to have ruled out. Does this not constitute an 'invalid' use of a theory? Since DeWitt has not responded directly to Holroyd's claims, it remains a possibility (though perhaps an unlikely one) that they might have been able to negotiate some sort of compromise. However, even this slim chance is absent in the next example, since the physicists concerned have explicitly rejected any parapsychological interpretation of their theory.

This example concerns the claims made by Jack Sarfatti, a rather colourful character who was trained as a physicist but is now closely involved in parapsychology organisations in California. He draws on elements of cosmology, relativity theory and QM to justify his viewpoint. For example, referring to the (conventional) QM concept of zero-point motion (that is, a vibration of atoms which persists even at absolute zero), Sarfatti writes:

"The assumption here is that the zero-point motion is not random as now assumed but contains sacred messages of the Hidden Wisdom that filter through to our profane limited understanding at the molecular level. Is this the mechanism of Divine Understanding?"[108]

One of the sources for Sarfatti's speculations has been the hidden-variable theories of David Bohm and his co-workers. Bohm and his colleague Hiley took exception to Sarfatti's use of their work:

"By taking these quotations out of context, he [Sarfatti] has created the impression that our work directly relates to his own conjectures regarding the observer-participator. The actual situation is very different."[109]

Another physicist, Stapp, also felt that Sarfatti had misrepresented him, and wrote to Sarfatti:

"You have quoted passages from papers of mine in a way that suggests that your ideas .....are in accordance with quantum theory. I think I should set you straight."[110]

Some of the claims made by Sarfatti and his colleagues might be charitably interpreted as rhetorical exaggeration, such as the following comment, made by Herbert, on Bell's theorem which concerns the non-locality of QM:

"If once-interacting objects are truly forever inseparable, then when you get out of the bathtub, you, in some sense, never really leave it....Bell's theorem gives precise physical content to the motto 'we are all one'."[111]

However, although Sarfatti and his colleagues could defend their style of writing in terms of a desire for dramatic effect, there remains the fact that they quote physicists in support of their views, when these physicists explicitly reject the claim that their work is consistent with Sarfatti's.[112]

Nevertheless, it does not automatically follow from this that Sarfatti's claims about physics, as opposed to physicists, are false. Obviously, Bohm may claim that Sarfatti falsely accuses him of wishing to introduce consciousness into physics, and it is difficult to refute any claim which Bohm may care to make about his own motives. Equally, it is difficult to validate such a claim. Moreover, one can argue, as indeed Sarfatti does, that Bohm's theory supports the concept of consciousness in physics, even if Bohm himself dislikes this argument:

"[Excluding consciousness] is the classical view of physics which Bohm and Hiley seem to want to retain. It is a view that we must respect and admire, even if we cannot subscribe to it."[113]

Presumably, Holroyd could, if he wished, argue on similar lines, claiming that the many-worlds view is a partial account of don Juan's abilities, but that the theory requires modification to allow for the possibility of transitions.

There is a further example in which the apparent falsity of parapsychologists' statements about physics seems hard to deny. In his book 'Space-Time and Beyond', Toben discusses the implications of the 'new physics' - QM, relativity, black holes, and so on - for parapsychology. He writes:

"All over the world, phenomena are occurring that cannot be explained within existing belief systems....However, if we properly [sic] interpret some of the existing accepted [sic] scientific theory, we find that explanations do exist."[114]

As in the case of Holroyd, the many-worlds view is cited with no indication of its marginal status. A more glaring error occurs in Toben's treatment of the experimental tests of local hidden-variable theories. He cites the experiment of "A.Holt" (actually Richard A. Holt) and claims that Holt's result indicates

"that the quantum potential (hidden variable) interpretation of quantum theory due to de Broglie and Bohm agrees more closely with experiment than does the conventional interpretation, which denies the existence of hidden variables."[115]

Toben seems to be unaware of Freedman and Clauser's experiment, whose results were published before Holt's experiment was completed, and which gave completely the opposite result. More seriously, Toben seems to have little grasp of the intricacies of hidden-variable theories. As discussed in Chapter Two, Bohm and de Broglie produced non-local theories, whose predictions agreed with those of QM for these experiments. Holt's results conflicted with these predictions, so that they support local theories and oppose Bohm's theories. (As we shall see in Chapter Five, even some physicists misunderstood the implications of Bell's theorem for Bohm's theories.)

In principle, Toben could argue that the apparent conflict between Holt's results and Bohm's theory is due to an error in our interpretation of one or both. However, this would involve a far more comprehensive rewrite of conventional wisdom than in the example above, in which Sarfatti presented his conflict with Bohm as a simple difference of opinion about the role of consciousness in physics. Certainly, drawing conclusions from experimental results is by no means unproblematic, as we shall see in Chapter Six, but it is difficult to see how Holt's results could be used to support Bohm's theory.

Thus, there seems to be a minimal sense in which the concept of 'error' is meaningful when discussing the application of physics into other fields of inquiry. However, such errors are wholly peripheral to the more important question with which this section deals - namely, the assessment of the validity of such extensions of physics. A criterion of 'validity' based on a search for factual errors is of little use in assessing many of the applications of QM into biology, politics and religion discussed earlier; such errors are the exception, and not the rule. Factual errors may indeed be a sign of incompetence, but it is a form of gross incompetence - like claiming that '2 and 2 make 5' - which no decent rhetorician would be guilty of.

Must we therefore conclude that any rhetorical usage of QM is as legitimate as any other? One final alternative has been suggested by Bedau. Referring to rhetorical uses of complementarity, Bedau concedes that the popularity of such usages bears little relation to the rigour of the arguments involved, nor to the extent to which the rhetoric accurately describes complementarity as the concept is used in QM. He argues that the term 'complementarity' is rapidly becoming meaningless. Wishing to avoid semantic confusion, he applies a sort of copyright rule: a 'correct' use of the term is one which matches Bohr's own usage:

"Is the would-be complementarist to be allowed to mean by complementarity whatever he pleases? Obviously not....What is left, then, but a tacit reliance upon the concept....as employed by Bohr, minus whatever is peculiar to its application in QM? But if tacit reliance, why not explicit?"[116]

Bedau's views are cited here not because they are particularly well-known or interesting, but because they usefully characterise a concept of meaning which the evidence presented in this chapter seriously weakens. Apart from the fact that Bedau's rule would be pointless unless everyone agreed to be bound by it, it would in any case be virtually impossible to apply the rule. For one thing, Bohr's concept of complementarity was developed over a number of years, so we must first decide which version of complementarity to use as our benchmark. More importantly, different physicists have understood Bohr to be saying very different things. For example, Einstein wrote:

"Despite much effort which I have expended on it, I have been unable to achieve a sharp formulation of Bohr's principle of complementarity."[117]

Another physicist, von Weizsäcker, while writing a review paper on the subject in 1955, re-read Bohr's early papers on complementarity and

"came to the conclusion that for over 25 years he had misinterpreted Bohr's notion...., the real meaning of which he now thought he had discovered. But when he asked Bohr whether his [new] interpretation... accurately presented what Bohr had in mind, Bohr gave him a definitely negative answer."[118]

Note that this ambiguity represents only doubt over what Bohr meant by complementarity; we have already seen the wide range of opinions held by other people about complementarity. It would seem that Bedau has failed to give us a reliable criterion for discriminating between valid and invalid extensions of QM.

Barbour provides a more satisfactory treatment of the problem. Like Bedau, he refers to the use of complementarity in physics in order to indicate how, if at all, the term should be used in other contexts. For example, in physics the term refers to different ways of analysing the same entity, such as an electron, in different circumstances. This excludes certain extensions of the term, such as the claim that science and religion are complementary, because "God and the world are different modes of being, not different modes of knowing a single being."[119]

As in the case of Bedau, such claims about what the term 'really means' in physics are open to question, but Barbour goes further; unlike Bedau, he does not feel that physicists' <u>own</u> views of what complementarity means should have primacy. For example, he argues that we need not accept Bohr's renunciation of ontological questions. He also notes the existence of alternative 'principles' in science, such as the search for unity:

"complementarity provides no justification for premature and uncritical acceptance of dichotomies."[120]

Most importantly, Barbour recognises that

"use of the Complementarity Principle outside physics is <u>analogical not inferential</u>. There must be independent grounds for justifying in the new context the value of two alternative sets of constructs."[121]

If we view concepts such as complementarity, determinism and causality as flexible resources which can be applied analogically or metaphorically in different contexts, we avoid a number of pseudo-problems such as the difficulty of defining 'valid' and 'invalid' uses, and the problem of accounting for the very wide variety of uses to which the concepts of QM have been put. <u>None</u> of these extensions emerges 'naturally' from QM; in each case, authors had to actively select particular features of the theory and neglect others, and then construct an account employing the chosen features.

Certainly, for a particular observer, some such accounts seem more persuasive than others. Accounts which contain factually incorrect statements may seem particularly unconvincing, provided of course that one is aware of the error. Quite often, the account is aimed at an audience which does not have such a technical background, and the fact that an account is 'really' incorrect is neither here nor there if the error remains undetected. (Besides, students of the mass media are well aware of the extent to which an audience's prejudices are resistant to any amount of documentary evidence and logical arguments.) Thus, although we could discriminate between extensions of QM by means of a criterion of 'persuasiveness', this would not only be subjective and transitory, but would also bear very little connection with more absolute notions such as 'validity'.

Discussion.

In the first part of this chapter, I showed how scientists in two very different cultural contexts presented their work in such a way as to make it acceptable to the demands of their particular milieu. For this claim to be plausible, we must of course accept that their work could have been presented in other ways. This is not difficult in the case of Soviet QM, since many Western observers explicitly stated that Soviet scientists were presenting a distorted view of QM. In the case of Weimar physicists, there is no proof that an alternative causal theory was possible, but it seems clear, from the different reception of QM by German and British physicists, and from the existence of hidden-variable interpretations of QM, that the particular way in which QM was presented - as an irrevocably acausal theory - was only one of several options.

I also argued that the Western view of Soviet QM as deviant or pathological is questionable. It is by no means clear that the use of dialectical materialism as a heuristic philosophy of science is valueless, or indeed that it is any less valuable than alternative philosophies, such as Popper's, which are explicitly cited by many Western scientists.

Thus, in an important sense, the development of the interpretation of QM in these two contexts can be treated symmetrically. In both cases, scientists perceived, and successfully responded to, external social pressures originating in the general cultural milieu, and presented their work in an appropriate way.

Since we are dealing here only with the interpretation of QM - that is, whether or not the theory is acausal or dialectical - one could argue that the substantive details of the theory (the equations which make up the formalism) are immune from such social processes. Despite some of Forman's more extreme claims, there is little evidence to support the view that external cultural influences did affect QM's formalism. However, in Chapter Four I shall argue that socialization processes within science do indeed play an important role in determining the goals and methods which scientists adopt, and in

Chapter Six I shall argue that such processes also affect the substantive conclusions which scientists reach about the meaning of experimental results.

One benefit of the present chapter, then, is to improve the precision of the terms "social context" and "scientists' responses", by specifying different sorts of contexts and different sorts of responses.

In the second part of this chapter, we saw how QM was used to draw inferences about many different fields of inquiry. The aim was not to provide a comprehensive list of such usages, but to provide a wide enough range, particularly of conflicting examples, to give credence to the view that concepts such as complementarity, uncertainty and indeterminism, originating within QM, have no clear, necessary or unequivocal implications for other fields. Such usages, as Barbour put it, are analogical and not inferential. Each such application of QM involves creative processes of selection, assumption, and the construction of accounts. There is no useful criterion which can discriminate in a general way between 'valid' and 'invalid' applications of QM.

The evidence presented here suggests that we should adopt a symmetrical approach to all extensions of QM. However, one can go further: it also suggests that scientists' accounts of QM (QM is acausal, dialectical, and so on) are not qualitatively different from rhetorical uses of QM in other areas (QM refutes Marxism, supports vitalism, and so on).

Apart from the support it provides for the general concept of meaning described in Chapter One[122], such a model is also useful in increasing our awareness of the problematic nature of statements about what QM (or any other theory) is 'really' like. In Chapter Four, we shall encounter accounts which rely on unproblematic characterisations of Bohm's theory as 'dialectical' and of other proposals as 'arithmomorphic'. We can now approach such accounts with a suitable degree of scepticism.

Chapter Four

Social Organisation of FQM[1] and the Conduct of Disputes

Introduction

It should already be clear from the material presented in Chapter Two that, in cognitive terms, FQM is a rather fragmented field. A very large number of different reformulations of QM has been proposed. Virtually the only things held in common by FQM workers are the past success of QM's formalism and the inadequacy of the conventional interpretation of that formalism. There are marked stylistic and methodological differences between the various alternative interpretations which have been proposed; some are highly mathematical, some employ mainly intuitive physical arguments, and others argue on philosophical grounds.

My first aim in the present chapter is to examine the _social_ structure of FQM. I will try to show that, in sociological terms, FQM is not a typical scientific specialty[2], and that its social organisation, like its cognitive structure, is fragmented.

I shall then argue that this congruence is by no means coincidental; the fragmented social structure (and low status) of FQM is at least partly due to the absence of consensus over methodology and goals. Because FQM workers find it difficult to communicate and collaborate with each other, there is little opportunity for coherent social structures to develop. Conversely, the absence of a coherent social structure means that there is no 'official' training system for entry into FQM, and few generally-recognised sanctions; as a result, cognitive consensus is unlikely to be achieved.

In this analysis, I shall emphasise _methodological_ differences between different sorts of FQM workers, partly because it seems a useful way to classify this field, and partly because such an approach also provides a useful way of describing _disputes_ in FQM. Such an approach has been attempted by Pinch[3], in his analysis of the reception of Bohm's 1952 hidden variable theory. I shall examine Pinch's work, showing how his findings are consistent with a methodological analysis. However, I shall go on to argue that Pinch's analysis is not entirely satisfactory. Specifically, he invokes concepts such as 'arithmomorphism' and 'the authority structure of physics' which not only lack generality (in that they do not apply in other disputes) but are also, I would claim, misleading and unnecessary. I shall try to show that a more satisfactory account of Pinch's data can be obtained by considering that physicists are concerned primarily with practical issues ('doing physics'), which inevitably involves a concern with methodology.

As a way of expanding and supporting this analysis, I shall then examine another 'communication breakdown' in FQM. The Bohm-Bub hidden variable theory was subjected to an experimental test. I shall examine the reaction to this test of Bohm, Bub, the experimenter, and a mathematician. Here again I shall argue that what seems to be a cognitive dispute over the status of a theory and an experiment is, fundamentally, a dispute over the choice of methodology.

The general implication of this analysis, which will be dealt with in more detail in my final chapter, is that when we attempt to account for the existence, conduct, or outcome of a debate in FQM (and perhaps in other sciences as well), we must take careful note of the social and technical context of the debate; scientists who do not share a common methodological orientation are unlikely to perceive the theory or experiment at the centre of the debate in quite the same way.

The status of FQM within physics

Let us begin by considering whether FQM constitutes a specialty within physics in the way that, say, solid state and high energy physics do. In institutional terms, the answer is clearly 'no'. FQM does not appear as an organisational subgroup within physics; for example, there are no FQM sub-committees on funding bodies, and no professional bodies to regulate this activity or organise regular conferences. In cognitive terms, FQM also lacks a clear identity; for example, it has no separate classification within Science Abstracts, nor are there any journals which are universally recognised as the appropriate forum for this work.[4]

In a wide-ranging study of the social organisation of science, Hagstrom makes a passing reference to FQM:

"Most of the topics I had judged to be controversial issues in science ....were defined as 'philosophical issues' by my informants, not scientific issues....Physicists sometimes react in this fashion to attempts to renew interest in deterministic interpretations of quantum theory....none of the physicists interviewed spontaneously mentioned this issue as an important unresolved issue when asked if they could think of any such issues."[5]

Also, Hanson writes:

"Physicists remain somewhat indifferent to issues of the Bohr versus Bohm-Feyerabend variety. Such issues they regard as ill-founded in the absence of any solid mathematical foundation to serve as the basis for discussions of 'interpretations'."[6]

The marginal status of FQM within physics was confirmed by many of my interviewees:

"That was one real drawback about this field, that it isn't really a field. It's not like high energy physics, there's a high energy physics group in the majority of physics departments, and there's a solid state group, and this doesn't really slot into any one of these."[7]

Not only is FQM not perceived as a standard cognitive sub-division of physics, but it also lacks many of the organisational features of such sub-divisions. The absence of professional bodies, and the scarcity of conferences, have already been mentioned. In addition, replies to a questionnaire[8] suggest that FQM has no organised recruitment pattern. Less than half the people who work in FQM have been students of anyone who was involved in this field, and many members of this minority claimed that their own interest in FQM arose for other reasons and that their approach was very different from that of their former teacher.[9] Most people claimed to have become interested in FQM after reading books or articles, rather than from talking to anyone they knew personally, and an interest in FQM seems to be just as likely to develop when a physicist is well-advanced in his career as it is when he is a student. In addition, very few respondents spent a major part of their time on FQM; for the majority of people, it seems to be an activity which takes up less than half of the time available for research.

Another feature of a scientific subgroup is the existence of journals which are widely perceived as the appropriate forum for discussion of the subgroup's content. FQM workers, when asked to indicate the journals which they most associated with work in FQM, listed a wide variety of journals with no clear consensus over which sources were central to this field. In many cases, FQM respondents listed general physics journals such as the American Journal of Physics, and Nuovo Cimento. 'Hard' physics journals (that is, those specialising in well-established fields with mainly empirical, quantitative papers), such as Physical Review, were only cited by the group of physicists who performed experimental tests of local hidden variable theories.

The only journal of standard form which might conceivably be regarded as a central forum of debate in FQM, and which restricts itself to 'fundamental' questions, is Foundations of Physics.[10] However, many FQM workers claimed that, despite its wide scope, this journal did not seem to them to represent FQM in its entirety. Some informants claimed that this journal specialised in the more speculative side of FQM, and that its contents were of dubious quality. However, despite these reservations, Foundations of Physics at least has the advantage of being included in Science Citation Index publications, so that it may provide us with a further sort of evidence about the status of FQM. Let us examine this evidence.

Citation Analysis of FQM

The limitations of citation analysis are well-known[11], but if handled with care, citation data may be of some assistance in checking conclusions about the status of a field gained by other, less quantitative means. Here I shall restrict myself to evidence about the status of the journal Foundations of Physics, using the Science Citation Index publication, Journal Citation Index. This index provides data on citations to and from any particular journal.

The first section of the index lists the 'Impact Factor' for various journals; this is defined as the ratio of actual citations of a given journal in a given year, to the total number of citable items published in that journal in that year. A large Impact Factor for a journal means that much of its content was cited within a year of publication, and it is assumed that this implies that the journal's contents had a strong impact on the scientific community. This assumption can be questioned, by arguing that articles can be cited for all sorts of reasons, other than the articles' quality[12]. However, let us examine the Impact Factors for a number of journals, as shown in Table 1. (Note that an Impact Factor greater than unity means that, on average, each article was cited more than once within a year of publication.)

The low Impact Factor of the _American Journal of Physics_ does not, I think, accurately reflect its status; rather, it is a result of its rather special content. This journal publishes a large number of relatively short articles and letters, many of which are not intended to be original research, but rather historical reviews, accounts of school and college physics courses, and suggested experiments for demonstrating the laws of physics.

Despite the obvious shortcomings of the Impact Factor as a means of assessing journals, it is clear that _Foundations of Physics_ has a very low score on this scale. This suggests (though it by no means proves) that this journal is of low status; its contents are either not widely cited or are cited more than one year after publication.

Another piece of data in the _Journal Citation Index_ is the 'Self-citing Rate'. This measures the extent to which citations to a given journal originate in articles which are themselves published in that journal. The results for a number of journals are given in Table 2.

Interpretation of these résults is, again, not straightforward. For example, is it reasonable to claim that citations to _Nuclear Physics A_ made in _Nuclear Physics B_ are not self-citations? Also, the age of the journal may be important: a journal published for the first time in (say) 1974 would abviously have a low Self-citing Rate for that year, since there would be no previous articles in that journal to cite. In fact, _Foundations of Physics_ is the youngest of the journals listed in Table 2[13] so its disproportionately large self-citing rate cannot be accounted for in this way.

There is here, therefore, _some_ evidence for the claim that, to a greater extent than for other journals, the only people who are interested in the contents of _Foundations of Physics_ are those who themselves publish in that journal. Other data also support this conclusion of narrow readership. For example, _Foundations of Physics_ was cited only 46 times in 1975, and these citations

Journal	Impact Factor (1975)
American Journal of Physics	0·33
Foundations of Physics	0·35
Nuovo Cimento B	0·8
Journal of Mathematical Physics	1·08
Progress in Theoretical Physics	1·64
Physical Review A	2·61
Physical Review Letters	5·91

TABLE 1

Journal	Self-citing Rate (1975)
Nuclear Physics A	33%
Nuclear Physics B	35%
International Journal of Theoretical Physics	35½%
American Journal of Physics	37%
Foundations of Physics	52%

TABLE 2

occurred in only 16 journals (including itself), whereas the International Journal of Theoretical Physics received 200 citations from 40 journals and (at the other extreme) journals like Nuclear Physics and Physical Review receive many thousands of citations from virtually every physics journal. Obviously, the total number of citations depends at least partly on the number of citable items, but the sources of citations may well reflect the extent to which a journal's contents are widely seen as relevant.

On the other hand, when we examine citations made by authors writing in a particular journal, we find that (in 1975) authors publishing in Foundations of Physics cited 343 different journals, books and other sources; journals cited ranged from Physical Review and the American Journal of Physics to Philosophy of Science and Mind. This pattern of citation over a broad range of sources is unusual; Nuclear Physics cites fewer sources, and is in this sense more 'introspective' than Foundations of Physics. This suggests that FQM authors make use of work done in a wide variety of fields, an impression also supported by more qualitative evidence such as interviews and correspondence.

It is difficult to draw any firm conclusions from citation evidence alone, mainly because of difficulties in deciding what, if anything, all these numbers really mean. However, the data can be interpreted as corroborating evidence for my earlier assessment of FQM, provided we look at all the citation data as a whole. For instance, the high self-citing rate of Foundations of Physics might seem to suggest that this journal represents a subgroup which deals with esoteric problems of little interest to most physicists (a suggestion which seems plausible) but the citing behaviour of this journal's authors suggests also a wide variety of viewpoints within FQM.

In fact, citation data of this sort are of relatively little use in trying to determine the degree of consensus or of social organisation within FQM. For instance, the fact that, in general, citations made in Foundations of Physics refer to a wide variety of sources does not tell us whether each individual behaves in this way, or whether the journal's authors each cite a small number of sources and different authors cite very different sources. Indeed, one might well wish to question the whole notion of drawing conclusions about an author's orientation from the journals which he cites; the range of topics covered in many journals is simply too wide for such conclusions to be very meaningful. Perhaps the best defence of my use of citation analysis here is that the conclusions reached are in agreement with those gained from correspondence, reading, and interviews.

'Schools of Thought' within FQM

Although FQM may have little or no organisational structure, studies of scientific sub-groups have suggested that informal communication networks may often permeate a field[14]. Such networks may originate in student-teacher relationships, and often centre around some key individual.

As pointed out earlier, most FQM workers did not enter FQM as a result of the influence of a teacher, and even those who did do so claimed that their views differed in many respects from those of their teacher. Few physicists' names crop up frequently as teachers of FQM. The major exception is David Bohm.

Many FQM physicists claimed that Bohm's hidden-variable interpretation of 1952 had been a major influence in shaping their attitudes towards FQM. However, for most people, the value of Bohm's work was not that it was correct but that it pointed out that the orthodox interpretation of QM need not be correct. Nevertheless, Bohm's department at Birkbeck College in London has produced a substantial number of PhD students who have written theses which attempt to reinterpret QM along lines similar to Bohm's. Does this, therefore, constitute a school of thought?

Many of Bohm's students, once their PhD research was complete, ceased to have any involvement with FQM. In fact, the only two former students of Bohm who have continued to publish frequently in FQM are Aharonov and Bub. Both, after co-authoring one or two papers with Bohm, have now diverged considerably from Bohm's own position. Bub, in particular, is now highly critical of Bohm's 'hidden-variable' concept, claiming that it, like the Copenhagen Interpretation,

"misconstrues the foundational problem of interpretation by introducing extraneous considerations which are completely unmotivated theoretically."[15]

Among people who did not study directly under him, Bohm is well-known, but is not a highly influential figure. The following statement sums it up well:

"I have a great respect for him, which was born with his 1952 paper....I sort of wish him luck, but I don't expect that I can help him with his programme."

To a certain extent, Bohm and his Birkbeck colleague Hiley form an isolated social group. By this I do not mean that no-one ever communicates with them, but that they themselves limit such interactions. Hiley explains:

"When Dave [Bohm] and I were starting off here, people did come in from the States, ...and we used to have tremendous arguments.... and we decided that it just wasn't worth it. In other words, we deliberately insulated ourselves, because we were getting into these tremendous arguments about red herrings, which weren't getting down to the core of the problem."[16]

Thus it seems clear that Bohm and Hiley no longer (if they ever did) take an active role in trying to achieve communication and consensus among a geographically dispersed 'school of thought'. In fact, Hiley claims to be in favour of diversity:

"There are very many different approaches. I think this is good, I'd like to encourage it. I don't see it as harmful. The difficulty [with QM] is so great, we need very many different approaches cross-fertilizing each other."[17]

The only other likely candidate as a leader of a 'school of thought' in FQM is Prince Louis de Broglie who, in the 1920's, first developed the idea that matter might have wave-like properties. He proposed a hidden-variable interpretation in 1926, abandoned it soon after, and only became critical of orthodox QM again in the 1950's. Now in his late 80's, de Broglie lives in Paris and is associated with a fairly coherent group of French physicists, notable Vigier, Lochak, Andrade e Silva and Mugur-Schachter. Most of them work in Paris, and they have all published articles either publicising or developing de Broglie's 'double-solution' theory as an alternative to QM.[18]

de Broglie's work on FQM is not well-known or understood outside France, as he himself acknowledges when writing about 'hidden parameters':

"these ideas have since been developed ....in numerous papers of de Broglie and his students. In this connection, it is a somewhat puzzling fact that these papers remain poorly known to those who criticise the ideas developed in them and even to some of those who are trying to emulate their particular approach to problems."[19]

de Broglie's group of students is apparently intensely loyal to their leader. In a publicity brochure for a book to commemorate de Broglie's 80th birthday, we find the following:

"A group of friends, colleagues and students of Professor de Broglie ....reveal him as the great discoverer of matter-waves, which revolutionised man's vision of the atom and which govern the working of the electron microscope, the transistor, and the laser. They also show him as the eminent leader of a school, always looking to the future, pursuing his work in collaboration with young scientists and defending tenaciously the conception of the world he developed 20 years ago."[20]

Although he no longer is actively involved in research to any great extent, de Broglie apparently still intends to dictate the direction of research undertaken by his group, expressing

"the hope that young researchers will devote themselves to developing, in the direction I have indicated in these last years, the ideas which allowed the birth of wave mechanics in France half a century ago."[21]

In a sense, then, de Broglie and his followers could be said to constitute a school of thought in FQM, by which I mean that they have a shared, distinctive viewpoint on how QM should be reinterpreted. However, the influence of this school, even in France, is rather limited. There was, for instance, no interaction between the followers of de Broglie and the physicists who performed experimental tests of local hidden variables. One French reviewer of the book cited above described it as:

"totally organised around the ideas of the Master,.... [with] excessive repetition of acts of deference and ritual formulas of quasi-religious tone."[22]

To this reviewer, the work of de Broglie's school exhibited "isolationist purism", and he concluded by asking

"may not [these ideas] be nothing but the temple of a small number of the chosen?"

Despite de Broglie's undoubted personal status in France, his research group receives no government funding. Soon after government funds were cut off in 1972, de Broglie's group formed the Louis de Broglie Foundation, which holds regular seminars and produces its own journal. The contents of this journal often go far beyond the scope of de Broglie's own ideas, but there is little effective dialogue between de Broglie's followers and other authors within the pages of the journal.[22a]

To sum up the findings of this section, it may indeed be argued that there are schools of thought in FQM, and I have discussed the cases of Bohm and de Broglie at some length[23]. However, it is certainly not the case that such schools exert any dominant influence on the development of FQM; nor have I been able to find many other examples of schools of thought in this field.[24] The concept simply does not seem useful for providing a general description of FQM. There are of course some institutions where two or more individuals are active in FQM. However, in many cases these individuals told me that there were many areas in which their opinions differed, and that collaboration with their institutional colleague was rare[25].

The evidence presented so far in this chapter indicates that the social organisation of FQM is not highly structured. In Chapter Two, I pointed out that, in terms of its cognitive content, FQM is rather fragmented, and that there is very little consensus. The latter point is well illustrated by two quotations:

"It can certainly be said that there is no sector of modern science in which so little agreement has been reached about the correct way of interpreting and developing the existing theory."[26]

and

"Time after time the old malaise over QM returns....the same issues keep coming back....and half-forgotten 'solutions' are refurbished, and served up as new by successive generations."[27]

In the next section, I shall look more closely at the lack of consensus in FQM, with a view to determining the interactions which may exist between FQM's <u>cognitive</u> and <u>social</u> position. One useful way of tackling this issue is to compare 'traditional' FQM work with the local hidden variable experiments, since the latter are quite atypical in both social and cognitive terms.

Causes of the lack of consensus in FQM

Many of my informants were acutely aware of the rather fragmented state of FQM, and they provided a number of explanations to account for this state. One account claimed that the diversity of views in FQM results from the fact that few of the issues can be tested experimentally. Not surprisingly, this reason was given most frequently by LHV experimentalists:

"All these wild, unphysical theories had been postulated....you can make theories all day long....but it all boils down to 'does it agree with experiment?'"

It is certainly true that the LHV activity, which centred round experimental work, shows a remarkable degree of both cognitive consensus and social cohesion not found elsewhere in FQM. For example, Bell's original idea that LHVs would be experimentally distinguishable from QM was developed independently (though in less general form) by two other physicists about the same time[28]. When this idea was taken up by two American theorists some years later, they quickly set about trying to organise an experiment and made contact with a group of experimenters, only to find (much to their surprise) that another experimenter had come up with a similar experimental proposal, again independently. Three LHV experiments were completed within three years, and during this period a number of other physicists went at least some way towards testing Bell's theory only to give up on discovering that experiments had already begun. By 1976, eight experiments had been completed, the implications of these experiments had been discussed at several conferences, and a fairly firm consensus (in favour of QM) had emerged.

These events will be discussed in much more detail later in this thesis. For the moment, the above extremely brief description will suffice to illustrate the fact that LHV was not typical of FQM. As far as the experimenters were concerned, the reason for this was that the experimental approach generates consensus, and organises activity, in a fairly unproblematic way. I shall question this view in later chapters. However, regardless of the validity of this view, it does not by itself explain the <u>lack</u> of consensus in other areas of FQM. The LHV experimenters extended their argument (though often only implicitly) by claiming that an experimental approach was the <u>only</u> way in which consensus could be achieved and activity could be 'properly' organised. This argument is much more suspect. In the first place, there are many intellectual activities, from theoretical physics to abstract mathematics and philosophy, in which experimentation seems to play a relatively insignificant part and yet which have not only a coherent social organisation and a marked degree of cognitive consensus, but also (at least in the view of some practitioners) a valid notion of progress. It would seem, therefore, that the absence of experimentation in FQM is not a sufficient explanation for its present state.

According to one informant, the proliferation of interpretations in FQM had come about because no 'good' interpretation had yet been found:

"Because no good interpretation has been proposed, anyone who's interested proposes his own, and everybody can have his own variation."

Yet most informants felt that their own interpretation was indeed a 'good' one. It would seem that the central difficulty in this field is an absence of shared criteria of what constitutes a 'good' interpretation. Indeed, some authors go as far as to claim that the choice of interpretation is purely a matter of personal taste. The most vivid illustration of this claim came in response to an article by DeWitt in which he described the 'Many-Worlds' interpretation[29]. The journal concerned published several replies in which this interpretation was criticised in explicitly aesthetic terms. For example, Sachs claimed that

"such a model does not appeal to my personal physical intuition"

and that it was not necessary

"to go to such extreme lengths of straining physical sensibilities".[30]

Ballentine criticised

"the arbitrary (and in my opinion silly) assumption that the world is splitting."[31]

Pearle is even more explicit:

"None of these interpretations can yet be decisively accepted or rejected on the basis of an experimental test, so the question of which interpretation to choose becomes a matter of personal taste. To me.....[the many worlds interpretation] appears uneconomical."[32]

Irrespective of any philosophical and sociological critiques of empiricism, it certainly seems to be the case that, for many physicists, FQM is not 'real' physics because it is not experimental.[33] This seems consistent with the views of LHV physicists, and with those of Hagstrom's informants. Among FQM authors, the absence of 'hard' empirical arguments may have led to the utilisation of purely rhetorical arguments. We have already seen one such usage, which might be summarised (or parodied) as 'It's all a matter of taste, and I don't happen to like this interpretation.' More subtle forms of rhetoric are also used. For example, a common tactic in FQM is to cite Einstein in one's support. Since Einstein never fully accepted the orthodox interpretation of QM, he has become a major folk-hero within FQM.

To take a specific example, DeWitt writes that the many-worlds interpretation

"is a completely causal view, which even Einstein might have accepted."[34]

His critics do not let this claim go unchallenged. Ballentine argues that Einstein actually supported the statistical interpretation. Sachs is even more explicit;

"I take issue with DeWitt's remark about the [many-worlds] approach as one that 'even Einstein might have accepted'. The features that Einstein anticipated in a fundamental description of matter were spelled out in his own writings....and they were not at all contained in the type of theory that DeWitt discusses."[35]

DeWitt counters with the following:

"It is probably only wishful thinking, but I like to think that Einstein....might have been surprised and pleased at Everett's conception which did not see the light of day, alas, until after Einstein died."[36]

This exchange is by no means unique. Pearle and Sachs have clashed over whether Einstein supported a statistical interpretation[37], and Bell and Jammer totally disagree on the issue of whether Einstein supported hidden-variable theories[38]. Bohr, too has been the subject of much textual exegesis[39].

The presence of such rhetorical exchanges is common in science. However, if (as some critics would claim) such arguments are the <u>only</u> kind to be found in most FQM work, and if personal taste is the only criterion used to choose an interpretation, then FQM is clearly in difficulties. Not only would this make it very difficult to achieve either consensus or progress in FQM, but it would make it impossible for us to arrive at any structured account of FQM's social organisation, because there would be none.

I do not believe this is an accurate picture of FQM. I would argue that we <u>can</u> perceive a structure of sorts within FQM, and that this structure is based on <u>methodological</u> distinctions.

Methodological Choice in FQM

Many of my informants believed that progress in FQM was possible. Individuals claimed to have set themselves certain goals, and to have chosen a certain set of procedures to achieve these goals. They were able to present a picture (one might say a rational reconstruction) of their activity, in which their later work followed on from, and progressed beyond, their earlier discoveries. In the LHV case, the completion of a series of experiments provided a rather unequivocal measure of progress. Elsewhere in FQM, claims to have made progress were seldom universally accepted because different workers held different views about what constitutes progress.

To some extent, these differences may ultimately lie at the level of 'personal taste'. However, I have found it useful to characterise much of the disagreement as a dispute over the choice of methodology to be used in FQM in the absence of empirical tests. For example, faced with apparent logical paradoxes in QM, one point of view is to attempt to alter QM, while another is to attempt to alter the structure of logic[40].

The sociological value of a methodological characterisation of FQM is that an actor's committment to a particular methodology is often not simply a matter of taste, but a reflection of that actor's previous training and institutional position. Many of my informants displayed clear connections between their professional background and their attitudes to FQM.

For example, a mathematician described the source of his dissatisfaction with QM as follows:

"The conventional formalism of....QM, von Neumann's formulation, has got all sorts of mathematical headaches attached to it.... topological problems which are really quite irrelevant to any practical calculation, but which it seems necessary to solve if you want to give a mathematically rigorous theory of QM."

This informant felt that non-mathematicians approach QM in a quite different way:

"X is a theoretical physicist, but he's definitely not a mathematician....that's why he gets so far with what he does. If you work like a true mathematician, you'd get stuck because you can't prove the next implication, but X simply jumps on."

In contrast, many physicists, such as Pearle, DeWitt, and Wigner, perceive the main difficulty with von Neumann's formulation of QM in physical terms - that is, they wish to know how the mathematical procedure of reduction can be described in physical terms, and they construct specific mechanisms by which reduction may take place.

For example, the Bohm-Bub hidden-variable theory, produced in 1966, was based on an earlier HV theory developed by Wiener and Siegel in the 1950's.[41] However, whereas Bohm and Bub were interested in the physical implications of their model, Wiener and Siegel were led to their theory on mathematical grounds. As Siegel told me:

"We chose this [formulation] for mathematical reasons, because of this remarkable....dichotomic algorithm which was the product of Wiener's marvellous, intuitive, mathematical mind."[42]

According to Siegel, they did not have high hopes for the theory, and felt it might well be "physically vacuous". It certainly had little impact on physicists[43], far less than Bohm's early HV theory of the same period.

"Nobody showed much interest in it. It was very much harder to understand [than Bohm's 1952 theory] , the maths was more complicated, and it was not so physical.... So far as the physicists were concerned, since it was difficult to understand, very few physicists gave it the time necessary to criticise it."[44]

Philosophers also drew explicit links between their training and their approach to FQM:

"In my case, it's more philosophy than physics simply because my background is mostly philosophy."

FQM contains elements of philosophy, pure mathematics, theoretical physics, and (in the case of LHV) experimental physics. Different individuals are drawn to take an interest in different features of FQM, and to some extent this seems to be affected by their training or their own self-classification as a physicist, a philosopher, and so on. But this multiplicity of approaches has itself had an effect on the status of FQM, simply because FQM appears, to the outsider, to be such a mixture of disciplines.

"A lot of philosophers haven't picked it up because the kind of maths you need to understand it is pretty esoteric, and it's hard to get a mathematician to pay attention to it, because it's motivated by non-mathematical considerations, so you're caught between two worlds."

"FQM is at the intersection of physics and philosophy....it studies physical theory and uses, or should use, philosophical tools.... The philosophy departments usually mistrust people with a scientific background....and it's not a recognised, established branch of physics."

and

"It's been the bane of my existence, because I publish papers in philosophy and physicists say 'oh well, he's a philosopher', and philosophers did have a tendency to say, 'well, he's a physicist.'"

As mentioned earlier, few people devote the majority of their time to FQM. Many informants explained this in terms of the low status of FQM in the eyes of their professional colleagues:

"It's been an odd experience for me, working in this field. It didn't have at all the kind of rewards that work in mathematical physics or other fields have....I haven't got any strokes from mathematical physicists whom I respect for this work. It's only a part of my research. I think I would have done more work in this field if I'd gotten more rewards."

At the same time, the fact that FQM is not technically well-developed or esoteric in its content makes it possible for people to 'dabble' in this field; it is fairly common to find mathematicians, philosophers and physicists publishing one or two papers in FQM then leaving this area, apparently for ever.

The lack of any coherent organisation or cognitive consensus in FQM may, in a way, be self-perpetuating. Since the field lacks an organised critical system, progress, even for an individual, may be difficult:

"I'm disappointed that so few people are working on this. I can't handle all the mathematical problems myself without years of work.. ..Also, I'm a Popperian, I believe you learn from being severely criticised, and one of the unfortunate things about not having enough people working on this stuff is that you never get criticised. I'm sure I've said lots of wrong things and I'd like to know this."

Thus, there is clearly a complex relationship between the status of FQM, its social structure, and its cognitive content. Its low status, and its lack of an organised social system, seem to be at least partly responsible for its failure to achieve cognitive consensus, apart from the very few 'schools', such as de Broglie's, where a tight, though introspective, consensus is maintained. Yet the lack of cognitive consensus, and the plurality of views within FQM, seem to contribute to its continuing low status. Only the LHV activity has broken free of this impasse, largely because a group of people focused on a specific problem and agreed on a methodology for solving it.

Methodological differences seem to be one major cause of FQM's condition. The concept of methodological divergence is therefore worth exploring further. Before doing so, I must stress the need to avoid over-simplistic models. For example, it would be misleading to divide FQM into three camps - physicists, mathematicians, and philosophers - on methodological grounds. Such a division is far too clear-cut to be an accurate picture. As we have seen, physicists (for example) do not share a common attitude to FQM despite their shared professional label. Although personal taste may play a part in generating this diversity, there is still a place for methodological analysis even within a single occupational group.

For example, DeWitt's support of the many-worlds interpretation may seem a strange position for a physicist to adopt, given the apparently strange physical implications of this theory. But if we look more closely, we find that DeWitt's position results from a prior commitment to what he himself calls 'naive realism'. As discussed earlier, the many-worlds view argues that since reduction does not appear in the basic formalism of QM, then reduction does not occur. An infinity of splitting universes is a necessary consequence of this conclusion, but to DeWitt this is far more acceptable than tinkering with the formalism for 'metaphysical' reasons:

"What becomes of reality? How can one treat so cavalierly the objective world that exists all around us?....What if we forgot all metaphysical ideas?....Let us try to take the mathematical formalism of QM as it stands without adding anything to it."[45]

This, then, is an explicitly methodological stand:

"[This approach] implies a return to naive realism and the old-fashioned idea that there can be a direct correspondence between formalism and reality."[46]

This interpretation, it is claimed, therefore represents a "straightforward....honest view of things."[47]

Thus we should avoid any simplistic account of FQM which claims that physicists favour experimentally-testable interpretations while philosophers argue on philosophical grounds[48]. Methodological differences can be much more subtle than this. DeWitt's case confirms this, and also reminds us that the methodology of an interpretation, and not just the content of that interpretation, can have a major influence on its reception.[49]

Having rejected a simplistic subdivision of FQM into three methodologically-distinct areas, the snag is that we must also reject the idea of using professional labels as an easy, foolproof way of identifying methodological orientation. Such a method may still be useful where the actors being studied are not members of the same professional group (and we shall see examples of this below) but it is of little use in characterising a dispute between, say, two physicists. Given the many diverse activities which go under the name of 'physics', it is not surprising that methodological differences exist within this professional group.

Of course, one could investigate the differences in training between two physicists to try to give a sociological account of their different methodological preferences. However, many of these preferences are, at root, philosophical. For example, to DeWitt, realism means restricting oneself to the contents of the formalism, and accepting whatever picture of reality this implies. To Pearle (among others) realism means questioning and modifying the formalism until it corresponds to a plausible physical model. Sociological processes may indeed play a part in determining such philosophical attitudes; however, other factors are undoubtedly involved, so that the origin of methodological differences remains an open question.

Having seen how the concept of methodological orientation may help us to characterise and account for the cognitive diversity in FQM, let us turn now to what may be an even more interesting application of the concept, namely, the study of conflict in FQM.

Conflict and Methodological Divergence: Pinch's case-study

In the case of the many-worlds interpretation, we saw that DeWitt's critics recognised that no conclusive arguments could be used against this theory. Although detailed technical criticism did occur,[50] many critics chose to avoid prolonged conflict by claiming that the choice of interpretation was 'all a matter of taste', and that they simply could not accept the view that the universe was continually splitting.

There have been other occasions in FQM's history where conflict has been more prolonged and overt, and in the remainder of this chapter I shall concentrate on these. Despite the fact that these disputes were conducted in terms of 'what is true' rather than 'which methodology is best', I shall argue that methodological differences did lie at the heart of these disagreements.

I begin with a discussion of Pinch's study of the reception of Bohm's 1952 hidden-variable theory[51]. This is an important paper, and in many respects Pinch's conclusions are consistent with my own. However, there are a number of areas of disagreement, and I will argue that in such areas Pinch's analysis is unsatisfactory.

The case-study examines the relationship between Bohm's HVT and a mathematical 'proof', advanced by von Neumann in 1932, which claimed to show that no HV interpretation of QM was possible. Pinch asks two questions: first, why did the creation of a supposedly impossible theory not lead to a detailed examination of the proof, instead of a rejection of the theory? Second, why did it take until the 1960's before the relationship between the theory and the proof was properly understood?

Pinch argues that Bohm and the supporters of von Neumann failed to communicate[52]. The clash was not simply due to the fact that these groups supported different philosophical positions, such as realism (Bohm) or instrumentalism (von Neumann), nor because Bohm was trying to reintroduce causality:

"Causality....has, in this dispute, been treated as 'window dressing' ....physicists seem to produce cognitive objects which they then call causal or acausal rather than basing their work on any committment to a particular notion of causality."[53]

According to Pinch, the reason why the conflict existed was that von Neumann had not anticipated the particular type of HVT which Bohm would later produce, and the reason why Bohm was unable to resolve the conflict was that he did not fully understand the axiomatic mathematical techniques used by von Neumann in constructing his proof:

"The failure of Bohm and von Neumann to communicate....can be explained in terms of their differing research techniques. Von Neumann was a mathematician committed to....using the research technique of axiomatisation. Bohm was a theoretical physicist committed to developing new theories."[54]

The lack of communication between (supporters of) von Neumann and Bohm seems, then, to be rooted in differences in methodology and competence between the two sides. This conclusion is quite consistent with my own analysis.

However, when Pinch goes on to examine the reception of Bohm's theory by other physicists, he constructs an account which seems less satisfactory. He argues that although commitment to a philosophical viewpoint, such as acausality, was not a powerful determinant, nevertheless there was a strong commitment among physicists to the 'metaphysical' principle of 'arithmomorphism'. This concept is taken from the work of Georgescu-Roegen;[55] according to the latter, arithmomorphic concepts are those which can be analysed logically, in contrast to 'dialectic' concepts which are qualitative and often contain contradictions. When applied to mathematicians like von Neumann who constructed logical proofs, this nomenclature makes a certain amount of sense; logical proofs involved the manipulation of arithmomorphic concepts. Pinch goes on to argue, less convincingly, that the reason why Bohm's theory was not covered by the proof was that the theory was dialectical. A less elaborate explanation is that the theory did not meet all the axioms on which von Neumann's proof is based. Von Neumann may therefore have been guilty of limited imagination when he constructed his proof (one might forgive him for this since the proof was written 20 years before Bohm constructed his theory) but it is not the case that theories such as Bohm's are not amenable to mathematical or logical analysis[56].

When the concept of arithmomorphism is applied not to mathematicians but to physicists, its validity is even more suspect; since (as Pinch himself points out[57]) physicists do not often use axiomatic methods of logical analysis in their own work. Despite this, Pinch argues that physicists are deferential towards logical 'proofs', although the only example of such deference to be cited is the treatment of the von Neumann proof itself[58]. He claims that logical proofs are the epitome of the 'arithmetic ideal' which is assumed to be incorporated in the 'authority structure' of physics. Thus, the reason why physicists defended the proof and attacked Bohm had very little to do with the content of either of these things, and much to do with their form:

"Bohm was not only producing a rival interpretation of QM but was, by his emphasis on qualitative and physical considerations, also challenging the authority structure of physics....The failure of

Bohm to achieve a social and cognitive redefinition is not surprising when so much was at stake and considering that he was fighting against the embodiment of the arithmetic ideal, the much worshipped and revered, but little understood, von Neumann proof."[59]

There are several flaws in this account. First, apart from the evidence of this particular case, Pinch does not establish as a fact the existence of the 'arithmetic ideal' as part of the 'authority structure' of physics. The argument, therefore, appears circular: physicists defended the proof because it was part of the authority structure, and we know it was part of the authority structure because they defended it!

Secondly, if physicists are opposed to 'dialectical' concepts, it is difficult to understand the high status of Niels Bohr's concept of complementarity, which explicitly sets limits on logical analysis and claims that contradictions are essential in our models of reality. Pinch himself describes Bohr's views as dialectical, and quotes members of the physics elite defending such views. Indeed, it could be argued that Bohm was trying to make complementarity <u>less</u> dialectical and more arithmomorphic by incorporating it into

"a precisely definable conceptual model."[60]

Third, as Pinch points out, the fact that von Neumann's proof is no longer accepted as a conclusive rejection of all HVTs (because of its restrictive axioms) has not improved the climate for such theories:

"The realisation that 'HV impossibility proofs' do not rule out HVs has not led to the success of Bohm's interpretation or any other such theory."[61]

Also, the demise of this 'embodiment of the arithmetic ideal' does not seem to have had any effect on the authority structure of physics either.

Let me suggest an alternative account for the rejection of Bohm's theory, and for the role of the proof in this rejection. When we examine the comments made by physicists on Bohm's theory, it is clear that it seemed to them to be a very retrograde step, and a return to a position which had only recently been abandoned after a long struggle. Pauli claimed it was 'old stuff, dealt with long ago', it was 'arbitrary', and it introduced an asymmetry for which there was no justification. Heisenberg claimed it was merely 'an exact replication of the Copenhagen Interpretation but in a different language', and Rosenfeld felt Bohm was inadvisedly 'attempting to restore determinism'.[62] Bohm himself openly admitted that his interpretation was experimentally indistinguishable from orthodox QM and that there was no evidence in favour of his view.[63]

Without wishing to substitute an 'empiricist ideal' in place of Pinch's 'arithmetic ideal' as a central element in the authority structure of physics, I would suggest that it is fairly easy to see, from the above comments, why Bohm's theory was unattractive to practising physicists. To an experimental physicist, Bohm's theory offers no guide to any practical steps which might be taken to test its validity. (When testable HVTs are proposed, as we have seen, experimenters do take an interest.) To a theoretical physicist, Bohm's theory offers only a more complicated, and rather alien, way of saying the same thing.

In other words, Bohm's theory was irrelevant for the aims and practices of most physicists; it was of no use as a resource. Most physicists therefore ignored his work. The fact that members of the elite chose to actively criticise, rather than ignore, Bohm can also be explained without reference to an arithmetic ideal. Until Bohm produced his HVT, his work had been both respectable and respected[64]. Insofar as it is the task of the elite to set goals and specify methods for their discipline, and to monitor and reward progress, it was clearly incumbent upon them to point out the error of Bohm's ways, and to dissuade anyone else from following what seemed to them a sterile path.

What role did von Neumann's proof play in this? Rather than acting as the main motive for Bohm's rejection, the proof merely constituted a useful rhetorical resource to legitimise the rejection, just as other rhetorical attacks on Bohm accused his theory of being retrograde, artificial, asymmetric, metaphysical, and so on.

It may well be true, as Pinch claims, that physicists did not examine either the proof or the theory to a sufficient extent to explain the contradiction. This again, though, was a rather sensible practical strategy. As Pinch points out, most physicists lacked the mathematical competence necessary for a proper analysis of the proof;

equally, they lacked any strong incentive to take time off from their own research in order to develop these esoteric skills. It is possible that some physicists rejected Bohm, not on methodological grounds, but because they erroneously believed that the proof was conclusive. Yet it seems more likely that the proof was an excuse, and not the cause, of Bohm's rejection.

There are further arguments in favour of this conclusion. There is, first of all, a general argument that it is inadvisable to give a major role to 'the power of logic' as a determinant of action.[65] Secondly, Bohm himself managed to reject the spell cast by von Neumann's proof, yet his critique of the proof in his 1952 paper, to quote Bell,

"seems to lack clarity, or else accuracy."[66]

Since Bohm was able to construct a refutation (however shaky) against the proof, one can presume that other people in the 1950's would also have been able to do. Yet few did.

Third, as already mentioned, Bell's later (and more rigorous) refutation of the proof has not led to an upsurge of interest in HVTs of the type produced by Bohm. The reason, I would suggest, is that such theories seem no more attractive to physicists now than they did in 1952. No doubt von Neumann was held in high esteem in the 1950's, even though physicists might not understand his work,[67] but it would be easy to exaggerate his authority in determining physicists' actions - after all, most physicists were quite aware of the fact that the great Einstein had strong reservations about QM, yet few people accepted Einstein's ideas.

If these arguments are accepted, then the fact that the proof was not 'really' a proof was quite irrelevant, as long as this fact was not glaringly obvious, and as long as it was convenient for physicists to think of and refer to the proof as a proof. Thus, although both Pinch and I would agree with Belinfante's claim that

"for decades, nobody spoke up against von Neumann's arguments, and....his conclusions were quoted by some as the gospel"[68],

I would not accept Belinfante's other claim that

"the authority of von Neumann's overgeneralised claim for nearly two decades stifled any progress in the search for HVTs."[69]

Bohm was able to work on his HVT despite the proof, and I do not think the proof deterred many people who would otherwise have joined him. Belinfante is a physicist, not a historian, and he tends to rewrite history from a present-day perspective with the advantage of hindsight. For example, he writes:

"I have always been puzzled how people could ever have been convinced by von Neumann's arguments....the lack of validity of [his postulates] in any decent HVT should have been obvious to anybody by inspection."[70]

It is true that for someone who now sees the proof as obviously and trivially wrong[71], there is indeed a problem in accounting for its acceptance, or at least its citation, over so many years, whether we attribute this to 'magic', as Belinfante does, or to 'arithmomorphic commitment', as Pinch does. I hope I have convinced the reader that neither of these viewpoints is correct.

In this section, I have introduced the argument that disputes and communication breakdowns in FQM can be the result of differences in scientists' practices, aims or methodologies. However, the argument would be unconvincing if it applied only to a single episode. Fortunately, this is not the case.

Wynne's study[72] of the rejection of Barkla's work in the 1920's provides many interesting parallels with Bohm's rejection in the 1950's. Like Bohm, Barkla was rejected not because he had been shown uneqivocally to be mistaken, but because he was out of step with the concepts, goals, and methods of 'orthodox' physics[73]. Like Bohm, Barkla (a Nobel prizewinner) was too prestigious to be simply ignored, so that the rejection of his work had to be overt, and the (practical) reasons for this rejection had to be supplemented by rationalizations and rhetoric.

In the next section, I shall examine another such episode, in which the opposing sides are theorists and experimenters. Since no mathematicians or proofs are involved, there is little scope for arithmomorphic and dialectical commitments as explanatory devices. However, I hope to show that methodological differences are central to the dispute.

<u>Papaliolios' Experiment and the Bohm-Bub Theory</u>

The technical details of the Bohm-Bub theory and the Papaliolios experiment have been given in Chapter Two. For convenience, I will summarize the main points here.

Bohm and Bub postulated the existence of a set of hidden variables whose values normally 'averaged out' to give the same predictions as QM. The process of measurement is considered to disturb the HVs and change their values. After a very short delay (the 'relaxation time') the values resume their equilibrium distribution and once more correspond to the QM predictions. Papaliolios set up an experiment in which a measurement is performed, followed by a second measurement very soon after the first; the time interval between the two was shorter than Bohm and Bub's estimate for the relaxation time, τ. No deviation from QM predictions was detected. Thus either QM is correct or the relaxation time is too small to be detected by Papaliolios' method.

Some additional technical details will be required for the discussion which follows. In their original paper, Bohm and Bub provide a numerical estimate for τ of approximately 10^{-13} seconds, based on the tentative equation

$$\tau \approx \hbar/kT$$

where $\hbar$ is Planck's constant divided by 2π, k is Boltzmann's constant, and T is the temperature (in Kelvin) of the system.[74] (In fact, taking T as 300°K, i.e. approximate room temperature, $\tau = 2{\cdot}5 \times 10^{-14}$ seconds.)

When Papaliolios published his result, he quoted Bohm and Bub's estimate for τ as approximately 10^{-13} seconds, but quoted the equation incorrectly as

$$\tau \approx h/kT$$

that is, without the factor of 2π.[75] (Taking T as 300°K, this equation gives $\tau = 1{\cdot}6 \times 10^{-13}$ seconds.)

In his experiment, Papaliolios was able to use times as short as $2{\cdot}4 \times 10^{-14}$ seconds, which is well below h/kT but comparable with $\hbar/kT$. Later work allowed him to reduce this limit even further, to $\tau < 1{\cdot}9 \times 10^{-14}$ seconds.[76] Thus the incorrect citation of h/kT instead of $\hbar/kT$ was, in the long run, irrelevant since Papaliolios managed to rule out <u>both</u> these estimates. However, since

Papaliolios' lower estimate was not published until seven years after his initial result, one might have expected Bohm and Bub, during the intervening period, to have defended their prediction by referring to Papaliolios' error. They did not do so; indeed, they both told me that they were not at all surprised to find their prediction refuted, and that they saw no point in trying to defend their prediction. In fact, what <u>did</u> surprise them was that anyone went to the bother of testing their prediction in the first place. Clearly, the attitudes of Papaliolios and of Bohm and Bub towards the prediction and the experiment were very different.

Papaliolios' interest in the theory was almost entirely due to the fact that an experimental test could be performed. In an interview, I asked him if he had been interested in FQM prior to his experiment:

"No. I have an interest in doing interesting experiments. Especially if they're easy experiments."[77]

The fact that Bohm and Bub had produced a numerical prediction was very important to him:

"I wrote to Bohm, saying I was grateful that he'd given a value for the time, because without such a value the theory is meaningless, because you could always say that the time is sufficiently short that things look like the usual quantum state....And without such a value for the time there is no experiment."[78]

From Papaliolios' point of view as an experimentalist, not only must a 'meaningful' theory make specific predictions, but the predictions must be testable in a practical way. This is clear in the next extract from the interview. I asked him what he would have felt if Bohm and Bub had suggested a value for τ of 10^{-23} seconds, a far shorter time than could be measured with Papaliolios' apparatus. He replied:

"Giving an unattainable number is no better than giving no number ....[the theory] would be nonsense, just an unnecessary complication to quantum theory. As long as it's untestable, and has no experimental consequences, you shouldn't even clutter up your mind with it."[79]

In his experiment, Papaliolios found that there was no deviation from QM's prediction; however, as he was well aware, this result does not <u>refute</u> the Bohm-Bub theory. It simply suggests that their estimate for the relaxation time was too large. Indeed, Bub <u>did</u> point out that this conclusion could be drawn:

"An experimental comparison between the statistical predictions of the quantum theory and this HVT has been carried out recently. On the basis of a photon polarisation experiment, it has been shown that if the HV hypothesis is true, then $\tau < 2{\cdot}4 \times 10^{-14}$ sec."[80]

Once his original experiment was completed, Papaliolios had to decide what further steps, if any, ought to be taken to test the theory. He refined his apparatus to reduce the upper limit of τ even further, though this was a relatively trivial extension of his original experiment and was not published until Papaliolios was asked to describe his work in a review paper some years later.[81] Clearly, a supporter of the Bohm-Bub theory could reply to such lower figures in exactly the same way as before, namely that they simply set an upper limit on τ .

Another possible line of action, which Papaliolios considered seriously, was to use Bohm and Bub's claim that τ varied inversely with temperature. According to this suggestion, cooling the apparatus to 100°K (the temperature of liquid oxygen) would increase τ by a factor of three compared with its value at room temperature. Thus an experiment which could measure times as short as $1{\cdot}9 \times 10^{-14}$ seconds would be a much more crucial test of the theory if the predicted value of τ was raised to (say) $7{\cdot}5 \times 10^{-14}$ seconds. In his original paper, Papaliolios discusses this possible approach:

"It is also possible to perform a more definitive test of Bohm and Bub's choice of $\frac{h}{kT}$ [sic] as the relaxation time, by repeating the experiment at lower temperatures. The lack of a theoretical understanding of this choice of τ , however, does not at this time justify cooling the apparatus to liquid air (or lower) temperatures."[82]

Neither Papaliolios, nor anyone else, has ever performed such an experiment. In Papaliolios' case, the reason for this was largely to do with external events; his main research interest was astrophysics, and

"then the pulsars came up and I just completely dropped these experiments and never returned to them."[83]

Why has no other experimentalist carried out a low-temperature test of the theory? The reason, I suggest, is that from an experimentalist's point of view such an experiment would serve no useful purpose. The experimental upper limit for τ could be lowered indefinitely without crucially affecting the theory. Similarly, the temperature-dependence of τ had 'no theoretical justification'.[84] In addition, since it was clear that τ was less than $1{\cdot}9 \times 10^{-14}$ seconds, a very short time, the possibility that τ might really be even smaller did not seem at all plausible, given the absence of any justification for such small values:

"As the upper limit is reduced it becomes more difficult to invent a believable physical process that might be responsible for it."[85]

In interview, Papaliolios was more forthright:

"Bohm didn't give me any justification for $\hbar/kT$. So if the guy who writes the theory essentially admits that it's a number he pulled out of the air then why should I waste time testing it....There has to be some theoretical foundation."[86]

At this point, the reader may be wondering why, given the arbitrary and unsatisfactory status of the prediction, any experimental tests were performed. In the first place, the experiment was both easy and cheap:

"I constructed the apparatus and did the experiment all within a period of two weeks.....the apparatus was trivial....it was done without any funding."[87]

Secondly, the experiment offered the chance of a publication, and in fact was published in the prestigious journal Physical Review Letters. Thirdly, although it seemed highly unlikely that QM would be refuted, the possibility was there. For the first time, an alternative to QM had produced a specific prediction for experimentalists to look at:

"My feeling was that QM was probably right. One has a feeling as to what's probably going to happen, but if you really believe that you wouldn't do the experiment."[88]

Clearly, Papaliolios thought that the experiment was an attractive one. He told me that he worked very quickly, because his experiment seemed 'obvious', and he thought that lots of people might be planning such experiments in the first few weeks after Bohm and Bub published their theory. In fact, only one experimental proposal other than his own appeared in print[89]. This involved relaxation effects in nuclear physics - very different from Papaliolios' photon polarisation study - and no such experiment was performed. One reason why Papaliolios might have seen his experiment as obvious was that he had been using polarisers in his undergraduate teaching, and, as he put it:

"It was just a coincidence that my thoughts were running along these lines when I happened to see the Bohm-Bub paper by chance."[90]

Let us now examine Bohm and Bub's attitude towards an experimental test of their theory. We shall see that such a test was not at all what they had in mind when they wrote their paper.

In their introduction, Bohm and Bub state that

"It is definitely not proposed that the theory developed here is likely to be a 'right' one."[91]

Instead,

"the main aim of the theory is to provide a language and a set of concepts."[92].

They argue that the Copenhagen interpretation is unnecessarily restrictive, since it maintains that we cannot talk about what is 'really' happening in microsystems; instead, QM provides us with a set of predictions which apply in specific experimental contexts. Bohm and Bub argue that

"science is surely more than <u>merely</u> a set of algorithms for an engineer's handbook. Science also aims at an <u>understanding</u> of the overall structure and order of movement of matter from the atom to the galaxies."[93]

To enlarge the scope of QM, they argue, requires a new descriptive language[94], and their theory attempts to do this. Despite its imperfections, such an attempt is worthwhile, firstly because it shows that an alternative language is <u>possible</u>, and secondly because it may lead, in time, to better theories.

"The theory, in its present form, suffers from a number of inadequacies, but it does provide a new conceptual structure in which certain questions can be considered, which cannot even be formulated within the framework of QM....In some cases, a new language opens up a different structure of thinking and thus leads to new kinds of actions in relationship to nature. What is needed now is a hypothetical tentative approach..."[95]

Clearly, this is rather distant from the concerns of experimentalists. Bohm and Bub <u>do</u> discuss the experimental implications of their theory, though this section occupies less than a page of their 17-page paper, and it is rather speculative. Discussing the value of the relaxation time, they write

"This is, of course, unknown, but some plausible suggestions can be made at this stage."[96]

Two mechanisms for relaxation - thermal and quantum mechanical - are suggested, though only in the former case is a specific prediction ($\hbar/kT$) made. In an interview, Bohm confirmed Papaliolios' suggestion that $\hbar/kT$ had little theoretical justification:

"The value of τ was just a suggestion, the number was almost pulled out of a hat.... $\frac{\hbar}{kT}$ was nearly a guess.... τ could be immensely shorter."[97]

The mechanism used by Papaliolios (altering the polarisation of photons) was not mentioned by Bohm and Bub. Papaliolios gained the clear impression that they did not expect either this or any other experimental test of their theory:

"I wrote to Bohm describing the experiment and the result, and I think I caught him by surprise:"[98]

Bub confirms this impression:

"I was utterly astounded when I learned Papaliolios was doing an actual experiment....For me, [the theory] was only a didactic model....it never occurred to me that these thoughts would be taken seriously....it wasn't that we'd proposed a rival theory to QM, but rather we had investigated, by means of the model, the possible structure of a class of rival theories."[99]

The experiment does not seem to have had much impact on the development of Bohm's or Bub's ideas. In later papers, they continued to argue that the <u>real</u> significance of a HV theory is that it introduces new concepts and a new mode of description. In a review of HVTs published in 1969, Bub makes no reference to Papaliolios' experiment.[100] Although, as stated earlier, Bub now rejects Bohm's HVT approach, this change seems to have been the result of the influence of the philosopher Putnam, and does not seem to have had anything to do with Papaliolios.[101]

Papaliolios, and Bohm and Bub, clearly held very different attitudes towards both the theory and the experiment. To Papaliolios, the presence of a specific numerical prediction was the theory's saving grace; it gave him a way to evaluate the theory in terms that were meaningful to him. Once the most direct test had been performed, the looseness of the theory's predictions made it uninteresting to experimental physicists, partly because <u>no</u> experiment could conclusively refute the theory, and partly because there was no 'believable physical process' to account for such short times.

To Bohm and Bub, the main value of their theory was its new conceptual structure, not its empirical validity. Bohm's attitude to his earlier (1952) theory was quite similar.[102]

It is of course possible that Bohm and Bub deliberately included a specific prediction to gain attention for their work, and deliberately chose a value for τ which would be experimentally accessible, in a way that 10^{-23} seconds would not. However, there is no evidence to support such a cynical view. Instead, all the evidence suggests that Bohm and Bub simply do not equate 'meaningfulness' with 'immediate testability' in the way Papaliolios does.

This case-study strongly suggests that methodological differences can play a central part in disputes. Both Pinch and I would agree on this point. However, this example can also discriminate between Pinch and myself, by providing further evidence against Pinch's use of the terms 'dialectical' and 'arithmomorphic'.

According to Pinch, certain concepts in Bohm's 1952 theory are <u>inherently</u> dialectical, and are therefore in conflict with the 'arithmetic ideal' of physics. Pinch is therefore arguing that the <u>structure</u> of the theory led to its rejection. In contrast, my analysis focussed on Bohm's <u>aims</u>, and on the way he <u>chose</u> to present and develop his theory. In other words, I argued that any analysis of what Pinch calls 'cognitive objects' must take account of the ways in which these objects are manipulated. Only in this way can we explain other features of FQM, such as the fact that an avowed realist like DeWitt finds the many-worlds theory attractive.

Applied to the Bohm-Bub theory, a methodological analysis would suggest that the two attitudes to this theory which we have already encountered were not the only possible ones. Scientists who held neither 'experimental' nor 'philosophical' methodologies would have attitudes to the theory quite different from those of Bohm <u>and</u> Papaliolios. Let us now examine the attitudes to the theory of a mathematician, Jerald Tutsch.

Tutsch published three papers on the Bohm-Bub theory, and in these papers he not only defends the theory but also argues in favour of more experimental tests as soon as possible. Clearly, his views are not identical with any we have yet encountered.

In his first paper[103], Tutsch points out the lack of theoretical justification for the figure of 10^{-13} seconds as the time interval over which QM might cease to be valid. He derives a new time of 10^{-15} seconds, based on a different mechanism. Although such short times are attainable in principle, this new estimate saves the theory from Papaliolios' original experiments.

In his second paper, Tutsch makes it clear that he does not share Bohm and Bub's concern with developing new conceptual structures at the expense of experimentation:

"Whether or not the new variables must remain hidden will be settled by mathematics and experimental physics, and not by philosophy."[104]

However, Tutsch also claims that Papaliolios' experiment has not settled the issue, because the measured time interval was too long. In fact, the experiment

"showed the randomisation time to be smaller than that conjectured by Bohm and Bub."[105]

In his final paper, Tutsch derives a new hidden-variable theory from

"a combination of very general and reasonable requirements concerning QM, physical measurement, and mathematical simplicity."[106]

He shows that this theory leads to the same equations as those used by Bohm and Bub, and again he stresses that Papaliolios' experiment does not constitute a proper test of either theory.

Tutsch's views seem to lie somewhere between Bohm and Bub's speculations about conceptual frameworks, and Papaliolios' concern solely with empirical testing. Tutsch's training, and his methodological orientation, are equally distinct. He was trained as a mathematician, and his work on HVT was performed for a PhD in the maths department at the University of Wisconsin. In a letter, he explained his viewpoint:

"My main interest in mathematics has been in nonlinear systems of differential equations, and I found the Bohm-Bub equations interesting from that standpoint....I always felt that HVTs were simply another interesting mathematical model of the world....[Bohm-Bub] is an interesting model of a HVT, but that is about all....It was my feeling that the Papaliolios experiment did not really test the Bohm-Bub theory, and that experiments of that kind never could."[107]

Tutsch's case supports the view that there is no simple dichotomy between 'arithmetic' and 'dialectical' concepts, or even between another possible pair of categories such as 'empirical' and 'theoretical' concepts. A single 'cognitive object', such as the Bohm-Bub theory, can be interpreted in many different ways.[108] The particular interpretation which an actor constructs is heavily dependent on his general methodological orientation.

Discussion

In this chapter, I have been concerned with two interrelated issues; the social organisation of FQM, and the lack of consensus in this field. I argued that the diversity of views in FQM, as detailed in Chapter Two, is reflected in the rather fragmented (if not anarchic) social organisation of this field. It seems clear that there is an interaction between the social structure and the cognitive content of FQM; however, this interaction is complex and two-way. The lack of a clearly-structured training, communication and reward system in FQM militates against the emergence of consensus by removing the most obvious means by which deviance could be sanctioned and agreement rewarded. Equally, the lack of any shared body of knowledge, or of a communal perception of progress, encourages scientists, mathematicians and philosophers with a wide range of different perspectives to 'dabble' in this field; that is, to invest a short period of time in FQM while retaining a more lasting committment to the aims and methods of their 'home' disciplines.[109] Even the relatively few individuals who spend the majority of their time in FQM seem to feel isolated, though this is often by choice; recognising the fundamental differences in outlook which separate many FQM workers, individuals often felt that discourse with groups who did not share their views was pointless.

The concept of methodological divergence can also be used to account for the conduct of disputes in FQM. This analysis began with an examination of Pinch's case-study. Despite our differences in terminology, Pinch and I are largely in agreement over the causes of the 'communication breakdown' between Bohm and the constructors of 'impossibility proofs'. However, I argued that concepts such as arithmomorphism and the dialectical/arithmetic dichotomy are unnecessary and misleading. This is not only because concepts such as 'dialectical' can be defined in many different ways (as we saw in Chapter Three) but also because this usage ignores many features of Bohm's particular case. Theories and ideas cannot be unproblematically identified as arithmetic or dialectical, and it is therefore unsatisfactory to argue that the physics elite opposed Bohm simply because he threatened the arithmetic ideal. The limitations of such a normative account, in which deviance is recognised automatically, and sanctions applied in an unproblematic way, have already been discussed in Chapter One. I presented an alternative account in which Bohm's work was depicted as a retrograde proposal which was perceived to be out of step with the aims and methods of the rest of the physics community.

Turning now to the reception of the Bohm-Bub theory, we saw that Bohm, Papaliolios and Tutsch disagreed over the theory's status and value because they each wanted to <u>use</u> the theory in different ways. One might even make a stronger claim, drawing on Wittgenstein's dictum that 'the meaning is the use': in a very real sense, their differing methodologies led these actors to <u>perceive</u> the theory in different ways. To Bohm, the theory was an example of an alternative conceptual structure; the attempt was so tentative that it might better be called a <u>metaphor</u> rather than a serious attempt to accurately describe reality. To Papaliolios, the theory was a source of empirical predictions, and the low 'quality' of its predictions meant that it was not a good theory. To Tutsch, the theory was a set of nonlinear differential equations which happened to contain terms which referred to physical observables. Since these actors held such different views about the meaning and purpose of the theory, it is hardly surprising that they failed to reach agreement. The different perceptions of the theory were firmly embedded in these scientists' practice. It would be very difficult for the participants to achieve consensus about the status of this theory without resolving these more fundamental differences in practice.

In conclusion, I have argued that a satisfactory account of the existence, conduct, and outcome of disputes in FQM must take into account the social and technical context of the dispute. This context includes the peculiarly fragmented social and cognitive structure of FQM, which has important effects on the manner in which FQM develops. In this field, actors with very different methodological orientations often converge on a single cognitive object, but they may fail to engage in constructive discussion about that object. Because of their methodological differences, they may, simply, fail to perceive the disputed cognitive object in the same way.

Chapter Five

Social Context and the Process of Scientific Investigation

Introduction

In previous chapters, I have examined the interaction between FQM and its 'social context' at a number of levels. In Chapter Three, I dealt with the general cultural and political context in which interpretations of QM were developed and transmitted. In Chapter Four I dealt with the interaction between the internal social organisation of FQM and its lack of cognitive consensus. In Chapters Five and Six I will look in more detail at a single episode of FQM - the experimental tests of local hidden variables (LHV) - and I will discuss the influences which the local social context exerted on the conduct of the physicists involved.

A study of LHV is particularly interesting for several reasons. First, as stated earlier, the LHV group was much more cohesive than much of FQM. The group had a clearly-stated purpose (namely, to test the validity of LHV) and they agreed on the methodology to be used in this task. As we shall see, not everyone agreed with the idea of experimental tests. This is a second reason why LHV is interesting. Unlike the cases discussed in Chapter Four, methodological issues were openly discussed in LHV.

Perhaps the most important reason for looking closely at LHV is the fact that it was an experimental activity. Thus, at least according to the traditional view of science, the outcome of this activity might be expected to be relatively immune from social influences in a way which more theoretical FQM might not. In Chapters Five and Six I will investigate the validity of these expectations. In the present chapter, I shall concentrate on the behaviour and attitudes of the LHV physicists, and in the next chapter I will examine the content of their knowledge-claims.

In Chapter One, I argued against normative accounts of scientists' behaviour, and in favour of an approach which can be labelled interactionist or contextualistic. This approach is based on the view that scientists are actively engaged in processes of interpretation and negotiation. Behaviour is not determined by passive obedience to a set of rules. Instead, courses of action are undertaken for a variety of practical, idiosyncratic reasons (that is, reasons which make sense for the individual within his particular context) and such actions may be retrospectively depicted as rule-governed.

According to this view, highly specific details of the local context in which behaviour occurs may be vital for a complete or consistent account of that behaviour. There are two reasons for this. First, although behaviour may always be goal-directed, the specific goal of a particular actor depends crucially on the context. Second, the choice of behaviour which is most likely to fulfil a particular goal will also depend on the context.

The general aim of this chapter is to examine and demonstrate the ways in which individuals come to terms with their local social context, how they adapt their behaviour to meet the demands of that context, and how the products of their behaviour become data in the deliberations of other individuals.

The first part of the chapter deals with a number of features of the development of the LHV activity. I will examine the formation of communication networks, the reasons why individuals chose to perform experiments, the ways in which they presented their work, and their response to anomalous results. In each case I will show that individual physicists were concerned with solving practical problems, and that their actions were tailored to fit the particular conditions under which they operated, and the particular goals which they were trying to fulfil. The second part of the chapter looks at how the LHV activity has become a fact to which other physicists must now respond. LHV, in other words, has altered the social context in which FQM activity takes place.

The Social Context of LHV.

Before discussing the effects of social context, it is obviously important to describe the relevant features of that context. This raises methodological problems, since what is seen to be 'relevant' may depend on the sort of account which is put forward. For example, one could argue that the fact that the LHV experiments were performed during the Vietnam war is a relevant feature of the social context of these experiments. There is little evidence in favour of this particular argument[1]. However, it use fully demonstrates that any account which links behaviour to social context is open to accusations of bias and selection. There is no complete defence against this argument; ultimately, the reader must judge the plausibility of my account for himself.

The context of the LHV experiments can be subdivided into a number of areas. In historical terms, the empirical success of QM, and the absence of any successful alternative theory, or alternative interpretation of QM, are very relevant. The historical context meant that any proposed alternative to QM would, a priori, be seen to be implausible. I do not wish to imply that all such alternatives would be rejected out of hand - evidently, this did not occur with LHV. However, there is little doubt that QM and LHV did not compete on equal terms, and it would be very surprising if this had no influence on the conduct of the LHV experimenters.

In social terms, most of these experiments took place in the USA, within university physics departments, and many of them were performed by fairly junior physicists at either doctoral or post-doctoral level. These physicists were therefore not entirely secure in their positions, and were heavily dependent on the support, or at least tolerance, of their superiors. At the most obvious level, some degree of acceptance of LHV experiments as 'valid physics' was required if a PhD was to be awarded.

In addition, the skills required for the LHV activity were largely experimental. There is a fairly well-established division of labour in science, particularly physics, between theorists and experimenters[2], at least to the extent that the kind of person who had been involved with theoretical FQM prior to LHV (such as Bohm) could not readily perform a LHV experiment, either because such a person would lack the necessary skills, or because he would be unable to gain access to the necessary apparatus.

Thus, the sort of person who became involved in LHV was necessarily different, in skills and methodological approach if not in temperament, from the 'typical' FQM worker. This might well be expected to play an important part in shaping the relationship between LHV and the rest of FQM.

The above elements are some of the features of the social context of LHV which are particularly relevant. Obviously, there are many features which were not unique to LHV and yet which influenced the conduct of the experimenters. Such features include the contemporary communication system in physics, journal refereeing procedures, the different status of various universities, and so on. There is little point in trying to list all such features.

Having presented an admittedly sketchy picture of the social context of LHV, let us now examine some aspects of the experimenters' behaviour to see if we can identify any features which seem to be causally related to this social context. I shall begin by examining the early history of LHV and the ways in which the members of the LHV group initially came into contact with each other.

Formation of Communication Networks in LHV

In the early history of LHV, there are many instances where two or more individuals independently put forward very similar proposals, both theoretical and experimental, and where each was unaware of the existence of the others. Of course, such examples of near-simultaneity are by no means restricted to LHV[3]; however, these episodes had an important bearing on the development of LHV, and are therefore worth examining in detail.

The general idea that it might be possible to distinguish experimentally between QM and local theories seems to have originated independently with three people: Bell, at CERN in Geneva, and T.D.Lee and Richard Friedberg, both at Columbia University in New York. The approaches of the three authors were not identical. Bell's LHV paper was a development of an earlier paper in which he had criticised 'proofs', such as von Neumann's, which claimed to show the impossibility of HVTs[4]. Lee had developed his ideas with specific reference to the behaviour of high-energy particles called K-mesons. He did not publish a paper referring specifically to tests of locality, although two authors, Inglis and Day[5], did discuss K-meson experiments of the sort Lee had described. However, partly due to technical difficulties with the production and rapid decay of K-mesons, neither Inglis, Day nor Lee himself developed these ideas further. Friedberg's analysis[6], though stylistically very different from Bell's, arrived at the same conclusions by a similar route, namely, by examining the Einstein-Podolsky-Rosen thought-experiment.

It is significant that none of these ideas made much of an impact; in fact, Friedberg and Lee did not even publish their conclusions on locality, and Bell's paper was published in the first volume of a new journal (Physics) rather than in an existing high-status journal.

It would be easy, in retrospect, to exaggerate the impact of these theoretical developments. After all, we have now reached the point where nine separate LHV experiments have been performed. Many papers on LHV have been published, and the conclusions drawn from the experiments have been widely circulated in the 'popular' scientific press[7], and even in less 'respectable' circles such as parapsychology and mysticism[8]. Indeed, one physicist, Henry Stapp, has gone as far as to claim that

"Bell's theorem is the most profound discovery of science."[9]

It is therefore tempting to portray the development of the LHV activity as a 'natural' progression from Bell's early paper through the experiments to the present day. In fact, Bell's paper was not at all well-known; although published in 1964, the first recorded reference to it did not appear until 1968, and this citing paper contains factual errors about the implications of Bell's ideas.[10]

The first experimental test of locality did not use Bell's ideas at all, at least initially. Leonard Kasday, who performed an experiment for a PhD at Columbia with Jack Ullman and C.S.Wu, originally developed his theoretical model in terms of Friedberg's unpublished results. He only learned of Bell's work when another physicist, Clauser, visited Kasday at Columbia in 1968 to find out some details about their experiment. In 1972, when Kasday completed his PhD thesis, he cited Bell as the source of his theoretical model, adding

"Professor Friedberg greatly clarified the significance of Bell's Theorem."[11]

Kasday's 'accidental' discovery of Bell's proposals was not unique. Clauser, who later performed several LHV experiments, also came across Bell's 1964 paper by accident[12], and Shimony, who was instrumental in organising another LHV experiment, only read Bell's paper because a mutual acquaintance told Bell that Shimony might find it interesting, whereupon Bell sent him a copy.[13] With the exception of Kasday's test of Friedberg's proposal, no LHV experiments were proposed until 1969, five years after Bell's original paper was published.

The explanation for this haphazard diffusion of Bell's ideas, and of the delay in producing experimental proposals, must take account of the underdeveloped social and technical context in which Bell's paper appeared. First, there was no existing network of experimental FQM workers, and no journal which could serve as the appropriate medium for such papers. Second, there was much uncertainty over the technical details of an experiment; both Shimony and Clauser initially felt that an experiment of the type proposed by Bell had in fact already been done.[14] Even when they realised that existing experiments were not sufficient to rule out LHV, they were still faced with the difficult task of designing a new experiment which could test LHV. Third, there was no organised communication between theorists and experimenters, so that even when Clauser and Shimony had (independently) decided on the form that such an experiment might take, neither of them knew how to go about getting access to the necessary equipment. In each case, accidental encounters were again important, as the following anecdotes illustrate.

Clauser had just completed a PhD in radio astronomy at Columbia University. He discussed the possibility of an experiment with Kasday, and decided, for reasons I shall describe later, that a different sort of experiment was required. He was also at this time looking for a job, and he gave several papers at seminars at different universities. At MIT, he met a postdoctoral student, Kocher, whose PhD experiment at the University of California at Berkeley, had involved measurement of photon polarization correlations. Clauser already knew that measurements of this sort could be used to test Bell's proposals, and he examined Kocher's work in some detail. He realised that it did not meet the requirements of a LHV test[15], but that a modified form of this experiment would be suitable. He contacted Kocher's ex-supervisor, Commins, at Berkeley, but found that there were no vacancies for postdoctoral students in this department.

However, Clauser obtained a postdoctoral appointment elsewhere in Berkeley, and once there he gradually 'worked his way into' the appropriate research group and performed a LHV test.

Around the same time, Shimony and his PhD student Horne were also trying to find someone who had access to the necessary apparatus. The chain of events in this case was even more tortuous. As Shimony put it:

"We went around asking everybody we had access to who had experimental knowledge of optics, 'where can we get photon pairs which we can use for the purpose of testing LHV?'"[16]

Early in 1969, Shimony gave a talk on FQM in Cleveland, Ohio, where he met Pearle, a doctoral physics student. Pearle suggested that Shimony should talk to Snyder at Harvard. Snyder recalled Kocher's experiment with Commins, and on checking the reference Shimony realised it was indeed suitable. Shimony also decided to ask Papaliolios, whom he did not know but whose paper on the Bohm-Bub theory he had read. Papaliolios was also at Harvard, and when Shimony showed him the Kocher-Commins paper,

"Papaliolios said, we have an apparatus at Harvard very much like this....Pipkin had a student, Holt, who was just beginning his doctoral work, intending to use this apparatus to look at the lifetime of an intermediate state. Papaliolios arranged a meeting ....We explained what was going on, why their apparatus was useful for testing Bell's inequality. We had to do a lot of explaining of the motivation of our experiment....This type of thing was very far from the concerns of Holt and Pipkin so we had to discuss the matter for about an hour or so before they became fairly convinced that we were on the track of something interesting."[17]

These episodes, and particularly Shimony's, illustrate quite clearly the lack of any organised communication system at the start of the LHV activity. In addition (and the relevance of this point will become clear later) Holt's involvement with LHV was pure coincidence; he happened to have access to the necessary apparatus. He had no prior involvement, or even much interest, in the philosophical aspects of FQM. Holt's own recollections of his early feelings confirm this view:

"I thought, sure, I'll whip that off in six months then get back to some real physics."[18]

As a final illustration of the effects of the poor communication system in LHV, we should note that Clauser, and Shimony and Horne, came up with virtually identical proposals while being completely unaware of each other's existence. Shimony later recalled his feeling that there was no great urgency in publishing a preliminary paper to establish priority, and that he and Horne could take the time to complete their detailed calculations first.

"We felt, it doesn't matter, no-one else is doing this anyway...that was an illusion."[19]

They discovered an abstract of a paper by Clauser on LHV in the Bulletin of the American Physical Society.[20] They contacted Clauser, and agreed that they should all collaborate in completing their theoretical calculations, and that they should keep in touch, once Clauser left for California, so that they could all monitor the progress of the experiments at Harvard and Berkeley.

In this section, I have traced the early history of LHV, in an effort to show that the outcome - nine completed experiments - did not follow naturally, or unproblematically, from Bell's initial paper. Events such as the five year delay before Bell's work was taken up, Kasday's switch from Friedberg to Bell, Clauser's move to Berkeley, and Holt's involvement in LHV, were all contingent events which depended on some highly particular feature of the social context in which LHV took place.

Deciding to Perform a LHV Experiment.

Individual physicists had to make a conscious decision to perform a LHV experiment instead of spending their time doing other things. Yet, as we shall see, there were some drawbacks to LHV compared to other possible experiments. It is therefore interesting to ask why these people chose to do LHV experiments.

As pointed out earlier, FQM work was generally held to be of low status. This perception might well be expected to be most strong among experimenters, bearing in mind the non-empirical character of most FQM work prior to LHV. Yet the LHV group had to work alongside experimental physicists. Many LHV workers felt that this led to problems:

"I had considerable problems....with finding a place to do this experiment. The comments you get....for doing this hidden-variable thing. They think it's a waste of time because they already know the result....X was very upset that I was spending too much time working on what was obviously of no importance, the results were already known, and it was crazy that I didn't believe the existing theory."[21]

PhD students involved in these experiments made similar comments about their supervisory committees:

"I encountered difficulties because of what the topic was, because I had to justify to the people on my committee what was going on. They had very strong biases about the subject matter....whereas with other types of thesis experiments it's taken for granted that, well, lots of people are doing this, you're measuring the coefficient of such-and-such, and you're okay."

We must be careful to keep these 'difficulties' in their proper perspective. There was no grand conspiracy to suppress LHV experiments, or to harass LHV experimenters. The experiments themselves were, after all, technically respectable, and Bell was a respectable theorist based at CERN, a high-status institution. The following quotation from an experimenter may help to clarify the position:

"B. said that my experiment was interesting, but he asked if I had a permanent position, because I would have difficulties, many people would say that my experiment wasn't interesting and that I was wasting my time. Well, I have seen people who <u>could</u> have said this, but if I explain the problem as I see it....many people finally say okay....T. said to me,'I would never give such an experiment to one of my students, but if one came and saw me with the same enthusiasm as you have, I would let him do it.' You see, experiments like this are a kind of a luxury. You can accept them from time to time."

It would seem that the LHV physicists deliberately chose to enter a field which was of relatively low prestige, and that they performed experiments which most people thought were very unlikely to produce either surprising or even particularly informative results. Evidently, <u>some</u> factors must have overridden these disincentives. What were they?

A rather facile way of accounting for these physicists' decision would be to say that they simply found the topic interesting. Certainly, one would be surprised to find physicists voluntarily working on a topic which they did <u>not</u> find interesting. In interviews, I <u>did</u> find that most people cited interest or curiosity as one of the reasons for their involvement. Apparently, people who were in favour of QM found just as much interest as those who were less convinced:

"I went into this business with the idea of disproving hidden variables once and for all."

"I got into the business with the idea that....hidden variables might really be there."

"I was very interested originally in the foundations of QM, simply because I couldn't understand it, and I wanted to understand it."

However, to leave the explanation at this point would be inadequate. For one thing, I found several people who expressed interest in LHV experiments yet who claimed that they would not have wished to perform such an experiment themselves, nor would they have advised other people to do so. For example, Nussbaum had completed a PhD under Pipkin at Harvard shortly before Shimony approached Pipkin and Holt. Nussbaum later went to the University of Tennessee; he considered the possibility of doing a LHV experiment there, but decided against it, partly because there were then two other experiments in progress, and partly because, as he put it,

"Given my other responsibilities and research commitments, my entry into the 'hidden variable sweepstake' seemed neither necessary nor wise."[22]

Another experimenter, Scarl, who had already done some 'fundamental' optics experiments, told me that although he found the topic interesting, he was reluctant to allow any of his students to perform such an experiment:

"You can't really put a graduate student on that kind of problem, because when he gets out there is no employment. That is, if you go out and say, 'hey, I did my thesis in hidden variables', and you try to impress, say, Lockheed Aircraft, I don't think they're going to be too interested."[23]

Whether or not such assessments of the 'risks' of LHV were accurate, these statements serve to remind us that intellectual curiosity is only one of many factors which may influence a scientist's decision about where to invest his time. Let us try to identify some other factors which were important in the LHV case.

Nearly all the people who performed the experiments were PhD or postdoctoral students.[24] I wish to suggest that, given the social context of LHV, such people were particularly suited for these experiments, and that in this (rather weak) sense the social context determined the sort of person who would get involved with LHV.

To some extent, the topic studied by a doctoral or post-doctoral student is a means to an end: namely, being awarded a PhD or producing acceptable reports at the end of a postdoctoral fellowship. None of my interviewees felt that their LHV work had 'typecast' them. They have now gone on to work in fields as diverse as atomic physics, laser fusion, and the psychology of perception. This 'flexibility' among physics students has also been found in other studies[25]. This may have helped to overcome any fears about getting involved in what was undoubtedly a rather unorthodox activity.

There are other relevant factors. For instance, a PhD student requires a topic which is not beyond his technical competence, yet which allows him to develop and extend his range of technical skills, and so enhance his future employment prospects. He requires a topic which will yield worthwhile results within two or three years, yet which is something more than a routine application of well-known procedures, differing only in detail from what has gone before.

Seen in this light, the LHV experiments begin to look more attractive. The work is technically feasible, yet can be presented as a fundamental test of one of the most important theories in physics. At the same time, it avoids the necessity for very expensive hardware or large-scale collaboration, involving twenty or thirty people, in sharp contrast to fields such as high-energy physics.

All these factors seem to have been important for the LHV experimenters. In their comments, quoted below, they come across as an opportunistic group, for whom an interest in FQM may have been a necessary, but by no means a sufficient condition, for their involvement. (In the interview extracts which follow, all emphases have been added to support the arguments made above.)

"I was in nuclear physics....<u>I was looking for a thesis project</u>. I always wanted to do experiments which sat on a table-top but nevertheless had some reasonable significance, and I'd always been concerned about the basis of QM....this seemed like a good thesis experiment."

"I didn't like the idea of a high-energy experiment as part of a big team, with all the politics and bureaucracy....the LHV experiment was technically difficult....I'd always been interested in QM.... The experiment was graduate standard. <u>It was just luck</u> that I was free at the time. Otherwise I'd have done a weak interaction experiment."

"<u>I was looking for a postdoc position</u>, or someplace to go when I finished my thesis on astrophysics, and I wanted to do something in FQM, although I didn't really have anything in mind until I read about LHV."

Another physicist, speaking before his experiment was completed, said "From the experimental point of view it's very interesting, because I'll learn many things, I'll use some techniques that I don't know. This is very interesting for an experimentalist."

There is still another feature of the social context which may have been relevant. The high status of QM may have led many people to feel that the outcome of the LHV experiments was a foregone conclusion. Equally, however, the high status of QM meant that if the experiments were to conclusively support LHV, they would have had a very great impact. As one experimenter put it

"There's a tremendous pay-off if QM is wrong. This would be phenomenal, very exciting....I don't regard it as a big possibility, though."

Here we should be cautious. It is one thing to speak wistfully, after the experimental results were known, of what the consequences might have been if QM had been falsified. (Some interviewees mentioned Nobel Prizes.) It is quite another thing to claim that this possibility was given widespread credence prior to the experiments. In principle, of course, any new experiment could lead to the downfall of an established theory. This Popperian claim was often used to defend the whole idea of doing LHV experiments, as we shall see. However, LHV was considered to be rather implausible, and (at least in retrospect) few of my interviewees claimed to have taken the possibility of refuting QM very seriously. Of course, given what they now know, my interviewees may have been rewriting history. We have no way of knowing what their expectations prior to 1972 really were.[26] Nevertheless, we can at least say that, given the other features of the experiments listed above, physicists did not need to believe that they were heading for a Nobel Prize in order to have good reasons for doing LHV experiments. The small chance of a large pay-off may have been one influence, but there is no reason to suppose it was a necessary, or even a strong, influence.

In this section, I have discussed a number of features of the LHV context which seem to have played a part in selecting the personnel involved. The decision to perform a LHV experiment did not emerge naturally as a result of intellectual curiosity alone. Instead, it was the result of a complex evaluation process, in which physicists assessed the experiment in terms of its relevance for their particular needs. The outcome of this evaluation process was that certain people ended up doing LHV experiments [27], while other likely candidates did not.[28]

Social Context and the Presentation of LHV.

In this section, I will examine the way in which the LHV group presented their work to the physics community, through published papers and theses. My aim will be to show that their presentation was not simply consistent with the general conventions which govern the structure and style of physics papers (such as a subdivision into introduction, data and conclusions; use of the passive voice; and citation of other relevant work.[29]) I will show that LHV papers also reflect the special context in which these papers were written; that is, the style and structure of these papers were strongly influenced by the low status of FQM in the eyes of most experimental physicists.

It is important to note that the LHV experimenters considered themselves to be 'respectable' physicists who just happened to be performing experiments in FQM, rather than FQM workers who just happened to be using experimental methods. Their training was in conventional physics; their colleagues in the departments where they worked were 'conventional' physicists; and they submitted their results for publication, not in FQM journals such as Foundations of Physics, nor even to 'general' physics journals such as the American Journal of Physics, but to high-status, highly technical journals such as Physical Review and Physical Review Letters. Clearly, the LHV group considered that the proper audience for their work was the mainstream body of physicists rather than simply FQM workers. It is therefore not surprising that they took active steps to counteract the fairly low opinion of FQM held by much of this audience.

One very common tactic used in many of the LHV group's reports was to lay great stress on the qualitative differences between their work and all previous FQM work. Previous work had largely been characterised by philosophical criticism or mathematical manipulation of axioms. The LHV work, they pointed out, was the first major piece of experimental FQM, apart from Papaliolios' experiment. Furthermore, and unlike Papaliolios' experiment, the LHV work offered the hope of a decisive result either for or against QM, unlike previous FQM work where (they claimed) the reader's own philosophical preference was the ultimate arbiter. Let me give some examples of these arguments.

"While some theorists were attempting to demonstrate the impossibility of hidden variables in general, others were trying to show by example that such theories could be constructed....The dramatic change in the state of affairs came about when Bell derived a general restriction on a wide class of HVTs."[30]

"At the very time when the axiomatic approach seemed to have reached a dead end, possibilities of experimental verification became apparent."[31]

"For forty years, physicists have wrestled with the implications of QM; the result is a vast inconclusive body of literature on the subject....the experiment proposed provides a decisive test.... the first conclusive test of the entire family of LHV theories."[32]

"In the absence of an experiment capable of discriminating between the different interpretations, the discussion of the foundations of QM was more the concern of philosophy of science than of actual physics."[33]

"Bell's theorem has profound implications in that it points to a decisive experimental test of the entire family of LHV theories."[34]

Apart from attempting to 'distance' the LHV activity from prior work on FQM on methodological grounds, and claiming that the LHV work would be decisive, the above quotations contain a third feature which seems to have been at least partly rhetorical. This is the claim of generality: the experiments would not simply test one LHV theory; they would test a 'whole class' or indeed 'the entire family' of LHV theories. In a sense, this is quite true, since Bell's analysis (which shows that QM and LHV make quite different predictions) is not restricted to any particular LHV theory. Bell's analysis depends mainly on the necessary consequences of any LHV theory.[35]

The generality of Bell's analysis would have been very useful if there had been, say, five different LHV theories in existence in the 1960's, each of which differed in its detailed structure; all such theories could be eliminated by one experiment.[36] However, no-one had proposed, let alone defended, any particular LHV; indeed, as far as I can tell, Bell was the first person to use the term 'local hidden variable'. Thus although the experiments might indeed test 'the entire family of LHV theories', the fact that no member of that family had yet been born suggests that this usage was at least partly rhetorical.

Even the claim of decisiveness had a large rhetorical content. Because of limitations of the efficiency of their apparatus, and on the choice of experimental design, certain assumptions had to be made before the experimental data could be validly used to discriminate between QM and LHV. This will be discussed in some detail in Chapter Six.

The LHV physicists were well aware of the need for assumptions, and the justifications for these assumptions were discussed in detail within experimental reports and reviews of the LHV activity. Although they did their best to argue that the assumptions were justifiable and did not detract from the value of the experiments, there is clearly some contradiction between claims of decisiveness and the need for assumptions. The best way to account for this, I would argue, is to interpret the 'decisiveness' claims as partly rhetorical, although, as we shall see, the LHV group felt there were very good reasons for accepting the validity of the assumptions.

The fact that the assumptions were publicly analysed in such detail is also consistent with the claim that the LHV physicists were concerned with presenting a good public image; the best way to do this was to be as rigorous as possible in pointing out all their assumptions. This, after all, is a central feature of 'good empiricist methodology.'

I do not wish to imply that there was a cynical or hypocritical concern with the presentation of a particular public image. The LHV group were not pretending to be good empiricists; as far as I can tell, they genuinely believed in the value of a rigorous, careful approach, and strove to follow this methodology as well as possible. In interviews, they pointed out that there were good technical reasons for this approach:

"You can't be cavalier here, because you're trying to rule out certain clearly stated ideas, and if, in the course of bringing these ideas into contradiction with QM at the real experimental level, you have to make some assumptions, then you've got to expose those assumptions."

In addition, they were also well aware of the low status of FQM and the benefits to be gained by clearly pointing out their rigorous apprach:

"In this field, publication should be done with higher standards than you would impose on a normal physics experiment. We've got to redeem ourselves from a generation of quacks."

Similar opinions were expressed in response to my questionnaire. More than half of the physicists involved in LHV felt that such an involvement could, in principle, hamper a young physicist's career. However, most of them added that this had not happened in their own case, and they attributed this to the fact that their work had been rigorous, and had been seen to be rigorous:

"Your career won't be hampered if you do your work well."

"Work in this area won't harm anyone if he does good work."

"I did encounter difficulties because of the subject-matter, but basically you show that you've done a competent experimental job and that's sufficient."

In this section, I have shown that the LHV group adopted a particular style of presentation, including rhetorical attacks on non-empirical FQM and an emphasis on 'good methodology'. Such behaviour was a deliberate response to the special context in which the LHV activity took place.

Social Context and the Response to Anomaly.

In Chapter Two, I pointed out that one and only one LHV experiment produced results which were both contrary to the predictions of QM and in clear agreement with Bell's prediction for LHV theories[37]. In the next chapter, I will discuss the processes by which this experiment came to be classed as an erroneous result by the LHV community. For the purposes of the present section, I shall simply take this result as a fact, and examine the response of the person who produced that fact, Richard Holt. In the next section, I shall examine the response of other physicists to Holt's result.

Holt's experiment[38] was similar in design to another experiment, performed by Freedman and Clauser[39], which was completed a few months before Holt's. Freedman and Clauser found that their results agreed completely with the predictions of QM. Initially, Clauser seems to have been fairly enthusiastic about LHV and, according to his own and others' accounts, he had entertained the hope of falsifying QM. In an interview, Clauser described his feelings about his own results:

"I was really disappointed....I wanted to find the fatal flaw in QM....there's not much you can do to deny the result. You do the experiment yourself and that's what comes out of it. What can you say?"[40]

Holt did not respond to his own results in this straightforward empiricist manner. Apparently, he had strong reservations about the validity of his results as soon as they began to appear, and these were reflected in the way he treated his results. For example, Clauser and Freedman published their results, whereas Holt did not, nor did he even submit them to a journal. Holt, and his PhD supervisor Pipkin, took a long time to reach this decision:

"We kept flip-flopping. One of us favoured publishing, the other didn't, then we both changed our minds....In the end, we decided not to publish, nor to keep it a secret."[41]

Their final decision was to produce an unpublished manuscript describing the experiment and the results. It is also relevant that although Holt's results started to appear in 1971, he did not submit his PhD thesis until 1973. The intervening period was spent in a (fruitless) attempt to isolate a source of error in the experiment.

Although both the thesis and the later unpublished paper describe an exhaustive series of error-tracing tests, they are written in rather different styles. Holt claimed that these stylistic differences reflect the different aims and audiences of these two accounts:

"The thesis was written very strongly....the mood I was in at that time was, if I'm going to present this, then whether I fully believe it in my own heart of hearts or not, I'm going to give it a fair presentation and not just, out of the side of my mouth, happen to mention that the consequences of this experiment could be very startling. I was going to make the presentation as strong as could be justified by the results....In the paper, the presentation is less strong, because this was for publication, where you really want to say nothing which is going to be speculative, you want to put in the minimum which you can justify. In particular, I felt one should take the attitude, 'Look, here are our results, we are very cautious about them, we don't accept them, but we think you ought to know about them'....A thesis is just a completely different thing from a refereed publication. What's appropriate for one isn't appropriate for the other."[42]

Why did Holt spend nearly two years checking his apparatus, rather than simply stating his results, then dropping the matter? He now describes this investment of time as a virtual necessity, in order to safeguard both his doctorate and his reputation:

"I was very disappointed with my results....I don't believe QM has yet reached its outer limits....and also, as a practical matter, it meant that I had to spend an extra two years looking for systematic errors to make sure that anybody would believe me....When it came time for my final oral, I was expecting a hard time, and a lot of suggestions on what could have gone wrong....but during those two years I had inputs from so many different quarters that just about everything had already been thought of....I know a lot of people have had the attitude before they knew about what I did that it was obviously just a sloppy experiment. Then they've come and looked at my apparatus and read my thesis and talked to my supervisor or me, and they almost invariably say, 'I'm really impressed that you looked at all these possible systematic errors, and I can't think of anything else. Not that I believe your results, but I do believe that it was a very carefully done experiment.'"[43]

Holt also stressed the fact that he had performed two other experiments during his PhD research which had absolutely nothing to do with LHV:

"I took the precaution of naming the thesis 'Atomic Cascade Experiments' and emphasising the fact that I'd done two other experiments as well that I wanted people to notice....one of these was I think a fairly important contribution, I was fairly pleased with that result, though everyone keeps looking at the hidden variable part of the work."[44]

At one time, while holding a very temporary post, Holt considered the possibility of repeating his experiment, and negotiations concerning this issue took place at three different locations.[45] However, at this time he was offered a more secure long-term post elsewhere, involving more 'orthodox' experiments. As he put it:

"If you're looking for a career in physics you can't just keep doing way-out experiments. You want to do some mainstream experiments too. People kept telling Clauser to go out and measure a few numbers instead of doing more of these crazy experiments. It was even worse for me, since I was doing them and getting the wrong answer."[46]

Let me summarise the alternatives available to Holt. He could have publicly disowned his result, claiming that it was due to an unknown error - but this would not have reflected well on his competence, at a stage in his career when he was looking for employment as an experimental physicist.

He could simply have presented his results as he found them, with no comments on whether he believed them, and moved on to another field - but given the surprising nature of his findings, disbelief would have been inevitable. Could this PhD student really have found a flaw in QM when another group, with a very similar experiment, claimed to have found none? Such behaviour on Holt's part would almost certainly have led other people, including his PhD committee, to assume that Holt had made a simple error.

A third option would have been to publish his result, claiming or implying that it was a valid result. Undoubtedly, this would have generated a lot of publicity, and if Holt's claim was later vindicated his prestige would have been greatly enhanced. But this is a very big 'if'. The central role of QM in contemporary physics, the marginality of LHV, Freedman and Clauser's result - all these factors strongly suggested that Holt's results were not likely to be corroborated by subsequent experiments. In addition, Holt now claims that he intuitively doubted his results all along.

There was yet another argument against claiming that the results were valid. Holt's own supervisor, Pipkin, had been involved in a similar situation in the early 1960's, when another of his students obtained results which apparently disproved quantum electrodynamics, another highly successful physical theory. This claim, which was published, was later shown to be spurious.[47] Although Holt stresses that no pressure was exerted on him to 'cover-up' his results, this embarrassing episode, so close to home, must surely have served as a cautionary tale. What would be gained by a public fanfare? Provided that his result was made known to the small group of physicists actively involved with LHV, there was little to be gained by sensationalism. Why risk having to make a retraction at a later date of a result which he doubted anyway?

None of the above courses of action seemed likely to produce a particularly favourable outcome. Seen in this light, Holt's actual response to his result was clearly a sensible one, given the context in which he found himself. Faced with a result which he did not believe, his actions seemed to him to be the best way of minimising any damage which might be done, and of optimising the outcome of what could have been a very embarrassing episode. By showing that the error was persistent, non-routine, and apparently deeply-rooted in the apparatus, and by permitting other physicists to examine his apparatus, without isolating the error, he attempted, as it were, to 'defuse' the error so that it did not reflect seriously on his own competence as an experimenter.[48] My impression from talking to nearly all the LHV physicists is that Holt has successfully presented himself as a good experimenter who had a bit of bad luck, obtained an incorrect result, yet treated that result in the correct sceptical manner[49].

The Response of Other Physicists To Holt's Result.

As stated earlier, I am not concerned in this chapter with cognitive aspects of LHV; that is, I shall not at this stage discuss the conclusions which the LHV physicists reached, either about the validity of Holt's experiment or the status of LHV in general. In the present section, I shall simply examine physicists' behavioural response to Holt - what they did, rather than what they thought.

Obviously, such a subdivision is an artificial one. The behaviour of the LHV group was clearly influenced by what they knew about the cognitive status of LHV. In addition, as we shall see in the next chapter, Holt's own behaviour had an important influence on the cognitive evaluation of his results.

Nevertheless, I hope to show in this section that many aspects of the response of other physicists to Holt's results owed little to their cognitive assessments of the validity, or otherwise, of these results. To a large extent, they were motivated by quite different concerns.

When we remember Holt's own response to his experiment, we might expect that this experiment would be totally ignored by other physicists, and classed as an obvious error. This is not in fact what happened. A number of physicists took an interest in Holt's work. Some of them visited his laboratory to inspect his apparatus, and two physicists, Clauser and Fry, performed separate experiments[50] which set out to resolve the disagreement between the experiments of Holt and of Freedman and Clauser.[51]

At first sight, this may seem slightly odd. It is certainly true that the two early experiments gave different results, but why were other people prepared to take Holt's result seriously enough to do further research on it, when Holt himself was apparently convinced that his result was simply wrong?

There is more than one possible answer to this question. One answer would be to invoke Merton's norm of 'organised scepticism'[52]; that is, scientists are not only supposed to be sceptical about novel results, but they are also considered to be sceptical about any attempt to dismiss such results, even if the dismissal is made by the person who actually obtained them.

There are several difficulties with this sort of account. I have already discussed some general criticisms of the normative approach in Chapter One. To quote a single pertinent criticism, if such a norm of 'scepticism about dismissals' were to be applied universally, it is difficult to see how scientists would ever find the time to generate new results, since all apparently erroneous experiments would have to be checked by independent observers. The existence of categories such as 'gremlins' and 'transients' in scientists' vocabularies, which serve to account for unexplained, often temporary deviations from expected results, suggests that in practice there are limitations on scepticism.

A more specific criticism of the 'scepticism' argument in the case of Holt is that many interviewees were able to produce a large number of reasons why they 'never believed Holt's result'. Although such statements may involve some retrospective rewriting of history, it cannot be denied that there were many good reasons for disbelieving Holt's result, even as early as 1973. This issue will be discussed in more detail in the next chapter.

I wish to put forward an alternative to the normative account, by arguing that belief or disbelief in Holt's result was by no means a crucial factor in the decision to perform further experimental tests. All that mattered was that Holt's result was, or could be presented as, an anomaly or a puzzle which had not been explained. Superficially, this seems very similar to the normative account; both accounts rely on the premise that science is a puzzle-solving activity, and that anomaly generates practice. The important point, though, is that not all anomalies give rise to practice - hence the existence of categories such as 'gremlins'. A great many anomalies are routinely abandoned without any prolonged investigation into their causes. The decision as to whether a particular anomaly should be followed up or dismissed is a complex one, and such a decision is not made solely on technical grounds.

As we have seen, Holt's particular position ensured that he did have to investigate his result in some depth. Had he not searched diligently for a source of error, and had he not been seen to have done so, he felt that his PhD might not have been awarded and his competence as a physicist might have been questioned. But why did other physicists choose to devote their time to this problem?

At the very least, they had nothing to lose; they were not responsible for Holt's result, so that if they did discover a trivial error in Holt's procedures, they themselves would not be embarrassed. More importantly, further experimentation could serve several functions which would not be served by simply ignoring Holt's result. The first such function is perhaps the most obvious: namely, that the disagreement between the two existing experiments might be resolved. However, it would be easy to exaggerate the importance of further experiments in this regard. Although more evidence in favour of QM would be more satisfying, it seems fairly clear that, even if no further experiments had been done, QM would have been vindicated in any case[53]. Further experiments could be presented as important for settling a 'live' cognitive conflict, but there were other factors which were probably more important in encouraging this work.

One such factor was the credibility of the whole LHV enterprise. As stated earlier, some rather extravagant claims were made for this activity; for the first time, a decisive answer to a problem in FQM would be available. Holt's results were obviously incompatible with such claims. Clauser, at least, was apparently acutely aware of this. He performed a further experiment which he described as an "experimental investigation of....[an] anomaly."[54]
Clauser's results were in full agreement with QM. He later expalined his rationale in the following terms:

"In the work which I and others did, we really ought to have put a great effort into getting very clean experiments to avoid criticism, and I think to some extent we were quite successful....the people I've been working with who have studied this have really made this thing respectable experimental physics, which it was not when we started."[55]

Another advantage of further experiments was that, like any other challenging piece of work, they offered the chance to develop skills and techniques which could then be applied to any number of other topics. Recall that many LHV physicists cited this as one of their reasons for becoming involved in LHV in the first place. This factor seems to have been a strong motive for Fry, the second person to investigate Holt's experiment.

Fry first took an interst in LHV as early as 1970, soon after he read the first published experimental proposal. Together with another physicist, he applied for a number of grants in 1970 and 1971 in order to purchase the necessary equipment. Although they did manage to obtain some equipment, they found that they could not overcome certain technical problems, connected with the hyperfine structure of the mercury atom. Also, Freedman and Clauser's result appeared at about this time, and Fry decided to abandon his experiment.

However, Holt's result completely altered the situation. As Fry put it:

"I didn't have the money to get around the [hyperfine] problem and so I abandoned the experiment in 1972. Then when the Holt-Pipkin result came out, that provided enough impetus for me to get more money to buy a laser and do my experiment."[56]

In fact, when Fry reapplied for funds in 1974, he was given money by an organisation which had rejected his application in 1970[57]. Admittedly, his second application included substantial technical improvements,[58] but Fry at least has no doubts that the change in the granting body's attitude was the result of Holt's findings:

"The reason they gave for their first rejection was that there were already two other experiments in existence....of course, when they disagreed, the higher [data collection] rate of my experiment became more crucial."[59]

Holt's result, regardless of whether Fry believed it to be valid, thus gave him an opportunity to negotiate for funds:

"Holt's experiment provided a basis from which I could argue to get some more money."[60]

I certainly do not wish to imply that Fry had no interest in the LHV issue other than as a way of getting money. He himself notes that:

"aside from the question of getting funds, someone had to resolve the disagreement....there had to be another independent experiment."[61]

Nevertheless, I think there is some reason for claiming that Holt's result was not a crucial part of Fry's motivation, but rather it provided a resource by means of which he could fulfil some wider aims in his work. For example, in 1973 Fry published a paper[62] on general applications of two-photon correlation experiments, in which he noted their usefulness in testing LHV theories as well as other uses such as finding the efficiency of photon detectors, and studying resonance fluorescence.

There are two further pieces of evidence. First, after publishing his LHV results, which were

"in excellent agreement with the QM prediction....and in clear violation of the LHV restriction"[63],

Fry made a further application for funds to improve his apparatus, in order to gain more data on the LHV question, and also to perform several quite different experiments involving two-photon correlations, along lines similar to those discussed in his 1973 paper. There is no suggestion that this application was motivated by doubts over the validity of Fry's existing LHV results. As he himself said:

"This [proposed experiment] doesn't really tell you anything new in a direct sense.... in the original LHV experiment I took data for about an hour and a half. If I took data for 300 hours, I would be able to have really small error bars, and get a really beautiful curve, and just to have that beautiful result it should be done. That alone is enough argument."[64]

Thus, Fry's improved experiment would be more satisfying from the point of view of experimental design, although it would not tell us anything we don't already know about the status of LHV.

The second piece of evidence is that, shortly after completing the LHV experiment, Fry moved temporarily to another university[65] in order to take part in another 'fundamental' experiment, this time to study parity violation.[66] Like many of the LHV physicists, Fry expressed a general preference for such fundamental experiments, rather than for more 'routine' work:

"Many people just go out and measure things....but I think it's much more exciting to do things like the hidden-variable experiment and the parity experiment."[67]

It seems clear from the evidence presented here that Fry was largely concerned with developing experimental techniques per se, and applying such techniques to a number of different 'interesting' empirical problems. The LHV experiment was simply one of these problems.

We should remember that the LHV group considered themselves primarily as experimental physicists, not FQM workers. None of them planned to spend their entire professional life doing LHV experiments[68]. Many of them claimed that, once it became clear that existing experiments favoured QM, it was difficult to see any point in further LHV experiments. In addition, none of the numerous alternatives to the Copenhagen interpretation of QM had led to realisable experimental predictions which could provide conclusive tests of these alternatives. Most of the experimenters therefore described their LHV work as 'a one-off job'.

Earlier, we saw how Holt was led to treat his result with suspicion because of the context in which it appeared. In this section I have argued that, for similar reasons, we ought to be surprised that other physicists apparently took it seriously enough to want to check it. At the very least, the decision to perform further experiments should be seen as problematic rather than inevitable. It is difficult to <u>disprove</u> the view that scientists are primarily motivated by a desire to search for truth and to resolve puzzles and anomalies. But such a view merely explains the existence of scientific practice; it says nothing about why some anomalies are explored and others are not. I have advanced other motivations (a concern with the reputation of this field, and an interest in acquiring apparatus and skills) which may account for the later experiments.

<u>The LHV Experimenters and Methodological Differentiation</u>.

We have already seen that the LHV experimenters were highly critical of non-empirical FQM work. They were careful to 'distance' themselves from such work in published comments, by contrasting its inconclusiveness with the 'decisive' nature of experiments. Such criticisms were partly rhetorical; however, they also reflect what seems to be a genuine distaste of 'traditional' FQM on methodological grounds. This attitude, I suggested, originated in the different background and training of the LHV group.

Although some of the LHV group simply performed their experiments then left this field, others, notably Clauser and Shimony, applied an experimental methodology to other aspects of FQM. Both these physicists seemed to feel very strongly that this approach was the best, if not the only way to properly tackle all issues in FQM. Not surprisingly, some non-empirical FQM workers took exception to this intrusion into 'their' field. Equally, however, other people were apparently inspired by the LHV activity to investigate other experimental approaches in FQM. There is even some evidence to suggest that the field of FQM may be becoming more clearly differentiated into different subgroups, each of which adheres to a particular methodology. The purpose of the present section will be to describe the above events, and to present them as a final example of the way in which the process of scientific investigation is affected by the social context in which this process occurs.

I begin by examining Shimony's viewpoint. Although he did not himself perform a LHV experiment, Shimony was central in bringing together many of the people who became most deeply involved in LHV[69]. He was also extremely vocal in his advocacy of empirical methods in FQM. For example, at a conference in 1970 (at which Bohm also gave a paper), Shimony commented on non-local hidden-variable theories, such as Bohm's, which do not disagree empirically with QM.

"There are nonlocal HVTs; certainly, no empirical evidence refutes this family of theories....There are, however, methodological objections against [such] theories at present....This family is too large, and some members of it seem compatible with any experimental data whatsoever. Before it can be taken seriously, some heuristic principles must be exhibited for restricting attention to a subfamily which is small enough to have definite empirical consequences."[70]

(DeWitt made a similar point about HVTs when he said that we should not 'change the rules' by postulating new entities because "there are presently no rules about changing the rules."[71] However, DeWitt uses this argument to support an interpretation which Shimony has explicitly rejected[72]. Thus, as we saw in Chapter Four, the fact that two authors reject one particular methodological approach does not necessarily mean that they both accept the same alternative approach.)

Shimony's most explicit defence of the experimental method occurs in a paper which he coauthored with some of his students in 1977.[73] To set this paper in its proper context, we should note that in 1963 Shimony published a review of several interpretations of QM. One of these was Wigner's 'consciousness' interpretation, in which it is proposed that the wave function is reduced during measurement by means of an interaction with the observer's consciousness. (See Chapter Two for details.) In 1963, Shimony rejected this interpretation on the following grounds:

"This is counter-intuitive in the extreme, though without apparent inconsistency. However, there is no empirical evidence that the mind is endowed with this power....and furthermore there is no obvious way of explaining the agreement among different observers who independently observe physical systems. Thus, [this] interpretation of QM rests upon psychological assumptions which are almost certainly false."[74]

The 1977 paper, written after most of the LHV experiments were completed, describes an experimental test of the 'consciousness' interpretation, and cites another (unpublished) experiment carried out by another of Shimony's students. The results of both experiments were negative, showing no evidence of any effect of consciousness.

Given that Shimony had already dismissed this interpretation in 1963, one might wonder why he chose to test it some years later. In the 1977 paper, two reasons are given. The first is that difficult problems, such as the interpretation of QM, often require radical solution. The second reason is very illuminating and is worth quoting in full:

"A second reason is methodological. We feel that there is value in exhibiting concretely how a radical hypothesis can be subjected to careful experimental scrutiny. The thesis that the scientific method combines openness to theoretical innovation with a critical insistence upon experimental test, is generally accepted, but exemplifications of this thesis are uncommon and instructive."[75]

Clearly, Shimony is largely, if not predominantly, concerned with making this general methodological point, and the consciousness interpretation simply provides a useful exemplar. Shimony's earlier (1963) comments suggest that the results of this experiment came as no surprise to him, and (as discussed in Chapter Two) the design of this experiment is not without shortcomings.

If it is true that this paper is at least partly a challenge to non-empirical FQM then the fact that it was published in Foundations of Physics is particularly appropriate. This journal provides a forum for many non-empirical theories and speculations; de Broglie contributed to its first issue[76], and much of Bohm's recent work has appeared there[77]. This journal has an explicit editorial policy of giving a favourable reception to speculative work; in the first issue, the editors noted that:

"very few scientific journals today encourage speculation not tied to hard and demonstrable facts. One wonders whether brilliant ideas are not lost by this restrictive attitude. Foundations of Physics will publish with suitable frequency disciplined speculations suggestive of new approaches in physics."[78]

Shimony was not alone in his belief that an experimental approach was very important for FQM. I have already quoted Clauser's statements that

"[we] have really made this thing respectable experimental physics, which it was not when we started"

and that this field required a very high standard of work because "we've got to redeem ourselves from a generation of quacks." Like Shimony, Clauser actively extended the experimental methodology into other areas of FQM.

Neoclassical Radiation Theory (NCRT) had been developed by Jaynes and other theorists in a number of papers published during the 1960's and early 1970's.[79] It is well known that QM and its extensions (such as quantum electrodynamics) require quantization of the electromagnetic field. A number of phenomena, such as the photoelectric effect and the black-body radiation spectrum, are routinely cited in physics textbooks as 'proof' of the need for quantization. Jaynes and his colleagues developed a new theory which did not invoke quantization (hence the term 'neo-classical'), yet which could account satisfactorily for the above, and many other, physical phenomena which apparently require quantization. They did not perform experimental tests of NCRT; they simply showed that this theory generated the same prediction as QM and quantum electrodynamics, arguing that we should not be too complacent in our acceptance of the evidence for quantization.

In 1972, Clauser published a paper[80] in which he argued that NCRT did not agree with QM's prediction for polarization correlations in

LHV-type experiments, and that experimental data, such as Kocher and Commins' experiment[81], and Clauser's own experiment with Freedman[82], supported the QM predictions.

In a later paper,[83] Clauser again discussed NCRT, noting that it disagreed with QM in its description of the physical processes which occur when light strikes a semi-silvered mirror, or any similar beam splitter. According to QM, light is quantized into discrete photons, and individual photons cannot be split, whereas a classical wave, no matter how weak, can always be split into two components of non-zero intensity.

Rather than take the attitude that 'QM must be correct', Clauser claims that alternative theories such as NCRT emphasise:

"the importance of experimentally demonstrating phenomena which require a quantization of the electromagnetic field."[84]

Although the half-silvered mirror is a classic textbook thought-experiment, Clauser points out that QM's prediction for single photons hitting such a mirror had never been rigorously tested experimentally,and he claims that this is unsatisfactory:

"That a photon is not split in two by a beam splitter is certainly 'old hat', and it may seem surprising that we have gone to the effort to test this prediction experimentally. What is in fact much more surprising is that evidently no such experimental test has heretofore been performed, and such tests are clearly of great importance."[85]

Clauser then shows that a LHV-type experiment <u>can</u> test this prediction and that NCRT is falsified. The statements above strongly suggest that Clauser, like Shimony, felt that an experimental approach was relevant not only to LHV but also to other aspects of FQM.

Only Clauser and Shimony have actively applied an experimental methodology to other FQM topics, but it seems clear that virtually all the LHV physicists shared their preference for experimental approaches to fundamental questions. Now that the experimental evidence is clearly in favour of QM, further experiments appear, to most people[86], rather unattractive:

"We all came away feeling that the theorists had to think up a new idea that would give experimentalists something to work on, other than just pushing the same idea into better and better experiments."[87]

Some interviewees made the even stronger suggestion that the LHV group had primarily been interested in the experiment, and that many of them had had very little liking for the particular theory being tested.

"In many ways, the activity of the last ten years over Bell's inequality has been a tempest in a teapot. It's just that people have been so happy at finding something they can actually do and think about that is more concrete and more in line with what other physicists do that there was an over-reaction to it....I don't think [LHV] was likely from the start."[88]

and

"I never liked those [LHV] theories....they were ugly, tentative, and clumsy."[89]

Many interviewees suggested that if a 'good idea'[90] came along, which could be experimentally tested, many of the LHV group would be willing to perform such a test. For example, Papaliolios, who was personally acquainted with most of the LHV group, said:

"Most of the people who've done these experiments are now in other areas....you can't be an experimentalist in hidden variable theories because there aren't that many experiments....If someone were to think of a new class of experiments to do, I think you'd find all of these people would immediately jump into the field. The interest is there, it's just the opportunities aren't."[91]

Impact of LHV on Other FQM Activity.

What impact has the LHV activity, and its challenge to non-empirical methodologies, had on other FQM workers? It would certainly be difficult for them to ignore this work completely, since it has been discussed not only in high-prestige physics journals such as Physical Review and Physical Review Letters, but also in review journals such as Reports on Progress in Physics, and Comments on Atomic and Molecular Physics, and in 'popular' scientific journals such as Scientific American and New Scientist, and in virtually all recent books on the interpretation of QM. The experimenters' claim to have made this work 'respectable' seems to be justified.

In looking at the behaviour of other FQM workers after LHV, it is difficult to prove that LHV exerted a causal influence. However, in the years since LHV began, a number of other FQM workers have tried to get their ideas experimentally tested, and in many cases these workers seem to have been directly influenced by LHV.

For example, Fitchard, who was formerly a student at Texas A & M University while Fry was performing his LHV experiment there, developed an experimental proposal to test the Heisenberg Uncertainty Principle. His publishing strategy was as follows:

"I'm sending my proposal to Phys Rev Letters first....the original LHV proposals were in Phys Rev Letters, so why not?....Since I'm proposing an experiment, I'd like to have lots of experimenters see it....The vast majority of physicists don't study foundations, and the only thing that's going to convince them is an experiment. That's why these experiments like Fry's are so important."[92]

Pearle, who had helped to put Shimony in touch with Holt and Pipkin, and who had published a paper on the interpretation of the LHV results[93], felt that the LHV experiments would have an important social influence on the field.

"I don't think LHV theory was likely from the start, but the experiments might be historically important, in the sense that they get people to think that this can be done, that you can question QM, and come up with a do-able experiment which can check it, and it was important to do it."[94]

Pearle published another paper on his own proposed modification to QM. This theory was similar to the Bohm-Bub theory, in that it proposed to solve the 'reduction' problem by adding a non-linear term to the Schrödinger equation. Unlike Bohm and Bub, though, Pearle was very keen to make his theory empirically testable:

"I have to make the theory more concrete. I've got to find a physical basis and an experimental test....I have some ideas, but I'm not an experimentalist. I'd like to talk to Clauser or someone else who does way-out experiments, that's the kind of guy I need to tell me whether it's feasible."[95]

To quote a third example, Mugur-Schachter, a theorist who was a former student of de Broglie, has published a number of papers in FQM. In 1977, for the first time, she included an experimental proposal to test one of her ideas. In the acknowledgements section of this paper, she notes

"I am grateful to Dr J. S. Bell for important remarks on the possibility of an experimental study."[96]

These three examples provide fairly clear evidence of theorists who had direct contact with LHV workers, and who were thus led to look at experimental tests more closely. Not all recent experimental proposals can be linked with LHV in such an obvious way[97]. As a possible 'intermediate' case, consider Matthys' PhD thesis, in which he investigates the feasibility of an experimental test of the Heisenberg Uncertainty Principle[98]. Matthys cites the work of Bell and others on LHV, and writes:

"The importance of performing potential experimental tests of these matters whenever they become technically feasible, hardly needs to be emphasised....Perhaps the main reason why the debate over the meaning of the uncertainty principle....seems to perdure [sic] endlessly, is that it may appear to be more a philosophical than a physical question....While Gedankenexperimente may be useful in criticising a theory or developing its implications, they are unsatisfying as a proof. The desire remains to find a specific physical measurement that can be used as arbiter between interpretations of the theory."[99]

The connection between this thesis and LHV is slim but perhaps non-negligible. Matthys' PhD supervisor was Jaynes, who three years earlier had seen his NCRT theory (and LHV) criticised by Clauser's experiment. This episode certainly impressed Jaynes at the time. Speaking at a conference in 1973, he said

"A perfectly harmless-looking experimental fact....can, at a single stroke, throw out a whole infinite class of alternative theories.. ..The mind boggles at the thought that any such thing could be possible....if the [argument] survives scrutiny, and if the experimental result is confirmed by others, then this will surely go down as one of the most incredible intellectual achievements in the history of science, and my own work will lie in ruins."[100]

It is not possible to <u>prove</u> a causal relationship between LHV and all subsequent experimental proposals. However, the evidence presented here does indicate that LHV has had an important influence on many FQM theorists. Yet it is by no means the case that <u>every</u> FQM theorist is now adopting an empirical approach. There are a number of other responses.

Some FQM authors stress the need for assumptions in the LHV experiments. (A detailed discussion of these assumptions and their justifications will be presented in the next chapter.) Such authors concluded that experimental approaches, by themselves, are insufficient to solve the problems of FQM:

"These experiments do not represent really direct tests, the fundamental process being always mediated by other phenomena (....which introduce extra elements and 'distort' the initial phenomena), so that one can wonder to what extent this type of experiment is really convincing. Therefore, it seems to me, that one is brought back after all....to the 'epistemological' discussion, as old as QM, on its interpretation, the significance of its concepts, and the possible insufficiency of those we have at the moment."[101]

Other theorists are even more critical of LHV. For example, Bub argues that the theoretical basis of the experiments is erroneous, and he claims:

"The Clauser-Horne-Shimony-Holt experiment, designed to test the Bell inequality, is therefore pointless."[102]

(Bub's arguments have themselves been strongly criticised by LHV workers.[103])

A number of FQM theorists told me in interviews that there seemed little point in testing a theory which conflicted with QM in existing domains. Although they were sympathetic to critiques of QM, they accepted the view (criticised by LHV workers) that QM was unlikely to be falsified quite so easily. They felt it was better to construct alternatives to QM which did not diverge from QM in existing domains. This attitude is most clearly expressed by Flato:

"One can of course check experimentally if it is the quantum prediction which is correct, or the [local] hidden variable prediction. This point of view....taken by some people quite seriously....seems naive to me. QM was proved up till now to be very successful (whenever applicable). Hidden variables (if they exist) should be physically meaningful, able to reproduce all predictions of QM, and have extra predictions in domains in which QM cannot solve 'everything'."[104]

This passage is taken from a book which contains theoretical <u>and</u> experimental papers on FQM. The clash of viewpoints contained in it was noted by a reviewer, Maiocchi. On the one hand, he quotes the claim by LHV experimenters that

"the problem of the validity of LHVs rests with the experimentalists."[105]

Maiocchi notes

"the eighteenth-century view of the function of crucial experiments which is at the bottom of [such] highly debatable statements."[106]

On the other hand, he characterises the alternative view as one in which

"the new theory appears as a 'research programme' still in its early stages, and it does not seem fair to ask it to be prematurely confronted with the experimental results which the 'older' QM has already faced and assimilated. Only a myopic empiricism can claim that a theory still being elaborated should be capable of explaining the totality (or near totality) of the available empirical evidence."[107]

As a result of the LHV experiments, the contrast between the experimental methodology and the methodology of 'traditional' FQM has been made more explicit. Not surprisingly, perhaps, not all the LHV experimenters are impressed by this defence of a non-experimental approach. As one of them put it:

"Obviously, there's what I call the lunatic fringe, that no matter what you do they say 'Oh sure, but all we have to do is go to a nonlocal theory, and all your results mean nothing'."

This is a rather uncharitable view of FQM, which ignores the historical fact that all hidden-variable theories which were the subject of serious theoretical analysis prior to LHV were non-local theories. Bohm's work is an obvious example of this. In a sense, then, the LHV activity was genuinely irrelevant for many FQM theorists. However, the social impact of LHV made it difficult for theorists to ignore it. As one theorist put it:

"The experimenters were extremely able and active propagandists, which helps their point of view but not ours. They've drawn everyone's attention away from work like ours."

While some theorists criticised the experimental methodology as 'naive', others produced more positive defences of non-empirical methods. For example, one interviewee, describing his own philosophical analysis of QM, told me

"Of course, this isn't empirically testable, but a study of the interpretation of QM cleanses the theory and avoids a lot of wasted time. Not all mistakes are empirical, many are formal.... A study of the interpretation doesn't lead to better testable predictions, it leads to clearer statements....it's useful because it can prevent people making mistakes."[108]

Other interviewees mentioned additional criteria, such as simplicity, objectivity, and coherence, which they believed were as helpful as empirical results when it came to assessing and developing interpretations of QM. One of the fullest defences of a non-empirical methodology to appear since the LHV experiments was written by Bohm, in the book which was reviewed by Maiocchi. Bohm writes

"In my view, the essence of what the theorist does is in the creative act of insight, and not in the detailed hypotheses that may follow."[109]

He points out, for example, that the theory of the luminiferous ether was never definitively disproved by experiment, but that it was abandoned by Einstein, in favour of a 'new insight', because it was leading to a "serious lack of clarity."[110]

Discussing his current theory, Bohm notes that "a great deal of work remains to be done before our insight will reach a point at which it will be fruitful to make well-defined and experimentally testable hypotheses."[111]

Of course, Bohm had been defending his approach in similar terms as far back as 1952 (see Chapter Four). It would therefore be an exaggeration to claim that such a defence in 1976 was prompted solely by the LHV experiments. However, at least one feature of Bohm's argument in 1976 is novel. Discussing the results of the LHV experiments, which support the idea of non-locality, he points out that his own 1952 theory was also non-local, so that the experimental results "definitely fit in with" his ideas. He concludes "the need for new forms of insight into this question is made even sharper and clearer than it was before."[112]

Bohm was not the only author to use this tactic. Other theorists were apparently well aware of the rhetorical or persuasive force of experiments, and have used the experiments to support theories other than QM. Logically, after all, any experimental falsification of one theory is also a corroboration, not only of QM, but also of a large, possibly infinite, set of other theories whose predictions happen to agree with those of QM for the experiment in question.

As an example of this tactic, the Mexican physicist de la Peña, together with his colleagues, postulated that the values of hidden variables might be affected by the act of measurement. This sort of LHV theory, they argued, was not falsified by existing LHV experiments. Indeed, they claim that

"the correct conclusion to draw [from the experiments] ....is that QM is not reducible to LHV theories in which the measuring process cannot affect the measured system. The experiments only serve to confirm the existence of these effects."[113]

An even more extreme example of the use of the LHV experiments to support unorthodox theories is provided by Reuven Opher in his 'dybbuk theory' proposal. Opher changed his name from Raymond Fox in the early 1970's. Under the name of Fox, he had published several proposals for LHV experiments.[114] In 1975, Opher published a paper[115] in which he postulated that quantum-mechanical wave functions were reduced under the influence of cosmic particles called dybbuks. (The term comes from the Hebrew word for a wandering spirit.) Dybbuks, if they exist, possess imaginary energy, imaginary momentum and imaginary mass. Discussing Freedman and Clauser's LHV experiment, Opher notes that

"The [dybbuk] theory predicts that an EPR paradox experiment which has a space-time interval between the analysers $>> (\delta, t)$ should obtain the results of conventional QM. This was the result of the experiment....in agreement with the above theory."[116]

Thus an experiment which seemed to its originators to support QM is being cited in such a way as to seem to lend support to Opher's rather unorthodox theory. This was not what the LHV group had in mind when they described the experimental method as decisive. Evidently, the rhetorical force of an appeal to experiment can be applied in more than one way.

In this section, I argued that the LHV activity altered the social context in which FQM operated, by presenting a paradigm case of <u>experimental</u> FQM. Some theorists were encouraged to follow this example, either because they felt that this methodology 'works', or because they felt it was a way of gaining respectability and recognition. Other theorists perceived LHV as a direct challenge to their own methodology, and their response was to openly criticise the methodology, not the content, of the LHV work.

As we saw in Chapter Four, such methodological differences underlie many disputes in FQM. What makes the LHV case special is the impact which LHV had, and the way in which <u>both</u> sides in the dispute (theorists and experimenters) dealt explicitly with methodological issues, rather than (as in previous examples) leaving such issues implicit. Because of these features of LHV, theorists were virtually obliged to respond to it. This does not mean that they were compelled to adopt experimental methods, as we have seen. By introducing a new factor into the social context of FQM, LHV opened up a whole range of new strategies.

<u>Changes in Editorial Policy Towards FQM</u>.

I have suggested that the social context of FQM - specifically, attitudes to non-experimental methods - has been strongly influenced by LHV. Now that the LHV activity is nearly complete, will these attitudes persist? One way in which such attitudes might become entrenched is by being incorporated into the editorial policy of FQM journals.

I have already quoted the editorial in the first issue of Foundations of Physics, in which it was claimed that speculative work would be favourably received by this journal (see Chapter Four). Other 'non-empirical' journals have also appeared in recent years. For example, the International Journal of Theoretical Physics, first published in 1968, has an explicitly tolerant policy towards 'philosophical' approaches. To quote from the first editorial,

"It is the intention to publish papers that carefully examine .... problems from the standpoint of either the physicist or the philosopher, and the inclusion of such philosophical detail as is necessary for elucidatory purposes will be accepted for publication."[117]

More recently, the Journal of Philosophical Logic first appeared in 1972. Its first editorial stresses the connection between physics and philosophy which operates through the intermediate subject of 'quantum logic':

"In my opinion, the virtual schism that has existed between the practise [sic] of quantum logic and what everyone understands to be part of philosophical logic is about to disappear. The interaction of logic with the foundations of physics will possibly be as fruitful as that with the foundations of mathematics."[118]

While these new avenues were being opened up for non-empirical FQM, some existing avenues were being closed down. In 1972, an editorial entitled 'Regarding Papers about Fundamental Theories' appeared in Physical Review. The concluding paragraphs begin as follows:

"We shall no longer accept papers of this type for the Physical Review. There will, of course, always be borderline cases, and we shall therefore give a very rough outline of the criteria and characteristics such a paper must have so that we may consider it."[119]

Several conditions are set which require that any assumptions used must be justified by their explanatory or heuristic power. The editor continues:

"Moreover, the author must show that the new assumptions do not contradict existing experimental facts. He must also investigate possible new consequences of his assumptions, and whether these could be tested by new experiments. It should not be overlooked that physics is an experimental science. In spite of the temporary hegemony of theorists, no physical theory is significant unless it can be related to experimental data."[120]

In 1970, Reviews of Modern Physics made another interesting editorial comment, when it published Ballentine's paper[121] on the statistical interpretation of QM. In a preface to this paper, the editor pointed out that his referees were unable to agree about its value. He claims that

"The subject of the following paper lies in the border area between physics, semantics, and other humanities."[122]

Unlike the editor of Physical Review, quoted above, the editor of Reviews of Modern Physics .

"does not feel called upon to draw the border of physics firmly or restrictively, the more so as the subject seems to maintain a broad appeal after 45 years of QM."[123]

However, the Editorial Comment ends with what might appear to be a warning:

"The Editor is experimenting with this note, trying to convey the flavour of controversy surrounding an unusual paper. He might take a different attitude on future occasions."[124]

I do not wish to imply that this apparent tightening of methodological boundaries in FQM was caused solely by LHV. The chronology of events is sufficient to render such a claim implausible. Neither would it be true to say that these shifts in editorial policies were a major cause of the explicit methodological focus in FQM in recent years. The statements by FQM workers about methodology often refer specifically to LHV, whether as an inspiring example of how to do FQM or as an example of how not to do FQM. Editorial policies of journals were seldom mentioned by interviewees as an influence on their behaviour, and such references are completely absent from published comments about the choice of methodology.

Nevertheless, statements of editorial policy may provide useful rhetoric, and actual editorial policies may exert important influences on the development of FQM. At the very least, journals may serve a labelling function. For example, when Clauser speaks of making LHV into 'respectable experimental physics', he may be referring to the use of a particular methodology, but the 'respectability' of this methodology, in his eyes, follows from his social location, and his personal history, in the physics community. At another level, respectability might be measured by the ability to have one's work accepted for publication in the Physical Review, though as we have seen, this acceptance is explicitly based on specific methodological criteria.[125]

It is too early to say for sure what effects these changing editorial policies, and the emergence of new journals, will have on FQM. They may contribute towards a differentiation of FQM into mutually exclusive subgroups with different methodologies. Alternatively, they may simply help FQM workers to identify the most appropriate location for work of various sorts, and so aid the flow of information throughout FQM.[126] Irrespective of the details, it is clear that the emergence of new journals, and policy changes in old ones, illustrate other ways in which the social context of FQM is changing.

Discussion.

Throughout this chapter, I have been concerned with demonstrating the ways in which individual scientists come to terms with their local social context, and how they adapt their behaviour to meet the demands of that context.

The examples provided also clarify the distinction between 'local' and 'global' context. There is not a dichotomy between micro and macro contexts; indeed, these two terms are better described as sections of a continuum which ranges from the individual through his workplace, his professional subgroup, and his status as a scientist, to his country and his cultural milieu.

Nevertheless, there is at least an intuitive distinction between general features of science, and features which are specific to a particular piece of scientific activity such as LHV. Early studies in the sociology of science mainly dealt with general features, such as the reward system, the communication system, disciplinary boundaries, and other structural features[127].

The central argument of this chapter is that, by ignoring the detailed microsociology of science, such studies provide an inadequate picture of the process of scientific investigation. For example, the style and content of LHV papers reflects not only general conventions of scientific writing, but also specific local features of the LHV activity, such as its historical association with FQM, the status of FQM, and the perceived audience for LHV papers.

Since there is no sharp dividing line between local and global contexts, we find that actors cite a whole range of motivations and reasons for their actions, some dealing with very particularistic features of their context (Holt's reference to Pipkin's earlier mistake; Clauser happening to meet Kocher), some related to more general aspects of LHV (Fry appealing for money to resolve an apparent conflict), and others referring to the wider context in which LHV occurred (Shimony and Clauser urging wider use of experimental methods to improve the reputation of the field; experimenters' claims that this work was misunderstood by their colleagues; theorists' recognition of the rhetorical power of experiments).

Nevertheless, all the factors listed above, together with the others discussed in this chapter, are clearly connected in a very direct way to the actual piece of scientific practice being studied, and these factors must be given their proper role in any satisfactory account of this practice.

Two other important findings emerge from this chapter. The first is the active nature of the individual's interaction with his context. In Chapter Three, we saw how QM could be used to generate a whole range of accounts, corresponding to the different aims of the people constructing these accounts. Even when physicists were responding to direct political pressures, as in the Soviet Union, the products of their activity illustrate the active, selective, and creative features of this response. In the present chapter, we have seen many examples of negotiation and manipulation of the environment, such as Clauser's entry into Commins' laboratory, Shimony's recruitment of Holt, Holt's extended study of his apparatus, the presentation of LHV as 'decisive', and so on.

The second finding is that the products of scientists' activities themselves became part of the environment with which other groups had then to come to terms. This point may be uncontentious when applied to knowledge-claims: yesterday's discoveries are incorporated into today's practice and tomorrow's textbooks. Yet the same conclusion applies to non-cognitive elements of the context.

For example, LHV did not alter the cognitive structure of theoretical FQM, since no FQM theorist believed in LHV anyway. At the end of the day (so the theorists claimed) we knew nothing more than we had done originally. Yet LHV did drastically alter the social context of FQM. As an exemplar of "how to do 'good' FQM", LHV could not be ignored, though of course different people were able to cope with it in different ways.

Chapter Six

The Evaluation of Knowledge

Introduction

In Chapter Five, I concluded that many aspects of the LHV physicists' behaviour could only be satisfactorily accounted for by referring in some detail to particular features of the social context in which this behaviour occurred. In the present chapter, I shall extend this claim to cognitive aspects of the LHV activity. That is, I shall argue that a satisfactory account of the way in which physicists draw substantive conclusions about the validity of hypotheses, the implications of experiments, and so on, cannot rely on a generalized model of 'scientific method'; instead, such an account must relate these conclusions to the specific context in which they were formed.

The first section of the argument examines the assumptions which physicists were forced to make because of limitations of their apparatus. Such assumptions had to be justifiable if the experiments were to have any validity as genuine tests of LHV. By examining such justifications, I shall show that the evaluation of the assumptions was strongly influenced by the cultural background shared by the LHV group. This culture provided a set of criteria by which the assumptions were assessed. Although physicists were unable to fully articulate their reasoning, it seems clear that this reasoning was given meaning and validity by (mostly tacit) references to 'what everyone knows'.[1]

I shall then examine physicists' evaluation of Holt's experiment. In keeping with the analysis of Chapter Five, I will argue that physicists had very good reasons for rejecting Holt even in the absence of further experimental results, though such reasons can only be seen as 'good' from within the particular culture of these physicists. Extending the argument, even the accumulation of new results (in favour of QM) does not necessarily (in strictly logical terms) invalidate Holt's result. However, the rejection of Holt's experiment, while perhaps not inevitable, is both meaningful and predictable given the context in which these experiments occurred.

An analysis of Holt's reception is made much easier by examining the reception of the other 'anomalous' experiment, performed by Faraci and his colleagues. Unlike Holt, Faraci's group defended their result, and the reception of this result differs in some interesting ways from that of Holt. This episode raises the important question of competence. As we have seen, Holt's concern with defending his reputation as a competent experimenter was an important influence on his behaviour. The different reception of these two experiments strongly suggests not only that an experimenter's reputation can be affected by the evaluation of his knowledge-claims (as Holt realised) but also that the evaluation of knowledge-claims can be affected by prior, or simultaneous, evaluation of the competence of the persons making the claim.

Throughout this chapter, I will be concerned with the way in which culture can affect the evaluation of knowledge. I shall introduce the concept of plausibility as a useful way of conceptualising this interaction. Referring to an idea as 'plausible' rather than as 'true' has a number of advantages. In the first place, it is more in keeping with a relativist or naturalistic methodology. It avoids any absolutist connotations while preserving the possibility that certain ideas may be accepted unanimously by a certain social group at a certain time. Even if some ideas are indeed true, such ideas are presumably also considered to be plausible by someone who holds them to be true.[2] Plausibility, then, should not alienate realists, yet it allows relativists to discuss concepts such as consensus and psychological certainty without having to introduce the concept of 'truth'.

My argument will be that in evaluating knowledge-claims, irrespective of whether conflicting claims are present, physicists approach the problem with a pre-existing culture which not only provides them with an observation language, a methodology, and so on, but also with a set of expectations of what is likely to be 'true' and what is unlikely to be 'true'.[3] In other words, culture provides a plausibility structure which actors use, at least in an informal, qualitative sense, to assign probabilities to different accounts. In the LHV activity, I shall try to show the operation of this plausibility structure in the evaluation of knowledge-claims.

Simply because culture seems to limit action, we should not suppose that actors simply respond passively to such constraints. In the previous chapter, we saw how Holt, Clauser and Fry actively manipulated features of their environment in order to achieve their aims. Such resourceful activity is also possible in the cognitive domain. I shall examine one physicist, Aspect, who seems to have altered the plausibility structure of this area of physics, not by discovering a novel result, but simply by behaving in a particular way. This episode will allow us to develop a fuller picture of the operation of culture in the evaluation of knowledge.

Obviously, it is rather simplistic to speak of the plausibility structure as if every physicist received the same training and held the same set of expectations. We have already seen that the LHV group did hold many attitudes in common, and that they shared a commitment to experimental methods. However, it would be surprising if there were no points on which they disagreed. I shall therefore go on to examine some aspects of LHV in which consensus was not obtained; I shall use this evidence to show the limitations of the plausibility structure as a determining factor, but also to demonstrate that where there is no shared plausibility structure, we should not expect to find consensus.

Finally, I shall introduce some purely speculative ideas concerning alternatives to the actual outcome of the LHV activity. If things could have turned out differently from the way they did, then it may be useful to examine such alternatives, comparing them with the actual outcome, as yet another way of investigating the factors which actually did cause the outcome we observe.

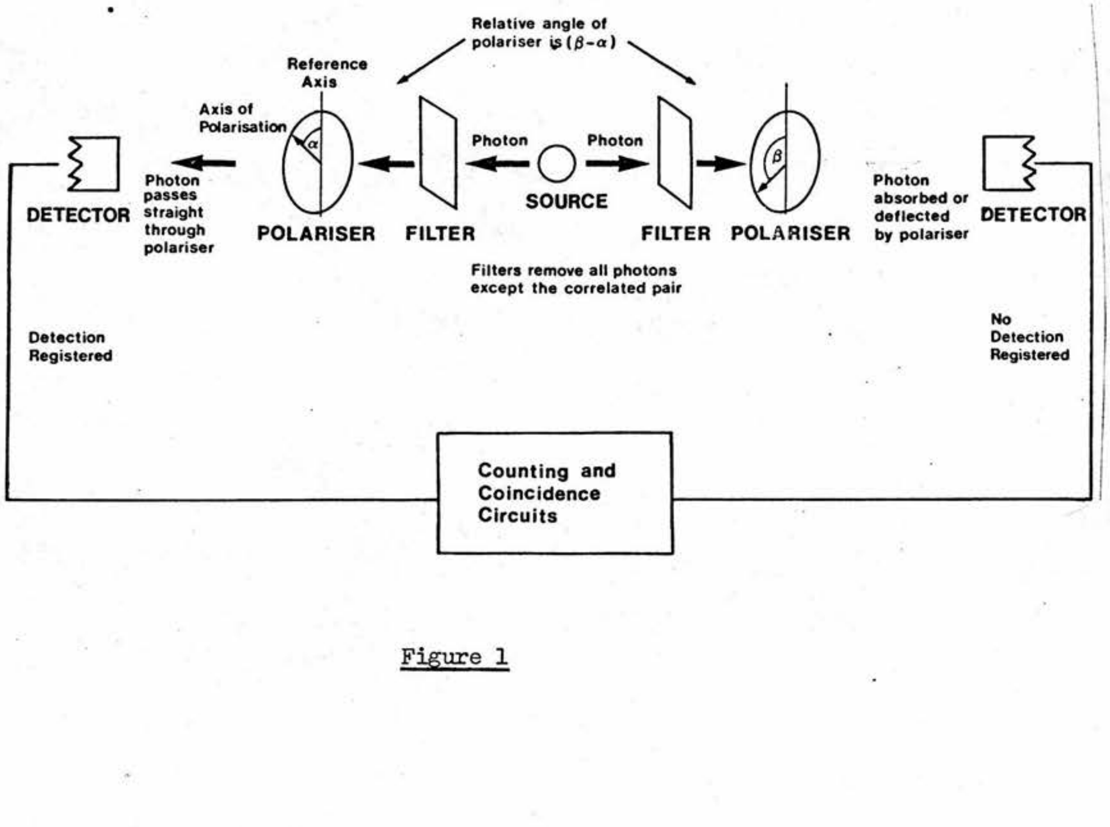
The Plausibility of Assumptions

Technical details of the LHV experiments were provided in Chapter Two; for the reader's convenience, the relevant aspects of that discussion are reproduced here. In addition, in order to fully develop the argument, some additional technical information is required.

Bell's work[4] showed that LHVT and QM gave slightly different predictions for the strength of correlations between the properties of certain pairs of particles. However, any experiment designed to discriminate between the theories had to use existing, imperfect apparatus. For example, one way of testing the theories would be to measure the correlations between the polarizations of two photons. Ideally, any such experiment would consist of a source of photon pairs, produced in such a way that, at least according to the QM predictions, there would be a strong degree of correlation between their properties. The photons must then enter polarization analyzers, which would allow photons in certain polarization states to pass through unaffected with no losses, while absorbing or deflecting all the photons which are in certain other polarization states. Photons emerging from the analyzers must then be detected with perfect efficiency. In addition, all other optical components (for example collimators and filters) must not absorb or depolarize any photons. Figure 1 provides a simplified picture of the experimental design and illustrates the fate of two photons with different polarizations.

In fact, it was impossible to attain this idealized procedure using existing equipment. Although a number of different experimental designs were available, each of them fell short of this ideal in at least one of the above respects. These imperfections made it impossible to discriminate between LHVT and QM in the way Bell described. However, it proved possible to modify Bell's results so that the two theories still gave measurably different predictions even in realistic experimental conditions: but the validity of such modifications could not be proved. It was necessary to assume various things about the apparatus. (As we shall see, the exact nature of the required assumptions depended on the details of the particular experimental design in question.)

Every scientific experiment depends upon certain assumptions, many of which, in most cases, remain implicit. For example, experimenters who periodically check that their apparatus is functioning correctly must assume that this continues to be the case between one check and the next. During data analysis, experimenters may draw lines joining points on a graph, with the implicit assumption that the areas between or beyond their actual data points are correctly described by their lines. The need for assumptions per se in the LHV case is not remarkable. As two authors expressed it:



Relative angle of polariser is $(\beta-\alpha)$
Reference Axis
Axis of Polarisation
α
β
Photon
Photon
SOURCE
DETECTOR
Photon passes straight through polariser
POLARISER
FILTER
FILTER
POLARISER
Photon absorbed or deflected by polariser
DETECTOR
Filters remove all photons except the correlated pair
Detection Registered
No Detection Registered
Counting and Coincidence Circuits

Figure 1

"Any argument whose scope is strictly limited to a discussion of ideal systems is of little value to working physicists, who endeavour to describe systems that can and do occur in practice."[5]

Nevertheless, the assumptions required in the LHV case are not trivial. There is more than simply extrapolation to be considered. A better analogy might be with the user of an electron microscope, who has to make not only 'routine' assumptions about accuracy and the proper functioning of his equipment, but also more important assumptions about whether a particular image represents a real object or an artefact of the preparation and viewing process.[6] The LHV physicists were dealing with postulated entities (the 'hidden variables') about which, by definition, very little was known. One had therefore to be very careful when making assumptions about pieces of apparatus whose behaviour might, after all, be governed by unknown entities.

Before we examine the details of the required assumptions, we should note that the LHV physicists were quite explicit in admitting that the assumptions could not be proven; the best that could be hoped for was to make them as plausible as possible:

"These [experiments] , of course, need auxiliary assumptions to be valid. As a result, their value depends on how reasonable the assumptions are."[7]

"There seems to be no recourse but to make a plausible assumption."[8]

Physicists were now faced with a gap between what they wanted to say - namely that the experiments would constitute a decisive test of LHVT - and what they could justifiably argue on logical grounds. How did they evaluate the plausibility of their assumptions in order to make this gap as small as possible?

Choosing an Assumption

The evaluation of plausibility in this case can be described as a two-stage process. The first stage was to examine the range of possible assumptions, each corresponding to a different experimental design, and choose the most plausible. The second stage was the construction of arguments which would maximize the plausibility of the chosen assumption. This distinction is both historically and logically useful. After all, it is one thing to produce a rank-ordering of candidates, and quite another to decide whether the top-ranking candidate is good enough to pass!

Let us consider two methods of testing LHV which were studied by the experimenters. Both involved measurements of the correlations between the polarizations of photon pairs, and thus both required methods of photon detection and polarization analysis.

One design used high-energy photons (produced by electron-positron annihilation) which could be detected with great efficiency but for which no simple method of polarization analysis existed. However, the way in which photons are scattered after hitting a target was known to depend on their polarization, so that a study of scattering behaviour could provide an indirect means of inferring the polarization of the photons. Unfortunately, a detailed description of the relationship between scattering and polarization could only be derived from QM, and since QM was one of the theories being tested the validity of this description was open to question. Clearly, there is some circularity involved in the use of QM to analyze data which are then used to check the accuracy of QM. In order to perform the experiment, one had to assume that this circularity was not vicious; that is, one assumed that whatever else might be wrong with QM, its numerical predictions for the scattering - polarization relationship were correct.

The alternative experimental design used low-energy photons (produced by atomic cascades) for which efficient polarization analyzers existed, but for which existing detectors had low efficiency. Only a small fraction of the photons which arrived at a detector would be counted. One therefore had to assume that the photons which were detected did not constitute a biased sample of the photons arriving at the detector. A more precise way of stating this, which was used in an experimental paper, is that one must assume that when a pair of photons successfully passes through the polarizers, the probability of their joint detection is independent of the polarizer settings.[9]

Nearly all observers felt that the high-energy assumption was the less reliable. However, it is difficult to find in print any rigorous arguments in favour of one assumption rather than the other. Both are in agreement with past experience, and in both cases it is possible to construct counter-examples - that is, LHV theories which would not be experimentally distinguishable from QM because of the imperfections in the apparatus. The comparisons between assumptions which do appear contain many explicit references to 'reasonableness'. Here is one example:

" For [high-energy experiments] a simple counterexample suffices to show that rather strong additional assumptions are needed here.... counterexamples for [low-energy] experiments are also possible, but they are much more difficult to produce. Correspondingly, the required additional assumptions are considerably weaker and more reasonable and the experiments more conclusive. This judgement is of course subjective, but it is reasonable."[10]

The underlined words in this statement are clearly evaluative, yet no basis for this evaluation is given, other than appeals to reasonableness. When tackled on this point in interviews, physicists invariably replied that this was indeed a subjective judgement, but one which was not difficult to make.

In a sense, this was not an important judgement, since both types of experiment were in fact performed. But why would anyone choose to perform a high-energy experiment if it was thought to be more open to question? The answer of course is that these deliberations did not take place in a vacuum. One group had already begun a high-energy experiment before its members knew of Bell's work, and the experiment was then adapted to the task of testing Bell's proposal. (See Chapter Five pp.132-33 for further details of this 'switch of allegiance' by Kasday and his colleagues from Friedberg to Bell.) As a result, this group was already committed to using high-energy photons.

Despite the apparent agreement that low-energy experiments used weaker assumptions, Kasday and the other members of the high-energy group were reluctant to concede that their results were not both valuable and reliable. For example, Kasday pointed out that even if LHV was correct, its predictions agree with those of QM in most areas, so that most of QM's predictions would still be trustworthy. In addition, Kasday devotes seven pages of his thesis to a review of independent experiments which support the QM predictions for the scattering-polarization relationship, concluding that "these experiments taken together provide strong evidence"[11] in favour of his assumption.

Other high-energy experimenters argued that the two types of experiment were complementary, and that testing QM in two energy ranges was better than testing it in one, even if the assumptions required in the higher energy range were somewhat stronger.[12]

Another member of Kasday's group, Ullman, stressed that all the experiments required assumptions:

"Some of the low-energy experimenters have said things like 'our experiment tests LHV and yours doesn't.' We weren't convinced by this. They had to use assumptions too, though different ones.... Probably ours are a little more shaky, but no-one's succeeded in even proposing an experiment that doesn't have some kind of a loophole which you can invent some hidden variable explanation for."

Choosing one or other assumption meant choosing one or other set of apparatus, and in the case of Kasday's group the latter preceded the former. This was an exception; the low-energy experimenters were in a position to choose between assumptions before they began to plan their experiments. Even here, though, we must not neglect the realities of life. Few scientists are in a position to purchase whatever equipment they wish. As discussed in Chapter Five, what actually happened was that Holt was recruited by Shimony and Horne simply because Holt had access to the necessary equipment, and Clauser moved from New York to California in order to gain access to similar apparatus. In a sense, then, the evaluation of rival assumptions was peripheral to the real negotiations over gaining access to equipment and funds. Had these 'real' negotiations produced different results, so that only Kasday's experiment was completed, it seems likely that his results, which were after all in good agreement with the predictions of QM, would have satisfied most people's curiosity about LHV.

Justifying the Chosen Assumption

Having decided which assumption to adopt, the LHV physicists then had to convince themselves and others that even this weak assumption was not sufficiently strong to invalidate any experiment. Shimony described the low-energy assumption as 'plausible' and defended it as follows. Part of the procedure in such an experiment is to measure the polarization correlations at a number of polarizer settings and to combine these measurements to give a certain number. LHV predicts this number will be less than zero, though it does not predict a specific value. QM, on the other hand, predicts a specific positive value for this quantity. Shimony argued that:

"If the experimental value turns out to be positive, a determined advocate of LHVTs could attribute the result to the falsity of the assumption. This special pleading would not be entirely unreasonable. However, if the experimental value is not only positive, but in very close agreement with the QM prediction, then the advocate of LHVTs would appear to be obsessive."[13]

In other words, it would not be 'entirely unreasonable' to claim that the sample of photons registered by the detectors is not completely unrepresentative of the total photon population, but it would be 'obsessive' to claim that the sample is so biased as to give a result which was completely contrary to the true state of affairs.

In another paper, Shimony and his colleagues concede that we cannot be <u>sure</u> that the apparatus is not behaving in a biased manner, since any experiment designed specifically to test this possibility would 'almost certainly' have a different set of values for its hidden variables compared with the values found in the correlation experiments. Thus any conclusions found in the former experiment need not necessarily be valid in the latter. Despite this lack of certainty, however, these authors feel that the assumption is reasonable, arguing that 'highly pathological detectors' would be required to render this assumption false.[14]

In interviews, physicists were asked to explain in more detail what was meant by a 'pathological detector':

"It's pathological because it would mean a detector behaving in a very unphysical, unreasonable fashion, something that one would not normally expect a detector to do."

"Pathological just means you can't really believe that such a mechanism is part of the world."

These statements exhibit the same sort of subjective, rather vague expressions as those found in published papers. It is highly unlikely that these physicists had other, more rigorous reasons for accepting their assumption which they neglected to state in print or in interviews. What seems more likely is that they simply found it unbelievable that apparatus could operate in such a selective manner, but they could provide no logically compelling justification for their views. Nevertheless, there is no doubt that for practising physicists, the assumption is indeed highly plausible. Because of their knowledge and experience, because of their immersion in the culture of physics, they felt strongly (even if they could not fully articulate this feeling) that the assumption was an unobjectionable one, and that experiments based on this assumption were not thereby devalued.

It would be wrong to imply that the LHV group were cavalier about the assumption. Several attempts were made to produce even weaker versions, with some success.[15] However, this effort was tempered by practical considerations. As one theorist said of his improved version:

"We haven't proved that's the weakest assumption you could make, but there's a point where it doesn't seem worthwhile to spend more time."

And to quote another theorist

"It's always difficult to get clean experiments. But there's a trade-off between important philosophical things and things we can do."

The predicament of the physicists is clear. Some sort of assumption was going to be necessary, and no matter how intuitively plausible it seemed, they felt obliged to provide as strong a justification as possible. What ensued were rhetorical references to the 'unreasonable', 'pathological', and 'obsessive' nature of any argument which attacked the assumption. This predicament, and the chosen solution, were clearly recognized by the experimenters:

"Whenever you see words like 'pathological' in physics, there's a reason for them, and the reason is you can't do anything about them."

"You use clever phraseology. You can really beat people down with the right words and impress the hell out of them."

Only in this way could the experimenters get down to the business of actually doing experiments, which was after all why they were involved. As long as the assumptions were weak enough for all practical purposes, as long as they seemed plausible, the experimenters were content to get on with the job.

The way in which the assumptions were evaluated illustrates a very important point. Physicists were convinced that certain beliefs were highly plausible, even though they could not articulate their reasons for this belief. Their conviction stemmed from their immersion in a shared scientific culture, a culture which

"legitimates and limits the parameters requiring control in the experimental situation, without necessarily formulating, enumerating, or understanding them."[16]

Later in this chapter I will discuss areas in which this shared culture either did not exist, or was not sufficient to enforce consensus. For the moment, note that Kasday, who performed a high-energy experiment and had to use a supposedly less reliable assumption, was able to draw on different elements of his culture (such as previous experiments) in order to justify his choice. This did not provoke a serious disagreement because Kasday's results were quite consistent with the low-energy results.

The Plausibility of Experiments.

In 1972, the first low-energy photon experiment (performed by Freedman and Clauser) produced results in good agreement with the predictions of QM[17]. Early in 1973, Holt's (low-energy) experiment gave results in complete disagreement with QM[18]. No other low-energy results were available until 1976, when Clauser, and Fry and Thomson, both found results in agreement with QM[19]. High-energy experiments were completed in 1971,1974, 1976 and 1977[20]. Apart from the 1974 experiment, all of these agreed with QM, and even the 1974 experiment, performed in Sicily by Faraci and his colleagues, was ambiguous rather than clearly in favour of LHV. Finally, another experiment completed in 1976 used protons rather than photons, and also gave good agreement with QM.[21]

Thus, by 1977, when I performed my interviews, it is perhaps not surprising that physicists were able to conclude that, despite Holt, QM had been confirmed. While it may be rather simplistic to imagine physicists simply adding up the score for and against QM, the sheer weight of numbers in favour of QM was obviously very influential, as the following quotation from a review paper makes clear:

"In order to maintain that a local realistic theory [i.e. LHVT] governs nature, one must invoke experimental errors not only to explain a violation of the inequalities [i.e. a violation of the LHV predictions] in seven out of nine experiments, but also to explain a very close quantitative agreement with the quantum-mechanical predictions in these seven."[22]

The situation becomes slightly less one-sided when we recall the general agreement that high-energy experiments require stronger assumptions, so that their results are less decisive. (The proton experiment, it was agreed, needed even stronger assumptions.)

However, even if we concentrate on the low-energy experiments, the score still favours QM by three to one. This was perceived to be conclusive, as is illustrated by the PhD thesis of Lamehi-Rachti, who performed the proton experiment. The main body of the thesis was written before the 1976 low-energy results were known, and Lamehi-Rachti discusses the low-energy situation in the following terms:

"We see that in this type of experiment....the experimental situation is far from being clear, with two experiments giving contradictory results, and it is difficult to draw a conclusion in the absence of other experimental results of the same type which would confirm or deny the results of one of these two experiments."[23]

In an appendix of the thesis, the results of the 1976 experiments are noted:

"Holt's experiment has been repeated by Clauser....It therefore seems that Holt's experiment has been marred by an experimental error. Another experiment....by Fry....has allowed a distinct improvement in the experiment....We can therefore conclude that the results of experiments of photon correlation in atomic cascades [i.e. low-energy photons] are in favour of QM."[24].

Statements such as these would suggest that the choice between Holt's experiment and that of Freedman and Clauser was not resolved until further relevant empirical evidence became available in 1976, and that the validity of Holt's result was an open question until that time. In many ways, this suggestion is misleading. The status of the two experiments was far from symmetrical prior to 1976. For example, many discussions of the experiments during this time relegate Holt's result to parentheses:

"Freedman's results showed that the quantum mechanical prediction held to sufficient accuracy to exclude hidden variable theories satisfying the localizability condition (although Holt's experiment seems in disagreement with this)."[25]

and

"the existing experimental evidence is generally against such an assumption (although there are two reports [i.e. Holt and Faraci] in its favour)."[26]

Other papers simply make no mention of Holt's experiment:

"Tests....of quantum theory have exhibited no contradictory evidence ....in the domain of atomic physics."[27]

We must be careful in interpreting these data, because of an even greater asymmetry between the two experiments. As pointed out in the previous chapter, Holt did not publish his results in a standard physics journal, unlike Freedman and Clauser. It is therefore possible that some authors had simply never heard of Holt's result. However, an unpublished account of Holt's experiment was widely circulated, and the experiment was discussed in published reviews.[28] It is certain that at least some authors who knew of Holt's result did not give it equal prominence with that of Freedman and Clauser. In addition, many interviewees claimed that they knew of Holt's result but that they 'never believed it'. A wide variety of reasons for this opinion were given, and many of these reasons make no reference to the later empirical data. These reasons can be grouped under several headings.

Inductive Arguments.

Many interviewees stressed the power and accuracy of QM, and claimed that they found it almost inconceivable that QM could have been refuted by such a relatively simple experiment:

"There's something very powerful about induction. QM has always been right and it's very hard to expect it not to be right."

"I could hardly believe that QM, which had explained so much so well, would be overthrown."

These views are of course reasonable, but they are far from conclusive. In fact, they are in conflict with the type of rhetoric used to justify the LHV experiments in the first place. In the early days, experimenters were eager to show that no-one could be sure that QM would be confirmed. They claimed that the past success of QM was irrelevant because LHVs would only show up in the special circumstances of these experiments:

"The history of science shows how little obligation Nature has to conform to our a priori conceptions."[29]

Ad Hominem Arguments.

Interviewees often referred to the reputation of the various experimenters, and to their competence and past record. Almost everyone had both good and bad things said about him, and opinions (not surprisingly) differed widely on these issues. One aspect which was frequently mentioned (and which was discussed in Chapter Five) was the involvement of Holt's supervisor, Pipkin, in an earlier experiment which claimed to have found a discrepancy in quantum electrodynamics.[30]

Here again, two interpretations could be placed on this episode. Some respondents cited this as a reason for suspecting that Pipkin 'had made another mistake'; others argued that, because of his earlier mistake, Pipkin would be doubly cautious and would have checked Holt's experiment very carefully to avoid a repetition of such an incident. Seen in this light, Pipkin's background supports Holt's results rather than weakens them.[31]

Expertise Arguments.

Interviewees on the periphery of the LHV activity were willing to defer to the judgement of the physicists who were most closely involved with the work.

"These people no doubt specialize in hidden variables and they probably had pretty good reasons for believing in QM."

Others carried this further, claiming that the only people who can really know everything about an experiment are the people who actually built and worked with that particular set of apparatus:

"The only people who could know for sure what went wrong are Holt and Pipkin because you've got to get down there and use that apparatus again to find out."

In areas of controversy where rival experimenters produce differing results, this deference to expertise can perpetuate disagreement, because each party can insist that he alone is competent to judge his experiment. This is not very relevant for the present case; as we have seen, Holt and Pipkin did not strongly defend their result, nor have they argued in favour of LHV. They did not even publish the result in the normal way. This seems to have left a strong impression on a number of people:

"The people who did the experiment didn't believe it. I'm certainly not going to believe it if they didn't."

Although they do not claim that their result is valid, Holt and Pipkin insist that neither they nor anyone else has ever successfully identified an error in their experiment. A number of suggested sources of error were proposed, but in each case, often after further tests of their apparatus, Holt and Pipkin claimed that the suggestion was false. As Pipkin put it, 'we still don't know what went wrong'.[32]

Holt himself could not fully explain why he rejected his result, except by references to intuitive feelings:

"An experimentalist gets a feeling for what systematic errors could do....and you develop a certain, almost intuition, I think, about whether you believe an experiment."[33]

Technical Arguments.

According to some respondents, the data of Freedman and Clauser was 'more forceful' than that of Holt, because the former favoured QM by six standard deviations, while the latter favoured LHVT by only four standard deviations. Yet both results are statistically significant according to the normal conventions of atomic physics. Thus, at the very least, Holt's experiment gave a definite result which cannot be attributed to random fluctuations.

A second criticism refers to the data collection procedures used in the two experiments. Holt took data for two orientations of the polarizers (at relative angles of $22\frac{1}{2}^{\circ}$ and $67\frac{1}{2}^{\circ}$). Freedman and Clauser took data at eleven orientations between 0 and 90°. However, all the experimenters had agreed on a theoretical analysis before the experiments which predicted that the maximum divergence between LHV and QM would occur at $22\frac{1}{2}^{\circ}$ and $67\frac{1}{2}^{\circ}$. Although data at other positions are useful, Holt's procedure cannot be considered incorrect.

These technical arguments have been summarized by Freedman:

"It wasn't as convincing as [our] experiment, just because they only measured it at two points....and with less signal-to-noise, so it seemed much more susceptible to systematic error, though they did a very good job of tracking [the errors] down, and they came to the right conclusion, [namely] that they couldn't find any."[34]

The 'Errors' Argument.

We have seen that many of the reasons given for rejecting Holt's result could be called 'nonscientific', or at least 'nonempirical'. Even the technical arguments so far presented are less than conclusive. However, when presented with a list of reasons, such as those above, some interviewees claimed that none of these was the 'real reason' for rejecting Holt. A similar point was made in a review article written by Shimony and Clauser:

"The probability is extremely high, in our opinion, that the results contradicting the predictions of QM were due to systematic error. This opinion is not based on a conservative acknowledgement of the great success of QM in the atomic domain. Rather, it is based upon the consideration that QM predicts strong correlations, whereas Bell's theorem sets a limit upon such correlations. Virtually any conceivable systematic error will wash out a strong correlation so as to produce results in accordance with Bell's theorem, rather than speciously strengthen a weak correlation."[35]

In other words, since errors usually reduce the degree of order in a system, they may reduce the 'correct' QM correlation so that the experiment appeared to yield the LHV value. In fact, Clauser, who later performed an experiment to check Holt's results, initially obtained results in agreement with LHVT. These results were eventually attributed to stress effects in an optical component, and when this fault was corrected, the LHV-type results disappeared.[36] Holt and Pipkin found that one of their optical components was also being stressed. This discovery was made after Holt's thesis and the Holt-Pipkin preprint were written, but in interviews, both Holt and Pipkin claimed that the stress effects were not large enough to invalidate the results.

Now, although errors could (and on at least one occasion apparently did) reduce a QM correlation to give a pseudo-LHV result, it is difficult to see how, if LHVT were correct, an error could artificially boost the correlation by just the amount required to yield the QM prediction. Thus the 'errors' argument allows us to dismiss results which favour LHV, but provides support for results which favour QM.

This is a powerful argument, and it was expressed in even more emphatic form in interviews:

"Nearly all errors, or maybe even all errors, although it's difficult to say that....but essentially an infinite number, all the ones you can think of, would tend to reduce the correlation."

"If anything goes wrong, you get the hidden variable result. Anything that goes wrong is going to give a weaker correlation. You can't get a stronger correlation than QM predicts."

But is it really the case that there are no errors whatsoever which could increase the correlations? When pressed on this point, interviewees tended to qualify their earlier statements:

"Oh, you can mock up a very perverse or psychotic type of mechanism."

"There are a lot of errors that could do it, but they're all sort of wierd."

"There could be such errors, but they have to be screwy, nutty things."

"[The errors argument] explains my prejudices, if you like. Obviously, someone could come up with a systematic error which goes the other way."

"That's a good point, but I can't think of an example....I haven't ever dreamed one up....I haven't really thought about it, though."

When pressed, interviewees did manage to dream up possible correlation-increasing errors, and they did indeed sound somewhat 'screwy', given what they (and I) knew about physics. Nevertheless, the above quotations indicate that the 'errors' argument depends for its strength on the fact that the LHV physicists shared tacit knowledge of what does and what does not constitute a 'screwy' or implausible hypothesis. Because of this agreement, it was not necessary to justify statements about the effects of errors on correlations; everybody knew that (effectively) all errors reduce correlations.

Nevertheless, this piece of tacit knowledge is by no means a watertight fact. For example, if the argument is taken seriously in its most emphatic forms, then a single experimental result in favour of QM would be proof of the validity of QM, since there is thought to be no 'non-psychotic' error which could falsely produce such a result. Now that further experiments have been performed, it is fairly safe to make such a claim, as one physicist did in 1977:

"There are many ways of washing out the correlation. But you cannot see any way of increasing the correlation. So one experiment that gives a good correlation for QM is enough....you can have ten experiments against QM because there are hundreds of reasons for washing out a good correlation....but nothing else except QM can explain....Freedman and Clauser's result."

If the errors argument was really as strong as is now claimed, we would expect to find it being used quite openly to reject Holt's result as soon as this result appeared. Yet Holt's thesis makes no mention of this argument, and in their unpublished manuscript, Holt and Pipkin simply state that

"in view of the results of the Freedman and Clauser and experiment, it is premature to claim that all systematic errors have been eliminated, and that the quantum-mechanical predictions are incorrect."[37]

Clauser and Horne, in their 1974 theoretical analysis of LHV, also fail to use the error argument. They simply note the existence of conflict between the two results.

When the error argument was used for the first time, in 1975, it was used more tentatively than in later comments. In 1975, it was claimed that 'there are many more systematic errors that reduce the correlation than increase it', while in 1976 'nearly all possible systematic errors' would reduce the correlation, and in 1977 'an infinite number, all the ones you can think of' would do so. Since Freedman co-authored the first two statements[38], and made the third in an interview, the statements as a whole are comparable, and seem to suggest that the error argument has been 'firmed up' as time passed.

Thus, although the error argument has been described as the 'real reason' for rejecting Holt's result, it does not appear to be different <u>in kind</u> from any of the other arguments used to justify this rejection. None of these arguments, taken in isolation, proves conclusively that Holt was wrong. When taken together, and placed in their social context, they make a very persuasive case, not only for ranking Holt as less plausible than Freedman and Clauser, but also for rejecting Holt's result completely. Since none of the arguments we have seen so far relies for its strength on further experiments, there is a great deal of justification for the claim made in Chapter Five, namely, that further experiments were not solely, or even primarily, motivated by a desire to resolve a genuine impasse between two equally plausible results. As I suggested in Chapter Five, it seems very likely that the above arguments would have been quite sufficient to justify Holt's rejection if, for some reason, no further experiments had been done.

Even in the absence of other results, the case against Holt is a strong one; but it is a case based on likelihood and plausibility, and not certainty. Despite the emphatic statements made by LHV physicists, some were willing to concede this point:

"Experiments, really, to a large extent, are plausibility arguments. The result of the experiment presumably means something. Something caused this. Then the plausibility comes in, and you try to convince people or yourself that what caused it is what you thought you measured, and that, unfortunately, involves arguments that you and I can understand, it's not purely objective. What we try to do is to make it as objective as we can."

The Existence of Other Results.

My argument so far has ignored a very important feature of the context in which interviewees responded to my questions. At the time they were interviewed, they knew that there were several experiments agreeing with QM, while only Holt's experiment agrees with LHV. Thus, although the case against Holt was a matter of plausibility, the body of experimental evidence made it a matter of virtual certainty. One interviewee expressed it like this:

"It depends on the number of experiments. If everybody gets correlations that are too high then you have to say that a screwy accident is happening to everybody. That I can't believe. One experiment, yes....but you're not going to get the same thing happen when another group does the whole experiment over again with a different apparatus. If several groups do it and they get the same thing, then the probability of something like that becomes vanishingly small."

The 'additivity' of results is well illustrated by Lamehi-Rachti's thesis (quoted earlier) in which he found it 'difficult to draw a conclusion in the absence of other experiments' when faced with only two conflicting results, while (in the appendix) he noted the later results, and thereupon dismissed Holt's experiment.

I have already argued that this apparent conversion is not an accurate picture of the response to Holt before, and after, other results appeared. Holt's result had effectively been dismissed long before other results were known, and most of the arguments used to justify this rejection made no reference to other experiments. Nevertheless, it is certainly true that the errors argument becomes more persuasive when a large number of results in favour of QM are available. The fact that this argument is now used retrospectively to explain why Holt was 'obviously wrong' is interesting if fairly predictable, though it annoys Papaliolios:

"It's a phony reason. I'm an experimentalist, and I believe that if you have to reach a conclusion on the basis of error analysis then your experiment is lousy and it should be re-done....Holt's experiment gave a definite answer, and the obvious thing is to do it again, with perhaps a slightly different set-up."

Anyone who wished to defend Holt's result would obviously have to deal with the other, conflicting, results. One tactic would be to claim that all the other experiments were wrong; clearly, this would be an extremely unconvincing argument. However, this is not the only option. Instead, it could be claimed that Holt's result is not comparable with the other experiments because the physical conditions under which they were performed were not the same.

To some extent, this point was recognized: Clauser went to great lengths to make his second experiment as similar to Holt's as possible. For example, Clauser's first experiment (with Freedman) had used photons from an atomic cascade in an isotope of calcium, whereas Holt had used mercury. In his second experiment, Clauser switched to mercury. Had he not done so, it could still be argued that LHVs only manifest themselves in mercury and not in calcium. Now we know this is not so:

"Holt's experiment has been repeated....by Clauser, with the same atomic cascade as Holt but with somewhat different equipment.... so whatever the fault is, it's not in the mercury atom."

But this point-by-point comparison could, in principle, be extended to a near-infinite number of features of Holt's apparatus, many of which were not duplicated by Clauser. Clauser himself describes his experiment as

"attempting to repeat, at least in part, the conditions of Holt's experiment."[39]

Clauser lists several 'possibly significant' differences between the two experiments. The fact that Clauser's experiment gives a different result from Holt's could be taken to mean that one of these differences was enough to prevent Clauser's experiment from being a functioning LHV detector. But of course this was not what happened. Instead, Clauser's experiment was seen as correct, and the differences between the two were seen as pointers to the likely location of Holt's error. For example, the error probably did not lie in Holt's excitation method, since Clauser used the same method and still got the 'right' answer; the error was more likely to lie in an area which Clauser did not duplicate.

Apart from their use in error-tracing, the differences listed by Clauser are seen as trivial. Thus, although Clauser himself was careful to qualify his statements about replication, his experiment is, for all practical purposes, seen as a straightforward complete replication of Holt's experiment.

"Holt's experiment has been repeated by Clauser."[40]

Holt himself agrees:

"Now that Clauser has done the experiment with the exact same cascade in mercury, with the same sort of lamp, I have to believe that it's got to have been the apparatus which caused the result. It couldn't have been the physics because everything is the same there."[41]

It would of course be possible to construct a long list of differences between the experiments. These would range from the technical[42] (different lens coatings, different photocathode characteristics, different levels of isotopic purity) through the strange (different locations, different times of the year) to the apparently ludicrous (different colours of paint on the lab walls, different number of letters in the experimenters' surnames). Given what physicists 'know' about atoms, and the behaviour of their apparatus, none of these differences is at all relevant. This knowledge is of course theory-dependent and rests on 'bold conjecture' rather than on unassailable truth[43]. The reader, however, will surely feel that some of these conjectures are not particularly bold ones. This feeling, I would suggest, simply reflects the extent to which the reader shares the same cultural background as the LHV physicists.

Although I have tried to cast doubt on the conclusive nature of the reasons advanced for rejecting Holt, and have tried to render them problematic, I certainly do not wish to imply that the LHV physicists were foolish, irrational or necessarily wrong in coming to the conclusions they did. The important point, though, is that the LHV physicists were by no means logically compelled to reach these conclusions.

Not all the reasons produced by physicists could be classed as 'good scientific reasons'. Indeed, they cover a wide spectrum of 'hardness' or 'scientificity', ranging from gossip to detailed technical arguments and experimental evidence. My aim in this section has been to show that as we move through these arguments, we find no epistemological discontinuity. There is no point at which the arguments cease to be appeals to reasonableness and plausibility, and start to derive from something firmer. All that happens is that as the arguments begin to accumulate, and begin to be phrased in more conventional scientific form, the case in favour of LHV (or Holt) begins to look less and less plausible. In other words, the counter-arguments needed to defend LHV, or the validity of Holt's result, begin to seem increasingly 'obsessive' or 'pathological'.

Ultimately, physicists vote with their feet. If each individual feels that the issue of LHVT has been settled to his satisfaction and leaves the field, then in a very real sense the issue of LHVT has been settled. Small loopholes may still exist,such as the need for assumptions, but most people do not see this as a serious reason for continuing to work in this field.

"It's perfectly possible [to improve on the experiments] but nobody is willing to spend the time, money or effort to actually perform it....I've got other things to do now."

"We all came away....feeling that the theorists had to think up a new idea that would give experimentalists something to work on, other than just pushing the same idea into better and better experiments."

"I don't really see any more experiments in hidden variables that I find really interesting, whereas I've got lots of other experiments that I've been putting off."

In their published statements, these physicists were usually careful to point out the loopholes and qualifications which applied to their conclusions. Nevertheless, the evidence against LHV, and the implausibility of this hypothesis, did lead to some small liberties being taken. For example, shortly before he left Berkeley and moved to a quite different job, Clauser performed another LHV experiment, this time using circularly-polarized photons.[44] He admitted (in an interview) that this experiment was done in a hurry, on a low budget, and that the results were not very precise. (This seems to have been the reason why this paper was published in Nuovo Cimento rather than the more prestigious Physical Review Letters, where Clauser's other experiments appeared.)

In this experiment, as in the others, correlation rates were combined into a single quantity, which Clauser called δ . (See Chapter Two for details.) QM predicted $\delta = 0{\cdot}002$ for Clauser's arrangement, while Bell's inequality demanded that $\delta \leqslant 0$ for all LHV theories. The experimental result was that $\delta = -0{\cdot}015 \pm 0{\cdot}025$. (The size of the error reflects the poor quality of this result.) In his paper, Clauser admits that:

"No actual violation of the inequality can be sought here."[45]

However, Clauser seems to ignore the fact that his measured value of δ is negative (and is certainly not equal to 0·002), when he concludes

"None the less, the predictions of [QM] for this case appear at least to be approximately valid."[46]

Thus, although this result could conceivably be used to defend LHV, the case against LHV had by this time been 'firmed up' to such an extent that such a usage was quite implausible; indeed, Clauser seems to be using this result in the opposite way, as a defence of QM.[47]

In interviews, as we have seen, LHV physicists were much more emphatic, and less cautious, than in their published statements. This is hardly surprising.[48] What is perhaps more significant is that, as we move out from the small 'insider' group who actually worked in LHV, and examine the views of other observers, we find that conclusions become more and more emphatic, and qualifications tend to disappear. For example, authors such as Zukav and Stapp, who have written about the philosophical implications of non-locality, ignore the evidence in favour of LHV and do not mention the assumptions required in existing experiments.[49] During my fieldwork, I encountered a number of physicists who had heard of the LHV activity but had not heard of Holt. Several people who had heard of Holt claimed (incorrectly) that the source of Holt's error had now been agreed. Finally, I found that apart from those who had been actively involved in LHV, few people knew much about the assumptions involved in the experiments.[50]

The Reception of Faraci's Experiment

In 1974, Faraci and his colleagues at the University of Catania, in Sicily, published the results of a high-energy photon correlation experiment.[51] These results disagreed quite sharply with the predictions of QM, and provided limited evidence in favour of LHV, although the evidence was not as strong as Holt's experiment.

Unlike Holt, Faraci and his colleagues did not concede that they had made a mistake. However, they have received no support from other experimenters. The most detailed published criticism of their experiment was made, as one might expect, by the group which had already performed a high-energy experiment, Kasday, Ullman and Wu. In a postscript to their 1975 report of their own experiment,[52] Kasday and his colleagues discuss several shortcomings in the Italian experiment, most of which involved subsidiary effects which might have taken place inside the apparatus and might have caused the anomalous results. Unlike the US group, the Italians had made no empirical checks to see if their results had been distorted in this way; instead, they had performed a theoretical calculation to estimate, and correct for, such effects.[53]

Kasday's group also pointed out a much more glaring error in Faraci's paper. Faraci had claimed that the two sets of results were in agreement. As Kasday put it

"This is not correct. Our results are in formal disagreement with theirs."[54]

Faraci's group continued to defend their result, largely by ignoring rather than refuting such criticisms. In 1976, they published a second paper in Nuovo Cimento.[55] This paper was submitted after Kasday's paper was published, also in Nuovo Cimento. It therefore seems likely that Faraci's group were aware of Kasday's criticisms, yet they make no reply to these criticisms. In addition, there is no doubt that Faraci's group were aware of the results of Kasday's experiment, and of Freedman and Clauser's experiment, since these experiments were cited in Faraci's earlier (1974) paper. Yet in their 1976 paper, Faraci and his colleagues make no mention of either of these experiments. Of their own experiment, they say:

"[the result] shows an anisotropy ratio lower than that predicted by QM but in agreement with Bell's limit for the existence of local HVs."[56]

This sounds rather as if agreement with 'Bell's limit' is something rather definite and significant, rather than being very ambiguous, neither in favour of nor against LHV. (Recall that QM predicts λ to be a specific positive number, LHV predicts $\lambda \leqslant 0$, and Faraci found $\lambda \approx 0$.) Bell himself claims that the term 'Bell's limit' is 'entirely unauthorized'.[57]

Faraci's group cite only one other experiment - Holt's. They write:

"Recently, some experiments (Holt and Faraci) seem to put in doubt the general validity of [quantum] theory."[58]

Since they neglect the other experiments in favour of QM, their paper appears to give undue support to LHV.

Also in 1976, a conference on the LHV experiments was held in Sicily.[59] Faraci's group was represented by Notarrigo; he defended their use of a theoretical analysis rather than an empirical check of the effects discussed by Kasday. He did not suggest any possible error in Kasday's experiment, but claimed that "the question....is still open."[60]

I attended this conference as an observer, and I gained a very clear (though of course subjective) impression that Faraci's result, and Notarrigo's defence of that result, were treated rather sceptically, despite the apparent 'one against one' situation among the high-energy photon experiments at that time. A significant factor, at least in the formal sessions and the debate between Notarrigo and Ullman (the representative of Kasday's group) was the fact that Notarrigo had to defend his result in a language (English) in which he was not fluent. Nevertheless, I do not feel that this was a crucial point in determining the verdict reached by the audience of LHV experimenters. Although I have no documentary evidence on this point, I gained the clear impression that most of the audience had made up their minds before they heard Notarrigo's defence.

Faraci's experiment provides a very useful comparison with Holt and Pipkin's experiment, in a number of respects. Both disagreed with QM; in both cases, previous (and apparently similar) experiments had agreed with QM; both were eventually rejected by most physicicists; and in both cases, further experiments were performed to check these 'discrepancies'. However, Faraci's group defended their result, whereas Holt and Pipkin did not. (As discussed earlier, Pipkin said at the conference 'we still don't know what went wrong with this experiment'.)

A further difference between the two experiments was that Holt's care in checking his apparatus and searching for errors seems to have led most people to conclude that Holt himself was a competent experimenter, and that he had simply been unfortunate. The informal verdict on Faraci's experiment was somewhat less charitable. This is clearly a sensitive issue, so the following comments are quoted anonymously:

"Kasday, Ullman and Wu just had a vastly more sophisticated experiment."

"There was no big fundamental fight between Faraci's and Kasday's experiments. Faraci's group simply weren't careful enough about taking some corrections into account. Their experiment would be more interesting if there weren't such an obvious flaw."

One informant said of the Sicilian conference:

"I think it was pretty much the consensus, although nobody really wanted to come out and say it in public, that it was just a second-rate shoddy experiment."

When asked to explain in detail why he preferred Kasday's experiment, this informant replied:

The amount of care being taken, the number of checks against systematic errors, the overall understanding of all the physical processes that were going on....Faraci et al [sic] left a number of things unexplained in their write-ups which turned out to be quite crucial....the thinking was quite muddled, the attempts at shielding and eliminating secondary scatter were very sloppy. Kasday had made really great efforts to eliminate a large number of systematic asymmetries which Faraci hadn't done. Faraci's statistics were quite poor in comparison, their electronics were pretty sloppy. It was just not professionally done."

These statements strongly contrast with the informal comments made by the same group of people about Holt's experiment. Although no-one thought Holt's result was correct, neither did any of my informants claim that Holt was incompetent. At most, they said that if they had been in Holt's position, they would have acted differently; for example, some said they would have published the results, and others said they would have used different error-tracing techniques.

Thus it would seem that the LHV physicists' judgement of Faraci was somewhat harsher than their judgement of Holt. This is an important point, to which I shall return later. However, there are also many similarities in the treatment of <u>both</u> pairs of conflicting results, namely, Holt-Pipkin v Freedman-Clauser, and Faraci v Kasday. It should already be clear that in each case, physicists were able to produce reasons for preferring the result which agreed with QM, and that many of these reasons had little to do with subsequent experiments. In the case of Holt, I argued in

Chapter Five that the resolution of apparent discrepancies was not the only, and perhaps not even the most important motivation for performing further experiments. I have yet to establish a similar point for the response to Faraci's experiment. I will now attempt to do so.

If the statements quoted earlier are truly indicative of physicists' assessment of Faraci, there would seem to be little point in performing further experiments. Yet two further high-energy photon experiments <u>were</u> performed after Faraci's experiment; one group, in Birkbeck College, London, was led by Wilson, and the other, in Bologna, Italy, was led by Maroni.[61] Why were these experiments performed?

I was unable to interview any of these experimenters.[62] However, I did interview Bohm and Hiley, who were also in the physics department at Birkbeck College.[63] Hiley told me that he and Bohm had suggested the experiment to Wilson as early as 1970, long before Faraci's result was known. The experiment was originally intended to test a theoretical proposal, advanced by Bohm and others, which was quite distinct from LHV.[64] As things turned out, the experiment took much longer than expected, mainly because the apparatus had to be moved to another building for administrative reasons partway through the construction stage. Thus it was only by accident that Wilson's results appeared after Faraci's. It is not surprising that Wilson and his colleagues cite Faraci, because Wilson's results are certainly relevant for any discussion of Faraci's experiment, but it seems fairly clear that Wilson's experiment was <u>not</u> prompted by any concern over an 'anomalous result'; indeed, it was not a response to Faraci at all.

Despite this fact, Wilson's experiment has been <u>presented</u> by other physicists as if it were a direct response to Faraci. For example, Clauser and Shimony discuss Faraci's result, then write "Wilson et al repeated the experiment."[65]
Pipkin, in a comprehensive review of the LHV experiments, writes: "Stimulated by the discardent [sic] results obtained by Faraci et al, Wilson, Lowe and Butt used an arrangement...."[66]

These descriptions of Wilson's experiments are probably a result of ignorance about the true origins of the experiment rather than deliberate misrepresentations, since no-one from Birkbeck College attended the experimenters' conference in Sicily, and (as discussed in Chapter Four) there is little communication between Bohm's group and the LHV group. However, this episode usefully illustrates how a sequence of events can be (inaccurately) presented in such a way as to conform to a stereotyped notion of 'scientific method', in which anomalies unproblematically generate further investigation.

The second experiment, by Maroni's group, does seem to have been a direct response to Faraci's experiment, though here, too, this statement requires qualification. Firstly, Maroni and his colleagues were not previously involved with FQM; like Wilson's group, Maroni was not motivated by strong doubts about the validity of QM. Instead, the experiment was suggested by colleagues who were interested in theoretical FQM. As Maroni told me:

"I am not a specialist in the problems concerning the foundations of QM....by our work we had it in mind to give a small contribution to the discussion of an important cultural subject without any professional implication."[67]

Secondly, Maroni did not learn of Wilson's experiment until after his own had been completed.[68] It is impossible to say whether Maroni would have become involved if he had known that another independent test of Faraci's results was taking place.

Finally, it would seem that Maroni's group did not have a high opinion of Faraci's experiment; having discussed the LHV experiments, including Kasday's and Faraci's, they write

"It turns out that all these results seem to support QM rather than LHV theories"[69],

although, to be fair, they do go on to justify their own experiment in terms of the need to resolve the existing discrepancies.

Unlike Holt and Pipkin, Faraci and his colleagues did not reject their own result. For this reason alone, Maroni's experiment is understandable. However, despite the fact that both Maroni's and Wilson's groups found results in agreement with QM, Faraci's group has continued to defend the validity of its results. For example, in April 1978 Faraci and Pennisi published a paper in which they derived further inequalities which would allow new experimental tests of QM and LHV. In this paper, they made only a passing reference to the existing LHV experiments:

"At present, much theoretical and experimental work is in progress to check.....QM."[70]

The reader is then referred without comment to the published abstracts of the 1976 conference papers. Faraci's own experiment is also cited, but with no indication of its findings.

Another paper, written in June 1978 by the same authors although not published until 1980, discusses yet more inequalities which, it is proposed, could be used to study triplets, not pairs, of photons and hence provide further tests of QM. Again, the existing experiments are cited but the results of these experiments are not discussed.

In a letter, also written in June 1978, Faraci told me

"Notwithstanding results of many groups which agree with QM, I believe that this problem is not at all concluded. As far as our results are concerned we are checking very carefully any possible source of systematic errors which could have modified the experimental conclusions."[72]

Faraci went on to make criticisms of Wilson's and Maroni's experiments, but he did not criticise Kasday:

"Wilson's experiment gives serious reasons of doubt about his results because of the geometry used (mainly the size of detectors). On the other hand, Maroni's data are less accurate than Kasday's."[73]

A group of Sicilian theoretical physicists, based at Palermo, have also adopted a sympathetic approach to Faraci's results. This group's response is particularly interesting because they adopt the same tactics as I did in my 'devil's advocate' defence of Holt's result in the face of mounting evidence in favour of QM. Their argument is worth quoting at length:

"The quoted results do not agree with each other, but most of them agree with QM....while [Holt and Faraci] do not. Being faced with this situation, our point of view is that all results obtained so far should be considered right, until a mistake is found that proves some result to be wrong, so that the problem is not to find which or who is right or wrong, but why different results are found, i.e. to find which relevant conditions are different in the two classes of experiments....Thus it seems necessary to put under control some parameter not yet considered."[74]

What conclusions can we draw from the reception of Faraci's experiment? Some aspects of this episode simply serve to support the conclusions derived earlier from a consideration of Holt's experiment. For example, most physicists seemed to find little difficulty in choosing between conflicting experiments (Kasday v

Faraci and Freedman and Clauser v Holt). It would be too facile to claim that they simply chose the results which agreed with QM. Equally, however, it would be inaccurate to claim that they kept a completely open mind until further experiments were performed. Instead, they evaluated the rival experiments by invoking a whole range of criteria, drawing on a variety of elements from their cultural context. This evaluation was far from being an example of straightforward deductive reasoning.

In the case of Holt, I pointed out that each of the reasons for rejection can, individually, be criticised. Rejection is a matter of plausibility, not of proof. However, given the context of Holt's result, the outcome was hardly surprising. Faraci's case provides further support for these arguments. The major difference between Faraci and Holt is, of course, that Faraci and his colleagues have continued to claim that their result is valid. Regardless of the persuasiveness of such claims, or their effect on the professional future of this group, the fact remains that such a defence is clearly possible.[75] Admittedly, the defence is a rather minimal one; Faraci's group has never criticised in detail the experiments which disagree with their findings, particularly Kasday's experiment. Nor have they tried to explain why their experiment alone displays the effects which they found. Yet it is clear that if they wish to pursue a search for 'relevant differences' between their experiment and all others, such a course of action is, in principle, open to them. The fact that, in their most recent papers, they seem to be adopting such an approach, is further evidence in favour of my arguments, concerning both their experiment and Holt's.

There is another important difference between Holt and Faraci, to which I alluded earlier. Holt's experiment, at least in one sense, was a success: the other LHV physicists did not conclude that Holt himself was an incompetent experimenter. Instead, they concluded that he reacted properly to a puzzling experimental anomaly. Faraci's group was judged more harshly: many LHV physicists claimed that Faraci's experiment was done in an unprofessional manner, with an unacceptable lack of careful checks on whether the apparatus was functioning properly. In other words, the experiment was taken as a reflection on the personal competence of Faraci and his colleagues, which was not the case with Holt. Holt's case indicates that the production of an anomalous result is not, by itself, sufficient to lead to the conclusion that the experimenter is incompetent. How, then, can we account for the different reactions to similarly anomalous results?

One possibility is to stress the fact that Holt 'recanted' by denying the validity of his results and declining to publish them in isolation. Faraci's group did publish. Yet it does not seem likely that this alone could account for Faraci's hostile reaction. After all, some LHV physicists felt that Holt should have published his results.

What seems more likely is that the quality of Faraci's work seemed inadequate. The published report of their results was brief, containing few details of their experiment; the paper contained factual errors, and introducing the misleading 'Bell limit'. It later became clear that their experiment was much simpler than Kasday's, omitting many important experimental checks. Their defence of their result was weak; they provided no account of the different results obtained by other groups.[76]

In other words, I am suggesting that the hostile reception given to Faraci's group was not because they obtained the 'wrong' result, nor even because they did not admit that their result was wrong. Instead, other physicists concluded that the work was simply badly done. The cultural context shared by the other LHV physicists included a set of expectations of how one should go about performing and presenting a physics experiment, and Faraci's group did not fulfil these expectations. It may be true that one aspect of these expectations is that a 'good' experiment should produce the 'correct' result - in this case, agreement with QM. But this is by no means the whole story.

The Social Construction of Plausibility.

In previous sections of this chapter, I have discussed the effects of the existing plausibility structure on the evaluation of new knowledge-claims. That is, I have been concerned with the constraints imposed by the cultural context in which these claims appear. However, it is clear that physicists are not merely passive respondents in this process. We have already seen how Holt successfully coped with his anomalous and unwelcome result. His actions did not make his result any more plausible, but they safeguarded his professional reputation. In this section, I shall deal with a case in which a physicist <u>did</u> succeed in increasing the plausibility of an idea by choosing a specific course of action. The discussion will centre around the different fates of two LHV 'loopholes', both of which claim that LHVT may have some validity despite the existing experimental evidence.

One of these loopholes, which I shall call the 'selective apparatus' proposal, has already been mentioned. It suggests that polarizers and detectors, which themselves may be governed by LHVs, may select a biased sample of the total photon population so that QM appears to be valid even when it is not. Although much of the early discussion of this proposal refers to the behaviour of detectors, it should be stressed that the behaviour of polarizers is also questioned, as this quotation from a letter written by a LHV physicist in 1970 reveals:

"It seems to us that the polarizer may profoundly affect the hidden variables carried by the photon....if polarizers were either ideal or identical then this could be ignored. But it is not clear to us that the polarizers are either ideal or identical....it is easy to conceive that the polarizer could randomly mix a well-ordered set of hidden variables to change a LHVT result into a quantum result."[77]

The possibility of selective polarizers has also been mentioned in several papers.[78] As discussed earlier in this chapter, the LHV physicist decided that this possibility was not at all plausible, and they made the assumption that this proposal was false.

The other loophole which I shall discuss has become known as the 'timing proposal'. It first appeared in Bell's original paper on LHV, published in 1964:

"Conceivably, [the QM predictions] might only apply to experiments in which the settings of the instruments are made sufficiently in advance to allow them to reach some mutual rapport by exchange of signals with velocity less than or equal to that of light. In that connection, experiments....in which the settings of the instruments are changed during the flight of the particles, are crucial."[79]

Expressed in rather loose terms, the timing proposal suggests the following. If an experimenter sets up his apparatus, and then goes for a coffee break, the photon source might, in some unknown way, 'learn' the settings of the polarizers. Knowing this, the source could then adjust the polarizations of the each photon pair which it emits, in such a way that the results would appear to show that QM was correct. Even if the experimenter takes a very short coffee break, the source would still have plenty of time, since this information might be transmitted at the speed of the light. To avoid this, the experimenter would have to wait until the photon source had 'read' the polarizer settings, and had sent out a pair of photons with suitable properties; then, while the photons are in flight (but before they arrive at the polarizers) he would alter the polarizer settings so that the photons would no longer be specially prepared for what they would meet. This alteration would have to be done extremely rapidly, since it must take place in less time than it takes light to travel a few centimetres. Since none of the existing experiments incorporated this 'timing' element, physicists had to assume that the timing proposal was false if their experiments were to be considered conclusive tests of LHV.

Let us compare the plausibility of the selective apparatus and timing proposals at an early stage in the LHV activity. At first sight, the timing proposal might seem to be more important, since it was mentioned in the very first LHV paper. The selective apparatus proposal did not appear in print until 1969, five years later.[80] However, it would be wrong to give much weight to this argument, since the 1969 paper in question was essentially the first to take up Bell's work. In terms of the historical development of LHV, the two papers represent the first and second stages of the work. It is also significant that the timing proposal is not mentioned in this 1969 paper.

Another paper, published in 1971, discusses both proposals, describing them as

"manoeuvres available to the advocate of LHV theories."[81]

The selective apparatus proposal is dealt with in 34 lines, and the timing proposal in 9 lines. Both are effectively dismissed, without being shown to be invalid. In each case, possible future tests of the proposal are briefly mentioned.

In 1974, an important review paper discussed both these proposals, and again both were given similar treatment.[82] That is, the existence of these criticisms was acknowledged, as was the possibility, in principle, of more definitive tests at some later date, though no details of such tests were given. The authors concluded that in the absence of such ideal tests, assumptions were necessary in the existing experiments, but in view of the rather extreme nature of these two criticisms, such assumptions were plausible.

It would therefore seem that neither proposal was taken very seriously at this stage, and certainly neither was believed to make existing experiments pointless. As we have seen, most experimenters began to feel that LHV had been given a fair trial, and had been found wanting. Although improved experiments were possible, most people did not themselves wish to invest the time and effort needed to perform them:

"Certainly, questions on the polarizers and on the nonlocality, those sorts of things are details that one can take a look at. My feeling is that those experiments aren't going to be particularly productive. We've looked for the obvious conflict in the obvious way, and we've resolved that."

Despite the apparent symmetry in the early attitudes to the two proposals, circumstances have now changed. An experiment is being planned which will test the timing proposal, whereas no-one is planning to test the selective apparatus proposal. What effect has this had on physicists' attitudes to the two proposals? What effect, if any, have the existing results had?

I shall begin with the selective apparatus proposal. Since it argues that pieces of apparatus, such as polarizers, may be governed by LHVs which can alter the behaviour of photons, it is possible that different types of polarizers might have different sorts of hidden variables and hence might have different effects on the photons. After all, our notions of sameness and difference are closely related to our theoretical ideas about the way apparatus behaves[83]. It is interesting to note that in the low-energy photon experiments, two different types of polarizers <u>were</u> used. Most groups used a pile of eleven to fifteen glass plates, all aligned at a special angle, while one group used calcite prisms - that is, crystalline blocks.[84] What is even more interesting is that the experiment which used calcites was Holt's experiment! This was listed by Clauser as one of the 'possibly significant' differences between Holt's experiment and Clauser's later 'partial replication'.

However, when interviewees were asked why no-one had tested the possibility that calcite polarizers are somehow special (a possibility which is perfectly consistent with the 'selective apparatus' proposal) I was told that this idea was completely implausible:

"It is intriguing why the calcite experiment doesn't agree with QM, but I don't think it directs you to wondering if there's some significant difference between calcite crystals and piles of plates."

It is important for the argument which follows to note that the most common reason for rejecting this possibility was that we know of no mechanism which would permit this special property of calcite polarizers to appear:

"Essentially, we know how polarizers work."

"It's hard to see how the type of polarizer can affect it."

"Every physicist believes that polarizers work in the way they're supposed to work."

Thus it is clear that the plausibility of the selective apparatus proposal has not increased in recent years. Holt's experiment could be interpreted as providing some empirical support for the proposal, but the LHV physicists have chosen not to interpret it in this way.[85]

Let us now turn to the timing proposal. Aspect, a French physicist, proposed a polarization correlation experiment in 1976 which would incorporate a rapid switching device, allowing a test of the timing proposal.[86] When we consider the earlier comments about the implausibility of timing, it should come as no surprise to find that Aspect's proposed experiment did not revolutionize the field. Most people felt that LHV had effectively been ruled out already, and did not think it likely that Aspect would find a violation of QM:

"Aspect's chance of success is pretty small. The LHV issue is pretty well settled now."

"If Aspect did get a hidden variable result, no-one will believe him, until it's done again, independently, by several people."

Published comments, though more restrained, generally managed to combine an acknowledgement of the existence of assumptions with a fairly emphatic claim that LHV was already ruled out. For example, Clauser and Shimony discuss Aspect's experiment, and conclude:

"Despite our caution concerning the assumptions, we regard the experimental refutation which relies upon them to be compelling.... Although further experimental investigations are desirable, we are tentatively convinced that no theory of this kind [i.e. LHVT] can correctly describe the physical world."[87]

Aspect's proposed experiment did not lead other physicists to persist with LHV work. Interviewees spoke of the need to decide how best to invest their time; to them, other areas were more likely to be fruitful than testing timing. Nevertheless, their attitude to Aspect himself was one of tolerance. After all, to many outsiders the <u>entire</u> LHV activity was pointless since QM was 'bound to be correct'. Many LHV experimenters had encountered such attitudes, and they themselves, perhaps because of this, were more charitable towards Aspect.

"The only inevitable decision for a scientist is what to do next ....where to invest your time. And if you invest your time in one place it doesn't mean that you condemn anything else as not worthy of investigation. It just means that's where you think your talents will be fruitful."

"I'd be very surprised if the timing experiment gave a different result, but I suppose it's worth doing, just to close one small loophole."

"Aspect's is the only remaining significant step that seems feasible."

Aspect himself does not claim that his experiment is <u>likely</u> to produce startling results, but by filling a logical loophole it may serve a useful purpose. In his published proposal, he says:

"Such a feature has been considered a crucial one by quite a few workers in the field, and therefore such experiments are worth doing."[88]

While this proposed experiment has not led physicists to expect timing to be correct, it seems clear that it has made physicists willing to tolerate such an experiment. In a sense, the fact that a competent physicist is willing to test the timing proposal has led to a general acknowledgment that the proposal is at least plausible enough to be worth testing. Aspect certainly feels that attitudes to timing have been changed by his efforts:

"In Bell's first paper, he says that [a timing experiment] would be a crucial [test] ....When I began to think about these things, people <u>had</u> written about timing, but they'd written very little....I very quickly realized that the next step in these experiments was timing.

Then I reread earlier papers more carefully, and I found sometimes one sentence, or perhaps nothing, or a footnote, where people said, 'Ah, perhaps there is a problem here; perhaps one could think about timing.' Today, everyone says 'now the next step is the experiment with timing.'"[89]

It must be conceded that, in their published comments, physicists have always encouraged further experiments. Without wishing to classify such comments as mere lip-service, I would still maintain that Aspect's efforts have altered attitudes. For example, published comments have continued to mention the existence of the 'selective apparatus' proposal, yet interviewees were strongly critical of the notion of experimentally comparing two different types of polarizers on the ground that there was no known mechanism which could lead to different behaviour in the two types.

Equally, however, no-one has ever explained how apparatus could achieve the 'mutual rapport' hypothesized in the timing proposal, and no mechanism has ever been produced which could explain how signals could be sent from one piece of apparatus to another. When I pointed this out in interviews, I was chastized for being intolerant:

"That's not the way you play this game. You're allowed to invent any mechanism which would make the thing go, so you're really not playing by the ground rules if you make that objection."

"You're not required to give a mechanism until you can see an effect [i.e. a violation of QM]. Then people would get busy trying to find out some way to explain it. But if the experiment is not outrageously difficult, it seems worth doing it whether you have a mechanism or not."

Aspect has not yet completed his timing experiment, though he has very recently completed a 'conventional' LHV experiment, with results 'in excellent agreement' with the QM predictions.[90] If his timing experiment were to disagree with QM, I would suggest that his result will not be believed. Whether or not Aspect's competence would be called into question would depend on the details of his experimental procedure and the manner in which he presents his results. As we have seen with Holt and Faraci, such factors (which are largely under the experimenter's own control) can markedly affect the outcome. Regardless of his presentation, timing is unlikely to be accepted. In other words, Aspect may have made timing plausible enough to be testable, but not plausible enough to be considered true on the basis of one new experimental result.

On the other hand, if Aspect's timing experiment were to agree with QM, its reception would present no difficulties. Indeed, I would suggest that if this outcome occurs, then in future retrospective reviews, Aspect's experiment will be placed in its 'proper' order, in what will seem a logical sequence starting with Bell's original paper in 1964, and tracing the 'development' of the timing proposal through the various footnotes and single sentences which Aspect mentioned. In other words, in line with the normal rhetorical style of scientific papers, it will be made to seem that the timing proposal was always plausible; that until it was tested no-one could be sure whether LHV was correct or not; and that it was conclusively falsified by Aspect's experiment. It is in this way that the plausibility structure of physics is continually reinforced and redefined.

Consensus and Cultural Context.

In this chapter, I have examined the role of culture in defining plausibility, and so influencing the reception of new knowledge claims. Yet it is clear from previous chapters that not all scientists who work on FQM share the same culture. There are, for example, methodological differences between specialties, which lead to different evaluations of interpretations of QM.

In my treatment of LHV, I have talked in general terms about the attitudes of 'the LHV group' towards Holt, Faraci, and Aspect. This rather monolithic approach is, I feel, justifiable, since I can find no major cases of disagreement over the reception of the work of these three people. However, it is important to stress that this consensus was not inevitable, but is a contingent empirical finding. In this section, I shall point out the limits of this consensus, and shall consider the implications of a breakdown in consensus.

When we examined the LHV physicists' analysis of the assumptions underlying the experiments, we found that terms such as 'reasonable' (and antonyms such as 'bizarre' and 'pathological') featured prominently in both published statements and interviews. We also saw that physicists found it difficult to articulate more fully what they meant by these terms. Such terms seem to operate as references to a shared culture, to 'what everyone knows'. Since many specific cultural elements are utilized in evaluating a specific proposal as 'reasonable' or 'unreasonable', it is difficult to explain clearly what these terms mean. Fortunately, in this particular case, the LHV group were able to agree on what was reasonable.

Obviously, not all physicists would agree in all cases about the reasonableness of a particular proposal, and this applied even within the LHV group. I shall quote an example. Many attempts were made to produce the weakest possible assumption in order to strengthen the force of the empirical data against possible LHV counter - examples. In 1976, Bell produced an analysis which required, as its basic assumption, that experimenters have free will.[91] This analysis was criticised by Shimony, Horne and Clauser on the grounds that:

"it would not be legitimate....[to rely] on a metaphysics which has not been proved and which may well be false."[92]

These authors then propose another assumption, and claim:

"our contention is that [our assumption] is more reasonable, though we do not pretend to offer a definitive proof nor do we think that one can be given."[93]

Although they concede that their claim cannot be proved, they provide a number of reasons in support of it. They argue that their assumption is more reasonable because it is specific rather than general; it makes weaker demands; and it postulates no unknown mechanisms. All that they assume is the absence of 'conspiracies', of the type best exemplified by 'Maxwell's Demon'.[94] They justify this assumption on the grounds that to deny its validity is to make science impossible. All scientific activity is based on the assumption that the natural world is passive and is not actively trying to deceive us.

Here we see the major difficulty involved in using a term like 'reasonableness'. Shimony, Horne and Clauser's supporting arguments are no more independent than their use of reasonableness. For just as individuals may disagree over what is reasonable, so they may also disagree over what constitutes a 'weak' assumption, and over the value of particular criteria such as specificity and generality. In other words, it is not possible to enforce consensus by appeals to reasonableness. Shimony and his colleagues seem to realise this, since their assumption is justified not on ontological grounds (appealing to what the world must be like) but on explicitly methodological grounds (appealing to what scientific investigation must be like).

Bell was not convinced by their arguments. In a reply, he wrote "I do not agree that [my] assumption....is an unreasonable one."[95] He rejected the methodological argument:

"A theory may appear in which....conspiracies may occur....I will not refuse to listen, either on methodological or other grounds,"[96]

although he adds

"but I will not myself try to make such a theory."[97]

This disagreement was not a violent one, nor did it have any important bearing on the interpretation of the LHV experiments. As Horne put it:

"We'll never revive [this argument] because we don't really disagree about anything....it's not worth pursuing....we might have some differing opinions on whether this assumption or that assumption is more plausible, but I think we agree essentially."[98]

This was not the only case in which consensus among LHV physicists broke down. Consider Aspect's 'timing' experiment. Although most physicists felt that the experiment was worth doing, roughly two-thirds of those involved in my questionnaire survey felt that it was possible 'to draw definite conclusions about LHVT' in the absence of such further experiments; one-third disagreed. When asked about the sort of experiments which would be required before a completely conclusive case could be made against LHV (a question which invited a rather precise, if not pedantic, reply) only two-thirds of the LHV physicists felt that Aspect's experiment, together with the use of more efficient detectors (to rule out biased selection) would definitively settle the issue. I do not think this implies that existing experiments are not persuasive; it simply indicates that loopholes remain and opinions differ about these loopholes. As one respondent put it:

"These [further] experiments would probably convince everyone but the most 'far out' people. The existing experiments have already convinced the sensible people."

It is interesting to note that even these rather limited cases of disagreement among LHV physicists did not occur until after most experiments were complete, and most people had ceased to be actively involved with LHV. This suggests that a distinction can be drawn between disagreements over major and minor issues. Today, further debate about the weakest possible assumption, or the sort of experiment required for a watertight case against LHV, is really rather trivial and unimportant. In contrast, disagreement in earlier years over the plausibility of <u>all</u> assumptions, or the validity of Holt's result, would have had much more serious effects on the LHV activity. Perhaps the most important feature of LHV, as far as these physicists were concerned, was that experimental tests seemed possible. There would have been little point in delaying such tests until complete agreement had been reached about the nature of <u>ideal</u> tests. Thus, the LHV group's common goal - doing experiments - and their shared cultural background in physics allowed and encouraged them to avoid dissension until the important experimental work had been done. There was plenty of time afterwards for individual philosophical preferences to be expressed.

If the above account is correct, we would expect to find that other FQM workers, who did not share the orientation of the LHV group, would not come to the same conclusions about the experiments. This is indeed what we find.

In Chapter Four, I pointed out that many theorists disagreed with the whole idea of doing experimental FQM in this way; to them, itseemed naive and pointless. But this is only one alternative to the LHV group's own attitude: there are many others, each drawing different conclusions from the same experiments.

Many authors have argued that the experiments have important and very general philosophical implications: the world is apparently non-local. However, Bohm feels that this result vindicates his own non-local HVT, a conclusion which the LHV group would certainly not relish; to Sarfatti this result offers the possibility of faster - than - light communication, though Bell disagrees with this; Ballentine claims that, like the Michelson-Morley experiment, this result is likely to lead to a full-scale conceptual revolution, with an outcome as yet unknown, while Stapp is much more sure about the outcome, arguing that we need a Whiteheadian 'process' view of reality. Augelli claims that an acceptance of QM means a rejection of realism and rationalism, while Brody and de la Peña argue that the experiments have no philosophical implications because their theoretical basis is not sound.[99]

Other authors have concentrated on the derivation of the inequalities which allow discrimination between LHV and QM. Several theorists have suggested that the role of the apparatus in altering HV values has been neglected, though these authors do not agree on what the role of the apparatus actually is.[100] Some people have tried to develop new inequalities which they can then apply to the existing data, while others see this as a waste of time.[101] The experimenters themselves do not agree about the implications of their results for the status of QM. For example, Fry and Holt told me that, as far as they were concerned, the results confirm their belief that QM is a good theory. More philosophically-oriented LHV physicists, such as Bell and Clauser, feel that there are still many problems with QM, although LHV is clearly not the solution to these problems.

The Possibility of Alternative Outcomes.

Throughout the analysis of the LHV activity presented in this and the preceding chapter, I have questioned the idea that these events followed an inevitable, unproblematic or 'natural' sequence; instead, I argued that the actual outcome which we observe was highly dependent on the context in which that outcome was shaped.

It may be useful to supplement these arguments by discussing hypothetical situations in which, given a slightly different context, the outcome of events might be quite different. Obviously, any discussion of how things might have turned out in different circumstances must be speculative, and the reader must judge the plausibility of any particular speculation for himself. Nevertheless, this approach may be useful in helping to indicate the problematic, and often rather fragile, nature of actual outcomes.

As an example, consider the assumptions required for the experiments. We have already seen that the assumptions cannot be justified in absolute terms, but because the LHV group happened to share common goals and a common cultural background, the assumptions remained relatively uncontentious. Nevertheless, if there had been a thriving group of LHV theorists who had wished to challenge the assumptions, at the risk of being labelled 'obsessive', it is difficult to see how appeals to 'reasonableness' would have quelled them, since by definition they would not have held the same views as the LHV group about what was or was not reasonable. We may also speculate about what might have happened if the high-energy experiment carried out by Kasday's group had disagreed with Freedman and Clauser's low-energy experiment. These experiments required quite different assumptions, and earlier in this chapter I pointed out that the high-energy group did not readily concede that their assumption was markedly stronger. Since the results agreed, there was no dissension, but if the results had disagreed I would suggest that the relative plausibility of the high and low energy assumptions would have become a matter of much debate.

Let us turn now to Holt's experiment. As I pointed out in Chapter Five, Holt could have chosen to publish his results and even to have defended them. In the present chapter, I have indicated a number of ways in which the hypothetical 'determined advocate of LHV' could have attacked the arguments used to reject Holt's result. It is not inconceivable that Holt, or someone else in his position, might have employed such tactics to defend a result in favour of LHV. For example, it is interesting to speculate about what would have happened if Clauser (who initially hoped to disprove QM) had obtained Holt's results, and vice versa. Clauser himself suggested that they might then have been 'at each other's throats'.[102]

If Holt's experiment had been performed by someone who aggressively defended its validity, this might well have led to a quite different style of debate and a different sequence of events. Of course, it is difficult to argue that such a change would affect the results obtained in (say) Fry's experiment. But it might easily have prolonged the debate over LHV, and prompted different experiments, such as an examination of the difference (if any) between calcite and piles-of-plates polarizers.

The differences between these hypothetical outcomes and the actual outcome are not altogether trivial. A defender of Holt's result might well have put forward a whole range of hypotheses, 'ad hoc' or otherwise, to explain the apparent clash between the rival experiments, and these hypotheses might have led to further experiments and, possibly, surprising results.

To quote an example which has some basis in actual events, Holt was contacted by Nick Herbert, who runs an 'alternative science' institution in California. Herbert's group, like Sarfatti's, is closely involved in the parapsychology movement, and he expressed an interest in Holt's result and its implications. Holt chose not to follow up this contact; indeed, Pipkin told me that one reason why they were reluctant to publish their results was because they did not wish to encourage such unorthodox groups, who might exaggerate the significance of Holt's results. If Holt had been willing to invoke parapsychological factors to account for his anomalous results, the involvement of such groups would be classed by some observers as anything but trivial.[103]

Faraci's group, and their reaction to their own result, provides us with a real example of the defence of an unorthodox result. Admittedly, the defence is rather weak (though, again, other arguments than those actually used are possible) and the hostile reception of this work may suggest that Holt was sensible in following his chosen course of action. Here, too, though, other possible outcomes exist. Faraci's group could have obtained a more favourable reception for their results. Many of the reasons suggested by interviewees for calling Faraci incompetent involve factors which could have been altered, such as Faraci's incorrect claim that his result agreed with Kasday's, and the lack of detail in the published account of Faraci's experiment. The fact that the Italian group relied on theoretical analysis rather than empirical checks for some of their conclusions is not itself a sign of incompetence. Holt similarly checked some possible sources of systematic error by theoretical analysis whereas Clauser, in his 'replication', checked them empirically. Yet Holt managed to emerge with his professional reputation intact. One reason for this, as Holt himself said, was that he not only checked many possible sources of error, but made it very clear, in his written reports, that he had done so. In Faraci's case, many details about their experiment were first explained, in a foreign language, at the 1976 conference.

Putting it crudely, their techniques of image management could have been better.

Thus I do not think it was inevitable that Faraci and his colleagues would be judged to be incompetent. Holt's own case refutes the argument that a result in favour of LHV is an unequivocal sign that the experimenter is incompetent.

Let us turn now to the different fates of the 'timing' and 'selective apparatus' proposals. The evidence presented earlier suggests that the different status of these proposals at the present time results not so much from differences in their inherent quality as hypotheses (if such a thing could be defined) as from the contingent fact that a physicist has chosen to test one of them and no physicist has chosen to test the other. I would therefore suggest that if some apparently sane and competent physicist were to postulate that different types of polarizers may contain different hidden variables, and sets out to test this proposal, then he will get the same sort of subdued but tolerant reception which Aspect received. Physicists would not say publicly that he was crazy, since they themselves drew attention to the assumption which has to be made if we wish to neglect the possibility of selective apparatus. Undoubtedly, though, they would wonder about his professional judgement, because there seem to be much more profitable ways to spend one's time than testing this rather strange idea. Needless to say, although they might tolerate such an experiment, they would not think it likely that any surprising results would emerge.

This, then, is another possible alternative to the actual outcome. Irrespective of whether the results of such an experiment agree with the QM prediction, there is an important difference between neglecting a hypothesis (or rejecting it tacitly or explicitly on grounds of plausibility) and actually testing that hypothesis experimentally. If my argument is correct, then the fact that one outcome rather than another actually occurred is a result of purely contingent sociological features of this activity.

Extending the above arguments to their limit, we arrive at the claim that a 'determined advocate of LHV' could continue to defend his viewpoint even today. As far as I can see, there is nothing in the rules of logic, or in the operation of human psychological processes, which would prevent anyone from claiming either that all the results in favour of QM are wrong, or (a weaker and more defensible claim) that Holt's experiment is, in some important sense, different from all the others. If these experiments had been performed in a context in which LHV was the dominant theory, such a defence would be more likely to occur and, with sufficient ingenuity, LHV might have been defended indefinitely.[104]

Given the actual context in which these experiments took place, it is not at all surprising that the defence of the hypothetical advocate of LHV is not taken seriously. I certainly do not wish to claim that it should be taken seriously. Within the existing culture of physics, into which the LHV group (as well as myself) had been socialized, LHV now seems totally implausible. The important point is that the assessment of plausibility and the evaluation of knowledge-claims has relatively little to do with logical axioms and deductive reasoning; it is a complex judgemental process which cannot be isolated from the cultural context in which it occurs.

Conclusions.

In this rather lengthy chapter, I have presented a great deal of evidence which provides strong support for a number of general conclusions.

In the first place, I have examined a number of beliefs held by the LHV group, concerning the acceptability of the assumptions, the invalidity of Holt's and Faraci's experiments, the invalidity of LHV and the status of the 'timing' and 'selective apparatus' proposals. Although such beliefs seem well-founded, I have tried to show that they were not arrived at by a process of logical deduction, but by reference to the general culture of physics and the local culture of the LHV activity. The grounds on which their apparent certainty is based are ultimately conventional. In making this claim, part of my argument was to show that it is possible to render such beliefs problematic, and to propose defensible alternative beliefs. This argument, therefore, provides evidence in favour of relativism. However, I do not believe that a full-blooded defence of relativism, on ontological or epistemological grounds, is either necessary or helpful. The methodological utility of a relativist approach to the sociology of science is, I would argue, its main value.

In other words, having established in principle that the actual outcome of the LHV activity was not predetermined by the constraints of the natural world, it is then possible to ask meaningful, and rather important, questions about the features of this activity which did cause the actual outcome. One approach to such questions is speculation about other possible outcomes; by considering the factors which might lead to such alternative outcomes, we can begin to identify some of the crucial elements which helped to determine the actual outcome. Another approach is to examine 'deviant' subgroups who drew different conclusions from the LHV activity, and to investigate the differences (in sociological terms) between such subgroups and the 'mainstream' LHV group. A third possibility is to present LHV physicists with a deviant proposal, and to examine the accounts which they give to justify their rejection of such a view.

By using all these methods, we can gain a great deal of information about the determinants of the LHV outcome. It seems clear, for instance, that the acceptability of the assumptions underlying the experiments depended crucially on the fact that the LHV group shared common goals and a common culture, which allowed them to distinguish between 'relevant' and 'irrelevant' features of the experiments, and 'plausible' and 'implausible' counter-objections, in a manner which they found hard to articulate. However, agreement over the allocation of reasonableness cannot be enforced, and other observers, who held different methodological preferences and who did not share the cultural background of experimental physics, did not always agree about the status of the assumptions. It is perhaps fortunate that the LHV activity, being almost wholly experimental, offered little scope for active involvement on the part of anyone who did not adhere to the experimental methodology.

When we turn to the assessment of Holt's and Faraci's experiments, we again see that the rejection of these results was framed in terms of a cultural context which, among other things, provided a set of expectations about 'proper' behaviour, and provided a plausibility structure in terms of which novel ideas were evaluated. However, this shared cultural background did not wholly predetermine the outcome. Individual physicists, such as Holt, were free to choose whether to accept or challenge the conventional plausibility structure.[105] But rejecting convention is risky, as Holt was well aware and as Faraci's case demonstrates. Holt's decision to reject his own result was itself a serious blow to the plausibility of any heterodox conclusion - indeed, one might argue that Holt's rejection of his result was as much a <u>cause</u> of the implausibility of LHV as it was an effect of the implausibility of LHV.

Aspect's case suggests that, by their choice of actions, individual physicists can actively modify the plausibility structure. It is certainly not true that individuals simply respond passively to the constraints imposed on them by their culture. Another (complementary) conclusion which can be drawn from this case-study is that although the natural world does not limit us to one unique set of conclusions to be drawn from empirical evidence, the <u>social</u> world (culture) effectively imposes rather tight boundaries on the range of conclusions which will be seen as plausible. In the abstract, <u>socially</u>-defined conventions may seem much weaker constraints on beliefs than the claim that <u>reality</u> constrains belief. However, the social world is very real and very 'hard' when seen from a member's viewpoint, so that we are very far from the position of claiming that 'anything goes'.

Chapter Seven

Conclusions

In this chapter, I will draw together the main conclusions which can be drawn from the data presented in this thesis. I shall also relate these conclusions to the findings of other empirical studies, and I shall discuss the relationship of these empirical results to the theoretical framework on which this thesis is based. Finally, I shall examine a general model of scientists' response to experiments; after criticising this model, I shall adapt it to bring it into conformity with my empirical findings.

Science and the Wider Cultural Context.

In Chapter Three, I examined the development of QM within two different cultural contexts. I was unable to confirm the claim that QM's formalism was affected by the cultural context of Weimar Germany; however, I concluded that the direction pursued by physicists, their willingness to entertain acausal notions, and their presentation of QM as an acausal theory, may well have been influenced by their cultural milieu. Similarly, Soviet physicists' interpretations of QM (again, not the formalism itself) were adapted to meet the constraints of their social context.

The similarities between these two contexts seem to me to be more important than the differences. Although the nature of the external influences were different, I do not think it is reasonable to conclude, as Born and Heisenberg did, that Weimar physicists were doing 'good' science while Soviet physicists were doing 'bad' science. Such conclusions say at least as much about the observer's own political position as they do about QM. Thus, a clear-cut distinction between external and internal influences on scientific activity is neither a sound basis for evaluating the products of that activity, nor a useful element of historical methodology.

Turning now to the use of scientific concepts in other subcultures, we have seen how elements of QM, such as complementarity and indeterminism, are employed in contexts outside science, as rhetorical resources used to support a particular position. The same element of QM can be used to support diametrically opposite views. Persons who adopt such tactics seem to hold the view that scientific knowledge can be extended unproblematically into other areas almost by a process of simple extrapolation.

However, the evidence suggests that such extensions involve not only careful choice of the 'relevant' aspects of QM from the totality of the theory (including all its possible interpretations) but also an active shaping of that aspect in order to present it as relevant. This creative model of such extensions raises the question of discriminating between 'valid' and 'invalid' extensions of QM. I argued that this distinction is meaningless.

This conclusion can of course be interpreted in at least two ways. Either all extensions are valid (where 'validity' is used weakly to signify the possibility of constructing many accounts; the social utility of such extensions would depend on the context) or all extensions are invalid (that is, there is no logically necessary connection between physical theories and theological beliefs). Both readings are correct, though each may appeal to different readers.

Thus, a study of QM in its cultural context demonstrates not only how cultural influences may operate on the development of a physical theory, but also how elements of a physical theory may be applied to contexts outside physics. I have also suggested that evaluative concepts such as 'validity' cannot be applied unequivocally to <u>either</u> sort of interaction.

Other studies of the relationship between science and culture are in broad agreement with these findings[1], and therefore provide support for the theoretical framework described in Chapter One. As well as providing support for these general ideas, QM also has three important advantages as a case-study. First, this single theory provides evidence concerning cultural effects <u>on</u> science and cultural utilisation <u>of</u> science. Second, the theory has been used to support, not just one or two, but a very wide range of (conflicting) views, so that the role of the theory (as a resource, not as a source of logical deductions) is made manifest. Third, and perhaps most important, the case of QM may help us to clarify the precise nature of the interaction between science and culture. It is very easy to claim that 'science is part of the cultural context', without specifying in any way the means by which culture, as it were, 'gets into' apparently esoteric activities like physics, which seem to be motivated by internal technical criteria. Similarly, we may inquire how it is that a scientific theory which, in its own area of application, may yield precise unequivocal predictions, can be used to support a variety of often conflicting views about non-scientific issues.

In QM, largely for contingent historical reasons, interpretation has remained distinct from formalism; indeed, interpretation has continued to be a focus of debate long after the formalism was agreed. Chapter Two illustrated the many ways in which the physical implications of the formalism could be interpreted. It should not surprise us to find that an even greater range of conclusions can be drawn concerning QM's implications for other fields.

Thus, the suggestion is that, although QM may be atypical in its overt differentiation between formalism and interpretation, other scientific theories can, in principle, be analyzed in the same way. The 'interpretation' component is negotiable in rather obvious ways. Here, then, is an aspect of scientific theories which allows culture to 'get in', and which admits of exploitation outside science in a multitude of ways. I do not wish to imply that other parts of scientific theories - the formalism, or the 'core' - are immune from external cultural influences. In Chapter Two, we saw that some critics of QM feel that the formalism itself should be modified. In Chapter Three, I even argued that an entire alternative theory to QM might be possible. Nevertheless, it seems to be the case that scientists routinely try to construct and maintain boundaries between those aspects of science which are 'properly' the subject of public debate, and those aspects which are considered to be internally determined. Further studies of this boundary maintenance, and the social processes which underlie it, would be very welcome.[2]

Social and Cognitive Structure of FQM.

In Chapter Four, it was found that FQM is a rather fragmented field, in both sociological and cognitive terms. These two aspects seem to be interrelated. The absence of an organised system of training, communication and rewards makes it difficult for cognitive consensus to emerge; at the same time, the absence of a common theoretical and methodological framework hampers the formation of such a social system. The contrast with the development of LHV, discussed in Chapter Five, helps to emphasise these points. LHV seems to have followed a fairly typical pattern of growth, as described by other students of scientific specialties.[3] The rest of FQM does not seem to have progressed beyond the initial stage, with a variety of often-conflicting perspectives and little communication. Indeed, it may be best to describe FQM as a 'failed field'[4] rather than as a field which is 'stuck' at the first stage of development. Mulkay, for example, admits that the "early stages of growth can only be recognized retrospectively".[5] Since areas such as FQM are, by definition, nebulous and therefore difficult for sociologists to study (or detect[6]), such areas may be more common in science than we at present realize.[7]

In Chapter Four, I also discussed the role of methodology in disputes in FQM. Methodological differences between FQM workers reflect the variety of origins of such workers, and seem to be one of the main causes of the lack of communication between disputants. To this extent, my findings agree with those of Pinch. However, I criticised Pinch's use of 'arithmomorphism' and related concepts. This usage, like those of complementarity cited earlier, seems to imply that conflict was somehow inevitable. It ignores the negotiability of descriptive terms which we have encountered in this thesis.

My alternative account of disputes in FQM rests on two general principles. One is that actors, who may fundamentally differ on matters such as methods and goals, may choose to frame their attacks on their opponents in terms of other issues, as a way of legitimising their position. This seems to have been the role of von Neumann's proof in the rejection of Bohm's theory.

The second principle is that meanings are constructed by practice. The disputed 'cognitive object', such as the Bohm-Bub theory, has a different epistemological status for each participant in the dispute. This status reflects the participants' views about what one ought to do with the cognitive object.

Of course, the existence of fundamental differences in methodology or perception does not imply that disputes in FQM can never be resolved. 'Incommensurability' should not be taken to mean that opponents are wholly unable to communicate because of psychological blocks. Bell, for instance, managed to explain why Bohm's theory was unaffected by von Neumann's theorem, and Bell's account is perfectly meaningful to both Bohm and contemporary axiomatists. However, the existence of different methodologies means that it is often difficult in practice for opponents to appreciate each other's point of view, and these differences are unlikely to be resolved simply by polite discourse. More fundamental shifts in goals or methods would normally be required.

Social Context and Scientific Practice.

It should not be thought that sociological accounts of science are only relevant for a study of controversies. One of the aims of Chapter Five was to indicate the socially-regulated nature of routine scientific activity. In a sense, this point is obvious: the existence of regulatory mechanisms (such as the refereeing system) and stylistic conventions (for example, in writing scientific papers) are well-known. The normative view of science presents a picture of a social activity which is governed by ethical as well as behavioural rules. Viewed from within this persepctive, some of my findings are not unexpected. For example, the LHV workers were clearly aware of the conventions of scientific writing, and they presented accounts of their actions which referred to general procedures, such as the 'proper' response to an anomalous experiment.

However, I went on to argue that the normative perspective does not in fact provide a fully satisfactory account of FQM. For example, it does not adequately deal with the element of choice which I believe was present at many points in the LHV activity. Physicists chose to become involved in LHV because they judged it to be advantageous, given their particular social location. Similarly, their style of presentation, with a strong contrast drawn between LHV and all other FQM, cannot be completely justified by the 'facts'; LHV was not devoid of theoretical assumptions, despite the rhetoric about 'decisiveness'. However, such behaviour is perfectly understandable given the particular historical and social context of FQM, and the sociological reasons for the desire to differentiate between LHV and other FQM.

When we examine the response to Holt's experiment, a similar picture emerges. In the case of Holt's own response, there was a clear concern with 'making out' - that is, behaving in a way which seemed to offer the best possible outcome from Holt's personal viewpoint. Holt was also keenly aware of the constraints and opportunities presented to him by his local social context. We could if we wished describe his response in terms of general 'rules' dealing with 'how one should respond to anomalous results'. But this would certainly miss a great deal of the detail of Holt's behaviour; it would also lead us to neglect the existence of choice. Faraci's case demonstrates clearly that, in a similar position, other options existed.

An account of Fry's and Clauser's response in terms of rules governing the reaction to anomaly again seems to miss the point. Not all anomalous results generate practice. These physicists had to choose to classify Holt's result as the sort of anomaly for which further investigation was appropriate before it made sense for them to describe their response in terms of empiricist norms. These norms, then, were a way of characterising their actions for rhetorical purposes, to help Fry to get a grant and to help Clauser fulfil his aim of raising the status of this field.[8] The norms did not govern their actions, except in a very weak sense - namely, having chosen to portray their actions as an experimental investigation of an anomaly, Clauser and Fry had to follow the normal conventionalised procedures of experimentation and presentation of results.

In Chapter Six, I also examined the methodological prescriptions made by some LHV physicists, and I argued that these prescriptions, together with the 'success' of the LHV activity, altered the social context in which non-empirical FQM workers operated. Here, too, we saw a range of responses, with some people proposing experiments and others attacking the experimental methodology as 'naive'. This seems to be another example of scientists altering their public statements, and to some extent their practice, in response to a changing social context. We can also see here the other side of the interaction between science and its social context: not only does the social context set limits on actions, it can also be affected by actions - in this case, those of the LHV group.

Plausibility and the Evaluation of Knowledge.

In Chapter Six, I examined the content of science, and the ways in which scientific knowledge is evaluated. This might be considered to be the most important part of this study. After all, if the content of science is immune from social processes, and is generated and evaluated according to wholly impartial criteria, then social processes are, in an important sense, quite trivial - 'truth will out'.

I examined a number of cognitive issues in the LHV activity, and argued that the validity or invalidity of theories, assumptions, and experiments, is evaluated by drawing on elements of the culture of physics. I used the term 'plausibility structure' to refer to the evaluative framework which is part of each physicist's culture, and which helps him to classify hypotheses as 'reasonable', 'likely', 'pathological', and so on.

By examining situations in which consensus broke down, I concluded that labelling a hypothesis as 'unreasonable' does not enforce consensus; it simply demonstrates the labeller's own preconceptions, assumptions and preferences - in short, his own plausibility structure. Non-experimental FQM workers have a different plausibility structure, so it is not surprising that they disagree about the cognitive status, as well as the methodological utility, of the LHV activity.

As with scientists' behaviour, so with their beliefs; we should not conclude that any belief is acceptable just because social, rather than purely empirical, factors, are involved in evaluation. At any given time, certain theories are judged to be more plausible than others, just as QM was seen to be more plausible than LHV. We should therefore not be unduly surprised that QM 'won' the encounter. However, the fact that social factors are involved makes it possible for the basis of evaluation to change over time. I argued that Aspect succeeded in increasing the plausibility of the timing hypothesis, at least temporarily.[9]

Moreover, we now have a mechanism to back up the relativist claim that all knowledge is negotiable. Relativism does not mean that all beliefs are negotiable here and now. One of the advantages of the term 'plausibility' is that it allows us to reconcile the apparent solidity of our current beliefs with the negotiability of belief revealed to us in microsociological studies.

Microstudies illustrate the tactics used by scientists to gain plausibility for their beliefs.[10] It can be argued that, if traced back far enough, all our present beliefs arose from such processes of negotiation sometime in the past, and that our current beliefs only seem certain and unshakeable because they are supported by contemporary social institutions, which act as sources of credibility and legitimation. Putting it crudely, the plausibility of a belief, here and now, is a manifestation of the distribution of power within our knowledge-related institutions at some time in the past.

Since the social context is dynamic, accepted beliefs very quickly cease to be easily comparable with rejected beliefs, because the former become the basis for future practice, generating new areas of cognitive development which in turn may modify or support the accepted beliefs. One form of empirical support for this view would require two (or more) studies of a particular set of beliefs at different points in time. Collins has performed such studies, and his findings provide strong support for this view.[11] He found that the existence of high fluxes of gravity waves was never accepted, but was initially taken seriously enough to be worth discussing and testing. On the other hand, a few years later,

"The existence of high fluxes of gravity waves is now literally <u>incredible</u>. My claim is not that sociology can bring them back, but that their demise was a social (and political) process."[12]

Although there is already a great deal of evidence in favour of the relativist view[13], many people seem to find it objectionable on an intuitive level.[14] I believe that the concept of plausibility can help to defuse such objections, by allowing us to acknowledge the seeming 'hardness' of our current beliefs, while providing a sociological explanation of this 'hardness'. Social conventions, after all can be just as hard to break as 'natural laws'. Territorial boundaries, and the grammatical structure of languages, are two examples of conventions which, for different reasons, are hard to change.

Another important advantage of plausibility is that it allows us to classify different sorts of 'negotiations' in science, and to pin down our intuitive feeling that, in a given context, some things are more negotiable than others. To clarify these issues, it may be useful to discuss models of negotiation in more detail.

Models of Negotiation.

In his studies of the construction of a laser, the construction of gravity wave detectors, and the replication of parapsychology experiments, Collins has developed the concept of negotiation over the competence of an experiment.[15] To illustrate this concept, he constructed a classification grid[16] to describe the attitudes of scientists to particular experiments (Figure 1).

The argument underlying this grid is that actors can in principle deflect the force of any critical experimental finding by arguing that the experiment in question is not really measuring the effect it is supposed to be measuring. Such arguments were in fact found in both the gravity wave and parapsychology case-studies. These findings support the view that 'reality', or our interactions with the natural world, is never, by itself, capable of settling such disputes. A further conclusion is that sociologists should not use the 'laws of nature', or empirical evidence, as a way of unproblematically accounting for the fact that most disputes are eventually settled. As Collins puts it:

"the originator can argue indefinitely that experiments which claim to be disconfirmations of his results, are not good replications."[17]

Nevertheless, there are many areas in science where experimental results are not treated in this way. These areas are those in which the phenomenon being tested is universally accepted. In such areas, everyone knows what a competent experiment looks like: a competent experiment produces the 'correct' result. Collins has suggested that in such cases, boxes 2 and 4 disappear[18]. An example of such a context is the construction of a laser (Figure 2). Here, instead of the phenomenon being defined by the outcome of a dispute over the competence of experiments, we find that the phenomenon, accepted by everyone, serves as a benchmark against which the competence of individual experiments (that is, whether or not they 'work') is assessed.

Such restrictions on negotiation do not weaken the relativist case; anyone who fails to construct a working laser is always at

		Believe in Phenomena under Investigation	
		YES	**NO**
Find results consonant with this belief	**YES**	1. Competent	2. Not competent
	NO	3. Not competent	4. Competent

Figure 1: Attitudes to Experiments (Collins' Model)

		Believe in the Phenomenon of Lasing	
		YES	**NO**
Construct a Working Laser	**YES**	1.Competent	
	NO	3.Not competent	

Figure 2: Attitudes to Laser Construction (Collins' Model)

liberty to argue that what he has built is indeed a working instrument, but one which is demonstrating a new effect.

Nevertheless, the contrast between figures 1 and 2 is so striking that we should make an effort to subsume them both within a more general model of negotiation. Any symbolic representation which fits on a piece of paper will inevitably be a gross simplification of the real social world. Yet such models may serve useful heuristic and explanatory functions. We should therefore try to extend and improve our models whenever possible.

It is clear that figures 1 and 2 do not represent all possible situations; to some extent, the situations which they depict can be considered to lie at opposite ends of a spectrum.[19] This portrayal is valid regardless of whether we accept a relativist account. For an empiricist, there are occasions when we are sure of the facts, occasions when we have no idea who is correct, and other occasions when at best we have preferences for one side rather than the other. For a relativist, the variable factor is not truth, or our knowledge of the natural world, but the degree to which certain beliefs and practices have become reified and institutionalised. We can describe this variable factor as 'plausibility'.

The fact that rival theories in a controversy often do not start off with equal plausibility has the effect of setting practical limits to the amount of negotiation which is likely to occur. In my own case-study, I could not help feeling that QM would inevitably 'win', despite my recognition of the relativist argument that:
"there is nothing outside of 'courses of linguistic, conceptual, and social behaviour' which can affect the outcome of these arguments."[20]

The point is, of course, that controversies do not take place in a vacuum; they are embedded in a wider social and cultural context with a pre-existing plausibility structure which, despite its conventional, relative character, is a factor - perhaps a determining one - in shaping the conduct, duration and outcome of controversies.

It is perhaps understandable that early relativist studies of science chose to emphasis the negotiability of scientific beliefs, and to play down factors which restrict these negotiations. It is an encouraging sign that such limiting factors are now being recognised.

For example, in an introduction to a collection of empirical papers, Collins writes:

"This interpretative flexibility was the main message of the 'first stage' of the relativist empirical programme. At the same time the papers go on to begin what might be called 'the second stage of the programme' by describing mechanisms which limit interpretative flexibility and thus allow controversies to come to an end."[21]

Let us now consider specific proposals for modifying Collins' graphical representation of controversies, in order to take these limiting factors into account.

There are at least three factors involved in scientists' evaluation of an experiment, or of the hypothesis which the experiment is testing. One is, clearly, the results themselves. It can be argued that an experimental result is not a truly independent variable, since the meaning of the result, and its implications for the hypothesis being tested, require interpretation; theoretical and other cultural factors are involved in this process. Nevertheless. there is, I would argue, a sense in which results are fundamental data in controversies, provided we are careful to discriminate between 'results' and 'interpretation of results'.

For example, I would not wish to claim that Holt's experiment proved that QM was wrong, nor that it proved that Holt had made an error. However, it would be difficult to deny the claim that Holt's results showed a level of polarization correlation much lower than that predicted by QM. The distinction, and the point at which we draw the line between 'raw data' and 'interpretation of data' is a hazy one, but within any particular context the distinction is not hard to make. As a methodological rule-of-thumb, we are effectively dealing with raw data when we reach a description of the results with which all the participants in a dispute would agree[22].

The second factor is what Collins has described as the actors' belief, or lack of belief in the phenomenon being tested. However, reference to 'belief' seems rather unhelpful, on several grounds. The first is the general methodological problem, discussed in Chapter One, that actors may produce different accounts of their beliefs in different contexts. Alternative terms such as 'allegiance' or 'commitment' can be more easily defined in behavioural terms.

The second problem is that, in Collins' grid, different actors hold diametrically opposite commitments. This yes/no dichotomy is not only simplistic, in that it does not allow for a range of opinions, but it also implies that scientists are deeply committed to a particular view. While this may be true in full-blown controversy, it does not seem to be true for the many uncontroversial areas in science which fit Kuhn's description of 'normal science'[23]. Scientists may have mild preferences for particular hypotheses, but when faced with critical experimental results they may reject that hypothesis, not because it had been conclusively disproved, but because their commitment to it was not strong. If, on the other hand, the hypothesis had been more important to them, they may then have adopted the strategy of rejecting the experiments which generated the conflicting results.

Figure 3 illustrates the above arguments in the case of LHV. Both Holt's experiment, and Freedman and Clauser's, gave clear results, one in favour of LHV and one in favour of QM. As described in Chapter Five, Holt, like Freedman, favoured QM while Clauser held at least a mild hope that LHV would be confirmed. However, Clauser rejected his favoured theory when he obtained his results, while Holt retained his commitment to QM and rejected his results. The lack of symmetry between boxes 2 and 3 might be thought to reflect the different levels of personal (psychological) commitment to the chosen theory. However, this assymmetry seems more likely to be an effect of the great difference in plausibility between QM and LHV.

Plausibility, the third factor in my account, is a sociological variable, referring to the status of a belief within the community as a whole. Provided the individuals involved have been socialized into the professional community, we do not need to worry too much about their individual psychology. Indeed, I would argue that a strong personal commitment to a hypothesis which exceeds, or conflicts with, the general cultural commitment to that hypothesis (the plausibility of the hypothesis) is relatively rare in science. I would therefore wish to replace Collins' grid with a diagram such as that shown in Figure 4.

The shaded region represents the vast bulk of scientific activity in which actors have not personally committed themselves in advance to a particular point of view. This area might be described

		Allegiance to QM or LHV: QM	Allegiance to QM or LHV: LHV
Results	Pro-QM	1. Competent Experiment (Freedman)	2. Competent Experiment (reluctantly?) (Clauser)
	Pro-LHV	3. Incompetent Experiment (Holt)	4. -

Figure 3: Attitudes of LHV experimenters to their own experiments

		Commitment to Phenomenon Under Investigation: YES	NEUTRAL	NO
Find Results Consonant With this Commitment	YES	1. Competent		2. Not competent
	NO	3. Not competent		4. Competent

Figure 4: Attitudes to experiments

as one of 'good science' or 'empiricism', with one very important proviso. The general cultural plausibility of the hypotheses being tested has still to be taken into account. This may indeed be a region of 'normal science', but it is not a region in which scientific activity is immune from social processes. Theory-laden observation terms are still employed, conventional practices are still followed, and knowledge is still evaluated in terms of a culturally-generated plausibility structure.

Thus, a general model of scientists' attitudes to experiments would have as its axes 'results' and 'plausibility', with the possibility of a third axis, to represent 'allegiance' or 'commitment'; most science, however, would have a co-ordinate near zero on this third axis. Figure 5 is a tentative attempt to describe science in this way. Let us consider a few of its main features.

The first point to note is that the diagram is roughly symmetrical. Experiments which confirm plausible theories and refute implausible ones are both likely to feature in science textbooks. Only theories on the extreme left of the diagram will normally be tested by science students, and the student's competence will be judged by the extent to which his results agree with expectations. However, it is also permissible for students in liberal institutions to test implausible theories such as Aristotelian mechanics and phlogiston theory, though again one must find the 'correct' answer. Theories which are less central to the plausibility structure are routinely tested, though the conclusions drawn from the results depend not only on the plausibility of the hypothesis but also on the 'strength' of the results. Thus, when results conflict with predictions, controversy may, but need not, develop. In any particular case, the outcome depends on microsociological factors such as whether the individuals involved wish to, or are allowed to, publicise anomalous results[24].

The diagram allows for hypotheses which range continuously from 'very plausible' (the tacit knowledge which is seldom treated as anything other than fact), through a middle range (in which hypotheses are seen as tentative and are readily rejected if experimental results appear to refute them) to 'very implausible' (the archetypal case being discarded theories from the history of science).

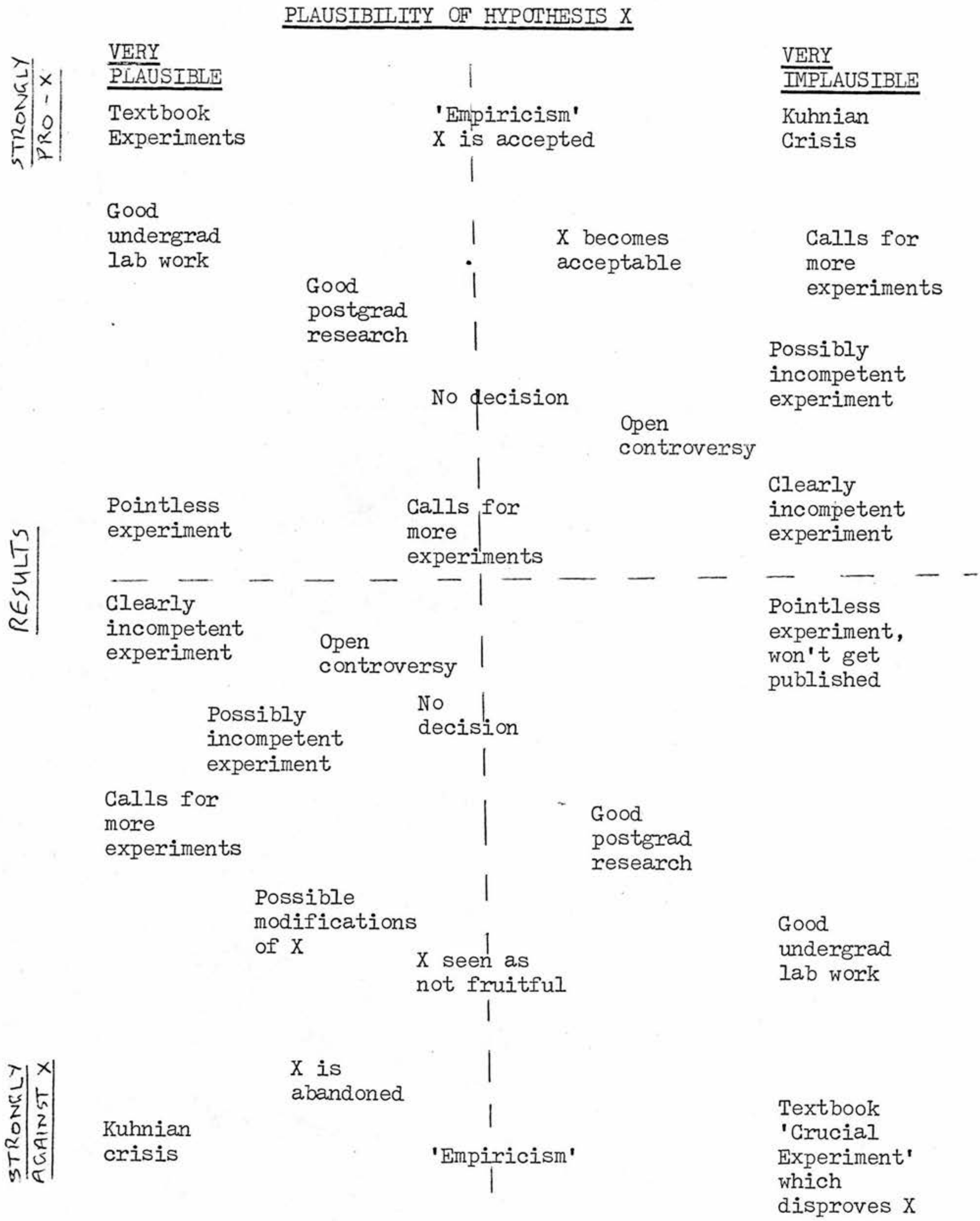


Figure 5: Attitudes to Experimental Tests of Hypotheses

Experimental results (bearing in mind the distinction between raw data and interpretation discussed earlier) also range continuously from results which strongly support the hypothesis in question (an example from QM being Fry's experiment) through 'non-committal' results (such as Clauser's circular-polarization experiment) to results which are in strong conflict with the hypothesis (such as Holt's experiment).

Within this continuous two-dimensional range, I have indicated the likely reactions to experiments at a number of points. The selection and precise location of such points may well be idiosyncratic. However, I believe the reactions described would be broadly typical for the locations shown.

The fact that the interpretation of results is not wholly independent of the theoretical framework may seem to weaken the validity of this model. It may seem impossible to assess the 'strength' of a result in an independent way. After all, we have already seen how apparently damaging results can become redefined as incompetent experiments.

The solution to the problem lies in the fact that science is a historical process. Just as there is a logical <u>and</u> chronological distinction between raw data (such as a correlation rate) and interpretation (such as the implication of Holt's experiment for QM), so there is a possibility of identifying the initial 'strength' of an experiment <u>before</u> the social processes of negotiation have been completed. Within a single workplace, such redefinition can be extremely rapid[25]; however, when the community as a whole is examined, changes over a period of years can often be discerned[26].

In any case, there are many contexts in which the interpretation of a result is relatively unproblematic and stable over time. For example, in the construction of a laser, or the assessment of an undergraduate lab experiment, the status of the results (right or wrong) is not in practice open to negotiation. Thus it is only in areas of controversy that the 'strength' of results will be renegotiated over time as the controversy is resolved. This will involve a shift on the graph of Figure 5; for example, 'results in conflict with a plausible hypothesis' will eventually be resolved either as 'incompetent experiment' or 'hypothesis modified'. The particular outcome will of course depend on many microsociological factors.

A diagram such as Figure 5 has, I believe, several advantages. In the first place, it provides a simple picture of scientific development which is recognisable to both the empiricist and the relativist. Of course, such observers would totally disagree about the role of the two factors shown. The empiricist would argue that shifts in plausibility are a result of experimental data, while the relativist would argue that the meaning of experimental results is negotiated to conform with the dominant plausibility structure. Nevertheless, the diagram usefully points to some of the many features of scientific development which are common to both relativist and empiricist models.

Another advantage is that such a diagram may help to make relativism, whether methodological or epistemological, more appealing, because it explicitly allows a role for empirical data as one (though only one) factor in shaping events. Science, as a social system, attributes a rather special institutionalised role to empirical data. It is possible to acknowledge the sociological implications of this special role while denying any epistemological justification for this distinction.[27]

A final advantage of this model is that it could, in principle, be predictive. I have already discussed how a particular conjunction of results and plausibility may shift its position over time. It should be possible to detect and monitor such shifts over a long period of time[28], and to investigate the features of social processes in science which cause either the meaning of results or the plausibility of hypotheses to be redefined. In Chapter Six I drew some tentative links between Aspect's actions and changes in the plausibility of the timing hypothesis[29]. Collins has taken two 'snapshots' of gravity wave experiments, and has claimed that the status of these experiments has also changed, and can be attributed to a number of social processes. Similar studies, with intensive <u>continuous</u> interaction with scientists over long time periods, might be highly revealing.

Footnotes to Chapter One

1) For more detailed accounts of this theoretical framework and its relevance for empirical studies, see B.Barnes, Scientific Knowledge and Sociological Theory (London: Routledge and Kegan Paul, 1974); D.Bloor, Knowledge and Social Imagery (London: Routledge and Kegan Paul, 1976); and M.Mulkay, Science and the Sociology of Knowledge (London: Allen and Unwin, 1980).

2) H.M.Collins, 'Stages in the Empirical Programme of Relativism', Social Studies of Science Vol 11 (1981), 3-10, quote at 3, emphasis in original.

3) One of the best illustrations of Popper's conception of knowledge is provided by the following striking metaphor: "The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being."
K.Popper, The Logic of Scientific Discovery (London: Hutchinson, 1968), quote at 111.

4) See, for example, I.Lakatos, Proofs and Refutations (Cambridge: Cambridge University Press, 1976).

5) I.Lakatos, 'History of Science and its Rational Reconstructions', in Buck and Cohen (eds), Boston Studies Vol 8 (Dordrecht: Reidel, 1971).

6) M.Polanyi, Personal Knowledge (London: Routledge and Kegan Paul, 1958); T.S.Kuhn, The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1970); M.Hesse, The Structure of Scientific Inference (London: MacMillan, 1974). For a clear, readable introduction to these philosophical issues, see A.F.Chalmers, What is this thing called science? (Milton Keynes: Open University Press, 1978).

7) See, for example, L.Wittgenstein, Philosophical Investigations (Oxford: Blackwell, 1953).

8) P.Berger and T.Luckmann, The Social Construction of Reality (Harmondsworth: Penguin, 1971).

9) See, for example, H.Garfinkel, Studies in Ethnomethodology (Englewood Cliffs, NJ: Prentice-Hall, 1967); and R.Turner (ed) Ethnomethodology (Harmondsworth: Penguin, 1975).

10) Garfinkel, op.cit. note 9, 79-94.

11) B.Latour and S.Woolgar, Laboratory Life (Beverley Hills: Sage, 1979) and S.Woolgar, 'Writing an Intellectual History of Scientific Development: The Use of Discovery Accounts', Social Studies of Science Vol 6 (1976), 395-422.

12) See, for example, Barnes, op.cit. note 1; B.Barnes, Interests and the Growth of Knowledge (London: Routledge and Kegan Paul,

1977); and D.Bloor, op.cit. note 1.

13)Collins' most explicit defence of relativism appears in H.M.Collins and G.Cox, 'Recovering Relativity: Did Prophecy Fail?' Social Studies of Science Vol 6 (1976), 423-44. See also the exchange of views between these authors and John Law, ibid., Vol 7 (1977), 367-72 and 372-80.

14)For a thorough discussion of the internal/external dichotomy, and reasons for its rejection, see Barnes, op.cit. note 1, Chapter 5.

15)S.Shapin, 'The politics of observation: cerebral anatomy and social interests in the Edinburgh phrenology disputes', in R.Wallis (ed) On the Margins of Science: the Social Construction of Rejected Knowledge, Sociological Review Monograph 27 (Keele: University of Keele, 1979), 139-78.

16)A concise critique of the normative view is provided by M.Mulkay, 'Sociology of Science in the West', Current Sociology Vol 28 (1980) 1-183, especially pp 43-64. For a more sympathetic treatment of Merton's view of science, see J.Gaston, 'Sociology of Science and Technology' in P.T.Durbin (ed) A Guide to the Culture of Science, Technology and Medicine (London: MacMillan, 1980), 465-526.

17)H.Becker, Outsiders (New York: Free Press, 1963).

18)M.Pollner, 'Sociological and Common-Sense Models of the Labelling Process', in Turner, op.cit. note 9, 27-40.

19)Mulkay, op.cit. note 1 and op.cit. note 16.

20)See, for example, J.Harwood, 'Heredity, Environment and the Legitimation of Social Policy' in Barnes and Shapin (eds) Natural Order (Beverley Hills: Sage, 1979); P.Forman, 'Weimar Culture, Causality and Quantum Theory 1918-27', Historical Studies in the Physical Sciences Vol 3 (1971), 1-115; D.A.MacKenzie, Statistics in Britain 1865-1930: The Social Construction of Scientific Knowledge (Edinburgh: Edinburgh University Press, 1981).

21)See, for example, D.O.Edge and M.J.Mulkay, Astronomy Transformed (New York: Wiley, 1976); D.Crane, Invisible Colleges (Chicago: University of Chicago Press, 1972).

22)See, for example, H.M.Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', Sociology Vol 9 (1975), 205-24; B.Wynne, 'C.G.Barkla and the J Phenomenon: A Case Study of the Treatment of Deviance in Physics', Social Studies of Science Vol 6 (1976), 307-47; Latour and Woolgar, op.cit. note 11; G.D.L.Travis 'Replicating Replication? Aspects of the Social Construction of Learning in Planarian Worms', Social Studies of Science Vol 11 (1981), 11-32; H.M.Collins, 'Son of Seven Sexes: The Social Destruction of a Physical Phenomenon', ibid., 33-62; Andrew Pickering, 'Constraints on Controversy: The Case of the Magnetic Monopole', ibid., 63-94; T.J.Pinch, 'The Sun-Set: The Presentation of Certainty in Scientific Life', ibid., 131-58. Other useful collections which contain empirical case-studies include Barnes and Shapin, op.cit. note 20; Wallis, op.cit. note 15; and K.D.Knorr, R.Krohn and R.D.Whitley (eds) The Social Process of Scientific Investigation

Sociology of the Sciences Yearbook, Vol 4 (Dordrecht: Reidel, 1980).

23) Collins, op.cit. note 2, 4.

24) S.Shapin, 'History of Science and its Sociological Reconstructions' (Edinburgh: unpublished mimeo, Science Studies Unit, University of Edinburgh, June 1981), quote at 8.
To be published in History of Science, Vol 22 (1982)

25) ibid, 21.

26) T.J.Pinch, 'What does a Proof Do if it Does not Prove?', in E.Mendelsohn et al (eds) The Social Production of Scientific Knowledge, Sociology of the Sciences Yearbook, Vol 1 (Dordrecht: Reidel, 1977), 171-215.

27) Interview with D.Bohm, Birkbeck College, London, 5th July 1977.

28) In a recent review, Hendry has questioned some of Forman's conclusions. This will be discussed in Chapter Three. See J.Hendry, 'Weimar Culture and Quantum Causality' in C.Chant and J.Fauvel (eds) Darwin to Einstein: Historical Studies on Science and Belief (Harlow: Longman, 1980), 303-26.

29) L.R.Graham, Science and Philosophy in the Soviet Union (London: Allen Lane, 1973).

30) D.Joravsky, Soviet Marxism and Natural Science 1917-32 (London: Routledge and Kegan Paul, 1961); R.Bentley, The Relationship between Dialectical Materialism and Soviet Quantum Mechanics (unpublished MSc thesis, University of Sussex, 1972).

31) This problem is of course a very general one, and has been discussed by many authors. For example, in an early but insightful treatment of this problem, Wright Mills writes:
"Rather than fixed terms 'in' an individual, motives are the terms with which interpretation of conduct by social actors proceeds"
C.Wright Mills, 'Situated actions and vocabularies of motive', American Sociological Review Vol 5 (1940), 439-52, quote at 439 (emphasis in original).
For a discussion of other methodological problems for the sociologist of science, see H.M.Collins, 'The Investigation of Frames of Meaning in Science: Complementarity and Compromise', Sociological Review Vol 27 (1979), 703-18; and M.J.Mulkay, 'Methodology in the Sociology of Science: Some Reflections on the Study of Radio Astronomy', Social Science Information Vol 13 (1974), 107-19.

32) Pinch, op.cit. note 22.

33) G.N.Gilbert, 'Being Interviewed - a Role Analysis', Social Science Information Vol 19 (1980), 227-36.

34) ibid., 233.

35) For example, Latour and Woolgar (op.cit. note 11) adopt this position. Ultimately, of course, such a position is forced on any author who takes a relativist approach seriously. While this may seem a weak position, it can be argued that a sociologist whose account of science is given equal (rather than inferior) status to the account of a scientist is not doing at all badly!

36)Bill Harvey, 'Rationality, Relativism and the Sociology of Science: The Case of Local Hidden Variable Theory' (unpublished mimeo: Science Studies Unit, University of Edinburgh, April 1978).

37)P.J.Werbos, 'Experimental Implications of the Reinterpretation of Quantum Mechanics', Nuovo Cimento Vol 29B (1975), 169-77; P.J.Werbos, 'Experiments on the Reinterpretation of Quantum Mechanics: Corrections and New Ideas', ibid., Vol 37B (1977), 24-34.

38)P.J.Werbos, 'A New Cosmology: Physical Aspects of the Universe', The Rosicrucian Digest (August 1976), 8-11; and 'A New Cosmology: Psychic Aspects of the Universe', ibid., (September 1976), 6-7 and 32-33.

39)Collins, noting Werbos' "ambiguous" status, agrees that he "probably" should not be included in the "core-set" of the controversy over tests of QM. See H.M.Collins, 'The Place of the Core-Set in Modern Science: Social Contingency with Methodological Propriety in Science', History of Science Vol 19 (1981), 6-19. Collins is, I feel, being too cautious here; according to his definition of the 'core-set' (roughly, those people who have made an active contribution to a particular problem), Werbos is not a member. None of the experimenters had heard of him; his work was virtually ignored. I shall argue in Chapter Six that scientists cannot be considered to be effective participants in a debate unless they are perceived by other participants as at least minimally competent.

40)Collins has argued (op.cit. note 39, especially footnote 4, p. 17) that there is still a role in a relativist analysis for a concept of 'incorrect scientific method'. He suggests two such roles: as a "native member's device for describing experiments considered to be wrong, or incompetently performed", which seems uncontentious, and "as a description of cases where all parties were agreed that incorrect procedures had been used", which is less acceptable. Within the relativist view, consensus has no special epistemological significance. We may inquire whether there is any fundamental difference between consensus, and a situation where a single person dissents from the majority view. If there is none, then why should labels like 'incorrect' only be applicable in one case and not the other? 'Incorrect' is always a social term, not an absolute label. It is with the hope of avoiding such terminological confusion that I shall develop the concept of 'plausibility' in Chapters Six and Seven.

41)The term 'plausibility structure' is taken from the writings of Peter Berger, who uses the term in a similar way, but to describe central features of our social identity rather than scientific theories. For example, in a discussion of religion, Berger writes: "Worlds are socially structured and socially maintained. Their continuing reality, both objective (as common, taken-for-granted facticity) and subjective (as facticity imposing itself on individual consciousness) depends upon specific social processes.... Thus each world requires a social 'base' for its continuing existence as a world that is real to actual human beings. This 'base' may be called its plausibility structure."

Peter Berger, The Social Reality of Religion (London: Faber and Faber, 1969), quote at 45, emphasis in original.

Because scientific theories are not as unshakeable as more central elements of our social identity, changes in the plausibility structure are not as threatening. Nevertheless, as we shall see in Chapter Six, the plausibility structure is resistant to change, and is itself an important factor in determining the outcome of scientific investigations.

Footnotes to Chapter Two

1) Detailed analyses of the interpretation of QM, with many references, are provided in the following books: Max Jammer, The Philosophy of Quantum Mechanics (New York: Wiley, 1974); B.D'Espagnat, Conceptual Foundations of Quantum Mechanics (Reading, Mass.: Benjamin, 1976); F.J.Belinfante, A Survey of Hidden Variable Theories (Oxford: Pergamon, 1973); F.J.Belinfante, Measurement and Time Reversal in Objective Quantum Theory (Oxford: Pergamon, 1976).

2) It is true that some classical quantities, such as the velocity of single gas atoms within a large volume of gas, may be practically impossible to measure in some situations. However, within classical physics it is always considered possible to improve the precision of our measurements without limit, at least in principle. In addition, it is always meaningful to describe an atom as possessing a specific velocity, even if we only have limited statistical information about the value of this velocity. As we shall see, neither of these features of classical physics apply with the same degree of generality in QM.

3) In the equation given, E and U are energy terms, m is the mass of the particle, h is Planck's constant, and ∇^2 represents the partial derivative $\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2}$

4) Strictly speaking, ψ is a complex function containing imaginary components. Probabilities are related to the square of the modulus of ψ, i.e. $|\psi|^2$.

5) For two interesting (but conflicting) accounts of Schrödinger's reaction to Born's interpretation, see E. MacKinnon, 'The Rise and Fall of the Schrödinger Interpretation' and L.Wessels, 'The Intellectual Sources of Schrödinger's Interpretations', in P. Suppes (ed), Studies in the Foundations of Quantum Mechanics (East Lansing, Michigan: Philosophy of Science Association, 1980).

6) In a bibliographic review of the interpretation of QM, DeWitt and Graham write:
"To read the vast literature on the interpretation of QM is a chore relieved by humour or excitement only rarely. What begins as an interesting venture in self-education gradually degenerates into a dreary routine. The major issues are so few and the words are so many that by the time one has covered the same old ground for the nth time it all begins to sound dreadfully dull."
B.S.DeWitt and N.R.Graham, 'Resource Letter IQM-1 on the Interpretation of Quantum Mechanics', American Journal of Physics, Vol. 39 (1971), 724-38, quote at 726. However, other authors such as Jammer do not perceive this repetition.

7) For example, let us suppose we wish to describe the orientation of a rotating wheel by specifying the angle θ between a particular axis (through the wheel's centre) and a radius of the wheel which we have painted white. Ideally, if we know the wheel's angular velocity ω (rad/sec) and the initial angle is zero, then $\theta = \omega t \times 360^\circ/2\pi$. However, any error in ω, no matter how small, means that, after a large enough time has passed, the error in θ will exceed 360° so that we would have no idea of the value of θ. Thus it is not true, even in classical

physics, that infinitesimal errors are always negligible.

8)Schrödinger's cat appears frequently in the literature on the interpretation of QM. It is interesting to note, however, that a wide range of different methods of killing the cat have been described. Whether this reflects authors' lack of German (the language in which Schrödinger's original discussion was written), or their desire to improve the technical sophistication of the device, or some other more Freudian motive, is unclear.

For example, Jammer (op. cit. note 1) opts for electrocution of the cat; D'Espagnat (ibid.) uses a 2-slit diffraction system instead of a radioactive substance, and uses 'poison' to kill the cat; J.M.Jauch, in his book Are Quanta Real (Bloomington, Indiana: Indiana University Press, 1973) uses 'prussic acid' instead of cyanide, and H.S.Green, in his book Matrix Mechanics (Groningen: Noordhoff, 1965) uses a half-silvered mirror and a photon. Detection of the photon operates a relay which fires a loaded gun at the cat. Evidently, the unfortunate cat has entered the folklore of physics, and embellishment of the original tale is quite permissible.

9)Jammer, op. cit. note 1, 474.

10)In AC theory, $\sqrt{-1}$ is usually denoted by j.

11)See, for example, L.E.Ballentine, 'The Statistical Interpretation of Quantum Mechanics', Reviews of Modern Physics Vol. 42 (1970), 358-81.

Ballentine is one of the few Canadian physicists to have written on the interpretation of QM. Although this paper on the Statistical Interpretation is his major contribution to the field, he has also published a number of papers, including a critique of the 'many-worlds' interpretation and a review of Belinfante's 1973 book on hidden variables.

Einstein's views, like those of Bohr, have been called to the defence of many interpretations. However, his sympathy for the statistical interpretation (though not necessarily for a hidden-variable theory) seem clear from his 'Reply to Criticisms' in the Festschrift Albert Einstein:Philosopher-Scientist, edited by P.A.Schilpp (New York: Harper and Row, 1959), 663-88.

12)A.Daneri, A.Loinger and G.M.Prosperi, 'Quantum Theory of Measurement and Ergodicity Conditions', Nuclear Physics, Vol. 33 (1962), 297-319; and 'Further Remarks on the Relations Between Statistical Mechanics and Quantum Theory of Measurement', Nuovo Cimento Vol. 44B (1966), 119-28.

Based in Milan, these theorists had been involved with an analysis of QM for some years prior to the publication of their measurement theory. However, apart from these two papers, plus some short replies to critics, they have not been actively involved in debates over the interpretation of QM.

13)J.M.Jauch, E.P.Wigner and M.M.Yanase, 'Some Comments Concerning Measurements in Quantum Mechanics', Nuovo Cimento Vol. 48B (1967) 144-51.

14)Daneri et al., (1966) op. cit. note 12, 127.

15)Quoted by Jammer, op. cit. note 1, 493.

Bub studied in London in Bohm's department, and completed a PhD, supervised by Bohm, in 1966. His thesis topic was the measurement theory now known as the Bohm-Bub theory. Bub later published several other papers on hidden variables, but early in the 1970's he abandoned this approach in favour of 'quantum logic'. He is now highly critical of hidden-variable theories. Bub continues to publish frequently on the interpretation of QM.

16)D.Bohm and J.Bub, 'A Proposed Solution of the Measurement Problem in Quantum Mechanics by a Hidden Variable Theory', Reviews of Modern Physics, Vol.38 (1966), 453-69.

Bohm is one of the few 'grand old men' in this field. His early work on HVTs in the 1950's was highly influential. Now a professor at Birkbeck College, London, Bohm has continued to publish prolifically on QM, though he does not now accept the Bohm-Bub theory.

P.Pearle,'Reduction of the State Vector by a Nonlinear Schrödinger Equation', Physical Review Vol. 13D (1976) 857-68.

Pearle began to write about nonlinear amendments to the Schrödinger equation in the late 1960's, when he was a student at Harvard. Although not a major figure on this field, he has published several papers, developing his non-linear theory and commenting on local HVTs.

17)Jammer, op.cit. note 1, 474-86.

Wigner, a Nobel Prizewinner, was deeply involved in the development of QM and nuclear physics from the 1930's onwards. His interest in interpretations of QM dates from the early 1960's.

18)Quoted by Jammer, op.cit. note 1, 484.

19)A.A.Ross-Bonney, 'Does God Play Dice?', Nuovo Cimento Vol 30B (1975), 55-79, quote at 68.

20)For a compilation of most of the major papers on this topic, see B.S.DeWitt and N.Graham (eds), The Many-Worlds Interpretation of Quantum Mechanics (Princeton: Princeton University Press, 1973). DeWitt normally refers to this theory as 'EWG' (Everett-Wheeler-Graham). He is perhaps being too modest: Wheeler's only contribution was to supervise Everett's research and write a very short paper commenting favourably on the theory. DeWitt supervised Graham's work on this topic and was almost entirely responsible for publicising this theory. Everett informed me that he had no involvement in the 'resurgence' of his theory, nor did he play any part in compiling the book cited above. He told me:

"I certainly approve of the way Bryce DeWitt presented my theory, since without his efforts it would never have been presented at all".

(Letter from Hugh Everett III to the author, 20th June, 1977.)

21)One difficulty with MWI is that when we measure a continuous variable (such as position), a literally infinite number of outcomes are possible, which, according to MWI, leads to an infinity of universes.

22)B.S.DeWitt, 'Quantum Mechanics and Reality', Physics Today Vol 23 (September 1970) 30-35, quote at 33.

23)Jammer, op.cit. note 1, 521.

24)A more accurate, though less well-known title, is the Indeterminacy Principle.

25)Such an experiment is at present quite impractical because, unlike glass (for visible light) and electric and magnetic fields (for charged particles like electrons) there is no way to refract a beam of gamma rays to any great extent.

26)The diagram is simplified, in that the intensity of the bright bands is shown to be constant across the screen. In fact, because of diffraction effects, the central bands are brighter than those on either side.

27)D.Bohm, 'A Suggested Interpretation of the Quantum Theory in terms of "Hidden Variables", Part 1', Physical Review Vol 85 (1952), 166-79, quote at 173.

28)Although this statement is generally true, there are some measurements which do not disturb a system and thus do not interfere with the causal evolution of ψ . For example, if a second observation of some particle is carried out soon after a first observation, and the value of the relevant observable has not changed during the intervening time, then the second measurement need not disturb ψ. As a second example, information can be gained from negative results, in which, say, a particle is found not to be in a certain location, without the particle being disturbed. Clearly, these examples are special cases.

29)Jammer, op.cit. note 1, 89-90.

30)See note 27 and D.Bohm, 'A Suggested Interpretation of the Quantum Theory in terms of "Hidden Variables", Part II', Physical Review Vol 85 (1952), 180-93.

31)D.Bohm, op.cit. note 27, 174.

32)This was no coincidence: Bohm set out deliberately to show that a HVT could reproduce all the predictions of QM.

33)For details of these fascinating debates, see Schilpp (ed), op.cit. note 11; for a summarised account, see Jammer, op.cit. note 1, 127-36.

34)A.Einstein, B.Podolsky and N.Rosen, 'Can Quantum-Mechanical Description of Physical Reality be Considered Complete?' Physical Review Vol 47 (1935), 777-80.

35)D.Bohm, Quantum Theory (Englewood Cliffs, NJ: Prentice-Hall, 1951).

36)In QM, as in classical physics, spin refers to a particles's angular momentum which is not due to orbital motion. However, in QM spin is quantised so that, for example, an electron can have one of only two possible values, $+\frac{1}{2}$ and $-\frac{1}{2}$.

37)One apparently obvious objection to this theory is that it seems to clash with relativity theory by invoking speeds greater than the velocity of light. This need not mean that HVT is wrong, merely that it is not consistent with relativity. Since the process involved is novel, such a conclusion is defensible. See B.D'Espagnet, op.cit. note 1, pp 90 and 238.

38)D.Bohm and Y.Aharonov, 'Discussion of Experimental Proof for the Paradox of Einstein, Rosen and Podolsky', *Physical Review* Vol 108 (1957), 1070-6; D.Bohm, 'Hidden Variables in the Quantum Theory' in D.R.Bates (ed) *Quantum Theory* Vol. 3 (London: Academic Press, 1962).

39)John Bell, a theorist based at CERN in Geneva, has had a major impact on this field, mainly through two papers written in 1964. One dealt with 'proofs' that HVTs could not be consistent with QM, and the other raised the issue of local HVTs which would satisfy EPR's demands. Bell has also published a number of other papers in this field.

40)See, for example, J.F.Clauser and A.Shimony, 'Bell's Theorem: Experimental Tests and Implications', *Reports on Progress in Physics* Vol 41 (1978), 1881-927, especially p 1922.

41)The exclusion principle states that in any system containing certain particles such as electrons, no two particles can be in the same quantum state. However, such particles can be spatially separated, so that one particle 'knows' the state of another, distant, particle and alters its behaviour accordingly.

42)H.P.Stapp, 'S-Matrix Interpretation of Quantum Theory', *Physical Review* Vol D3, (1971), 1303-20, quote at 1316.

43)This does not mean that measurement of one photon *causes* a *physical* change in the second photon. Rather, the choice of apparatus used to observe the first photon determines the particular equation which is relevant for describing the pair of photons and therefore for describing the second photon.

44)J.S.Bell, 'On the Einstein-Podolsky-Rosen Paradox', *Physics* Vol 1(1964), 195-200.

45)For details, see Jammer, op.cit. note 1, 306-12, and Clauser and Shimony, op.cit. note 40, 1892-1900.

46)For example, in the derivation used by Clauser and Horne (see Clauser and Shimony, op.cit. note 40, 1896),

$$\lambda = \frac{R(a,b) - R(a,b') + R(a',b) + R(a',b')}{r_1(a') + r_2(b)} - 1$$

and $\lambda \leq 0$ for LHV.
Here a, a' and b, b' represent different pairs of polarization analyser settings, R(a,b) is the coincidence rate at settings a and b for the pair of polarizers, and $r_1(a')$ is the rate of single particle detections of polarizer 1 at setting a'.

47)Papaliolios is an experimental physicist at Harvard University. He had no active involvement in this field before he did his test of the Bohm-Bub theory. As we shall see in Chapter Four, this test did not require a great deal of time or expense. Since performing his experiment, Papaliolios has taken an active interest in other experimental tests of QM.

48)Bohm and Bub, op.cit. note 16, 466.

49)QM predicts that the intensity of the transmitted light varies with the square of the cosine of the angle between the axes of B and C.

50)J.Hall, C.Kim, B.McElroy and A.Shimony, 'Wave-Packet Reduction as a Medium of Communication', Foundations of Physics, Vol 7 (1977), 759-67.

Shimony, who holds a joint professorship of physics and philosophy at Boston University, was deeply involved in organising the experimental tests of LHVT. He has written widely in the philosophy of science as well as on the interpretation of QM.

51)For details of the individual experiments, see Clauser and Shimony, op.cit. note 40. Biographical information on the individuals involved will be provided, as required, later in this thesis.

52)For example, if we consider the linear polarization of two photons emitted simultaneously in the annihilation of positronium (an electron-positron pair) in its singlet state, the coincidence rate is proportional to $1 - (\cos\theta)^2$, where θ represents the angle between the axes of the polarization analysers. These photons are emitted with perpendicular polarizations. Photon pairs emitted in some optical cascades have parallel polarizations, so here the correlation rate varies with $(\cos\theta)^2$.

53)An atom is excited either thermally, or by laser, or by collision with ions in a gas discharge tube. One of the atom's electrons jumps to a higher energy level, and soon returns to its original level either directly or via one or more intermediate levels. Each transition to a lower level is accompanied by the emission of a photon carrying off the excess energy. When several photons are emitted (as when the electron travels via intermediate levels), a 'cascade' is said to occur.

54)The positron is the anti-particle of the electron. When a particle meets its anti-particle, mutual annihilation occurs, that is, the mass of both particles is converted to energy according to the famous equation $E = mc^2$. To conserve momentum, spin, and so on, the energy is emitted as two high-energy photons (gamma rays) travelling in opposite directions.

Footnotes to Chapter Three

1)P.Forman, 'Weimar Culture, Causality and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment' in R.McCormack (ed), *Historical Studies in the Physical Sciences, No. 3* (Philadelphia: University of Pennsylvania Press, 1971).

2)L.Graham, *Science and Philosophy in the Soviet Union* (London: Allen Lane, 1973).

3)For example, D.Bloor, in *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976) describes Forman's study as "fascinating and controversial" (p.4); and B.Barnes, in *Interests and the Growth of Knowledge* (London: Routledge and Kegan Paul, 1977) concludes a list of empirical studies with "....and, more speculatively, Forman" (p. 95).

4)This is particularly so because Forman examines modern *physics*, which is traditionally perceived as the 'hardest' of the sciences and one which is especially impervious to social influences.

5)The term 'new' is conventionally used to distinguish the theory developed in the 1920's from the 'old' quantum theory proposed by Bohr in 1913. The latter was empirically accurate, but lacked a coherent structure and a theoretical justification.

6)Forman, op.cit. note 1, 88-91.

7)ibid., 87-8.

8)ibid., 84.

9)ibid., 88.

10)ibid., 86.

11)ibid., 86.

12)ibid., 91.

13)ibid., 7, emphasis added.

14)ibid., 6.

15)ibid., 59.

16)ibid., 58.

17)ibid., 62-3, emphasis in original.

18)ibid., 99.

19)ibid., 95.

20)ibid., 105.

21)P.Forman, 'The Reception of an Acausal Quantum Mechanics in Germany and Britain' in S.H.Mauskopf (ed) *The Reception of Unconventional Science* (Washington: American Association for the Advancement of Science, 1978), 1-50.

22)Forman, op.cit. note 1, 7-8.

23)ibid., 109-10, emphasis added.

24)Forman, op.cit. note 21, 23.

25)Both quotations from Forman, op.cit. note 21, 38; emphasis added in each case.

26)John Hendry, 'Weimar Culture and Quantum Causality' in C.Chant and J.Fauvel (eds) Darwin to Einstein: Historical Studies on Science and Belief (Harlow: Longman, 1980), 303-26, quote at 316.

27)Forman, op.cit. note 21, 23.

28)Hendry, op.cit. note 26, 316.

29)D.Bohm, 'A Suggested Interpretation of the Quantum Theory in Terms of "Hidden Variables", Part I', Physical Review Vol 85 (1952), 166-79; 'Part II', ibid, 180-93.

30)M.Jammer, The Philosophy of Quantum Mechanics (New York: Wiley, 1974), 110.

31)Hendry, op.cit. note 26, 314.

32)ibid., 314.

33)It is interesting to note that Spengler supported such a radical relativism, writing:
"In the historian's view, there is only a history of physics. All its systems now appear to him as neither correct nor incorrect, but as historically, psychologically conditioned by the character of the epoch, and representing that climate more or less completely" (quoted by Forman, op.cit. note 1, 32).
Schrödinger also acknowledged, and welcomed, a close correlation between scientific theories and the 'fashion of the times' or 'temper of the age'. See E.Schrödinger, Science, Theory and Man (New York: Dover, 1957). This is rather ironic, since Schrödinger's own interpretation of QM was not well received in the cultural climate in which it was produced.

34)D.Joravsky, The Lysenko Affair (Cambridge, Mass.: Harvard University Press, 1970).

35)D.Joravsky, Soviet Marxism and Natural Science 1917-32 (London: Routledge and Kegan Paul, 1961); R.Bentley, The Relationship between Dialectical Materialism and Soviet Quantum Mechanics (unpublished M.Sc. thesis, University of Sussex, 1972); L.R.Graham, op.cit. note 2.

36)Bohm and Oppenheimer are two good examples of Western scientists who became politically suspect during the McCarthy period. See P.M.Stern, The Oppenheimer Case (New York: Harper and Row, 1969); for details of Bohm's case, see S.G.Brush, 'The Chimerical Cat: Philosophy of Quantum Mechanics in Historical Perspective' Social Studies of Science Vol 10 (1980) 393-447, footnote 109.

37)Graham, op.cit. note 2, 71.

38)Jammer, op.cit. note 30, 248.

39)Brush, op.cit. note 36, 423.

40)For details, see Bentley, op.cit. note 35, 32-6.

41)Quoted by Joravsky, op.cit. note 35, 246.

42)ibid., 246.

43)Quoted by Bentley, op.cit. note 35, 54.

44)For details of the 'Markov affair', see Graham, op.cit. note 2, 75-81 and Jammer, op.cit. note 30, 248-9.

45)Quoted by Bentley, op.cit. note 35, 57.

46)ibid., 58.

47)M.Born, Physics in My Generation (New York: Springer, 1969), 36.

48)W.Heisenberg, Physics and Philosophy (New York: Harper, 1958), 203.

49)Quoted by Graham, op.cit. note 2, 96.

50)ibid., 97.

51)ibid., 97.

52)For example, Bentley argues that Bohr did not change his position. See Bentley, op.cit. note 35, 77-9.

53)ibid., 83.

54)ibid., 86.

55)Graham, op.cit. note 2, 81-93 (Blokhintsev) and 101-7 (Omelianovskii).

56)Bentley, op.cit. note 35, 90.

57)Graham, op.cit.note 2, 87-8.

58)ibid, 88 and 100.

59)For example, Jauch and Piron, writing about the Bohm-Bub hidden-variable theory, claim that
"it is contrary to good scientific methodology to modify a generally verified scientific theory for the sole purpose of accomodating hidden variables". See J.M.Jauch and C.Piron, 'Hidden Variables Revisited', Reviews of Modern Physics Vol 40 (1968) 228-9, quote at 228. As we shall see in Chapter Four, Bohm has a very different idea of what constitutes 'good scientific methodology'. In Chapter Six, I shall examine the use of the concept of 'reasonableness' in the evaluation of experimental tests of local hidden variable theories.

60)An English translation of this book was published in 1968 as D.I.Blokhintsev, The Philosophy of Quantum Mechanics (Dordrecht: Reidel, 1968).

61)Graham, op.cit. note 2, 91.

62)Bentley, op.cit. note 35, 94.

63)See Chapter One of A.D.Aleksandrov, A.N.Kolmogorov, and M.A. Lawrent'ev (eds) Mathematics: Its Content, Methods, Meaning (English translation by American Mathematical Society; Cambridge, Mass.: MIT Press, 1969). Most of the arguments in favour of Marxism were omitted from the first edition of this translation. See Science and Nature Vol 1 (1980), 40.

64)Graham, op.cit. note 2, 434-5.

65)It should be noted that Graham arrived at the conclusions quoted here fairly recently. In an earlier study, 'Quantum Mechanics and Dialectical Materialism', Slavic Review Vol 25 (1966),

381-410, Graham wrote
"The laws of the dialectic play no role in the minds of Soviet physicists except very rarely, perhaps, as a reminder that 'matter and its laws are more complicated than a simple materialist might think'." (p.418)

In reply to this claim, Fock produced a spirited defence of DM; this evidence impressed Graham, and was at least partly responsible for his change in position. Even now, though, Graham maintains that while DM may provide a general interpretive framework, it does not constitute a comprehensive scientific methodology:
"DM could not help a scientist with the details of laboratory work. It would never predict the result of a specific experiment." (Graham, op.cit. note 2, 439).

66)This problem was discussed in detail in Chapter One.

67)Graham, op.cit. note 2, 107.

68)Of course, the sanctions available to Western supporters of Copenhagen to help them enforce their views, were undoubtedly milder.

69)M.Born, op.cit. note 47, 171.

70)ibid., 172.

71)ibid., 172.

72)Quoted by Graham, op.cit. note 2, 102.

73)A.Garuccio and F.Selleri, 'Quantum Mechanics and Society' (unpublished mimeo, Istituto di Fisica, Universita di Bari, 1977). These authors take a very strong interpretation of Forman's work, arguing that an acausal QM was largely a deliberate response to the Weimar context, and that
"there seems therefore to be a strong connection between QM and the birth of the fascist states". (p. 15)

It is rather ironic that Forman's paper, which is a study of QM in its sociopolitical context, is itself being used as a resource in political debates!

74)ibid., 15.

75)T.Bergstein, Quantum Physics and Ordinary Language (London: MacMillan, 1972), quote at xi. Jammer also points to parallels between complementarity and a number of earlier belief systems, including the Sophists (5th century BC) and the thoughts of the 12th century Arab philosopher Ibn-Rushd. See Jammer, op.cit. note 30, 104-7 and 197-211.

76)L.Rosenfeld, 'Strife about Complementarity', Science Progress Vol 61 (1953), 393-410, quote at 394.

77)ibid., 407-8.

78)In Chapter Four, we shall see that debates about what Bohr (and even more commonly, Einstein) 'really thought' are common among those who study the interpretation of QM.

79)Rosenfeld, op.cit. note 76, 408.

80)Rosenfeld, in S.Rosenthal (ed), Niels Bohr (Amsterdam: North Holland, 1967).

81)N.Bohr, Atomic Physics and Human Knowledge (New York: Wiley, 1958), quote at 10.

82)Quoted by Jammer, op.cit. note 30, 88.

83)Bohr, op.cit. note 81, 11.

84)M.Born, Atomic Physics (London: Blackie, 1961), quote at 323.

Other QM theorists made similar points. For example, Heisenberg wrote:
"One of the arguments frequently used against vitalism is that there seems definitely to be no place at which some 'vital force' different from the forces in physics could enter. On the other hand, it is just this argument that has lost much of its weight through quantum theory". (op.cit. note 48, 103.)
For other examples, see Jammer, op.cit. note 30, 88.

85)Garuccio and Selleri, op.cit. note 73, 25.

86)J.M.Levy-Leblond, 'Ideology of/in Contemporary Physics', in H.Rose and S.Rose, The Radicalisation of Science (London: MacMillan, 1976), quote at 158-9, emphasis in original.

87)Rosenfeld, op.cit. note 76, 407-8.

88)See E.P.Wigner, 'Remarks on the Mind-Body Question' in E.P. Wigner, Symmetries and Reflections (Bloomington, Indiana: Indiana University Press, 1967), 171-84; and E.P.Wigner, 'Are We Machines?', Proceedings of the American Philosophical Society Vol 113 (1969), 95-101.

89)J.Monod, Chance and Necessity (Glasgow: Fontana, 1974), quote at 110.

90)ibid., 112.

91)ibid., 112.

92)ibid., 112.

93)Levy-Leblond, op.cit. note 86, 159.

94)Other examples include Bergstein (op.cit. note 75), who applies complementarity to linguistics, and authors quoted by Jammer (op.cit. note 30, 88-9) who have applied it in fields as diverse as anthropology, Kantian philosophy and theology.

95)C.W.Rietdijk, On Waves, Particles and Hidden Variables (Assen: Van Gorcum, 1971), quote at 130.

96)F.J.Belinfante, A Survey of Hidden-Variable Theories (Oxford: Pergamon, 1973), quote at 18.

97)D.M.Mackay, The Clockwork Image: a Christian Perspective on Science (London: Inter-Varsity Press, 1974), quote at 15.

98)ibid., 15.

99)Rosenfeld, op.cit. note 76, 399.

100)Mackay, op.cit. note 97, 107, emphasis in original.

101)Ian G.Barbour, Issues in Science and Religion (New York: Harper Torchbooks, 1966.See especially pp. 298-314.

102)C.E.Rosenberg, 'Scientific Theories and Social Thought', in S.B.Barnes (ed) Sociology of Science (Harmondsworth: Penguin, 1972), 292-305, quote at 301 and 303.

103)See, for example, I.Cameron and D.Edge, Aspects of Scientism (Siscon report, Dept. of Liberal Studies in Science, Manchester University, 1975).

104)Fritjof Capra has suggested that there are much broader links between modern physics and Eastern philosophy. See F.Capra, The Tao of Physics (London: Fontana, 1975). For a critical discussion of such attempts at 'parallelism', see Sal P.Restivo, 'Parallels and Paradoxes in Modern Physics and Eastern Mysticism: I - A Critical Reconnaissance', Social Studies of Science Vol 8 (1978), 143-81.

105)See, for example, C.Castaneda, The Teachings of don Juan, A Yaqui Way of Knowledge (Berkeley, Calif.: University of California Press, 1968).

106)S.Holroyd, Psi and the Consciousness Explosion (London: Bodley Head, 1977), quote at 161.

107)B.S.DeWitt, 'The Many-Universes Interpretation of Quantum Mechanics' in Societa Italiana di Fisica (B.D'Espagnat, ed), Foundations of Quantum Mechanics, Proceedings of the Enrico Fermi International Summer School, Course 49 (New York: Academic Press, 1971), 211-54, quote at 213 and 226.

The Many-Worlds interpretation is an obvious candidate for science-fiction stories, and DeWitt is cited in G.Zebrowski, 'The Cliometricon', in G.Zebrowski, The Monadic Universe (New York: Ace Books, 1977). For a more serious, yet still speculative discussion of this interpretation, see P.Davies, Other Worlds: Space, Superspace and the Quantum Universe (London: Dent, 1980).

108)J.Sarfatti, 'Towards a Quantum Theory of Consciousness, the Miraculous and God' (unpublished mimeo, Physics/Consciousness Research Group, San Francisco, 1977). In another document entitled 'Help! I need some $', circulated at the same time, Sarfatti describes the above paper as 'an academic outrage'.

109)D.Bohm and B.J.Hiley, 'Some Remarks on Sarfatti's Proposed Connection between Quantum Phenomena and the Volitional Activity of the Observer-Participator', Psychoenergetic Systems Vol 1 (1976), 173-8, quote at 173.

110)Letter from Stapp to Sarfatti, 30th October 1978. Sarfatti circulated this letter, and his reply, to the people on his mailing list. His reply does not refute Stapp's criticism, but instead appeals for (financial) support in setting up an experiment to see who is right. As Sarfatti puts it: "Only proper homage to Our Lady, Dame Nature, will tell us who is Champion in this Quest for the Holy Grail of Quantum Enlightenment" (letter from Sarfatti to Stapp, 14th November 1978).

111)Quoted in 'A Modest Proposal to the Foundation for the Realization of Man' (unpublished mimeo, Physics/Consciousness Research Group, San Francisco, 11th February 1976), Appendix II, p. 15. As discussed in Chapter Two, the long-range correlations which QM predicts can indeed persist over long distances, but these correlations break down once the correlated subsystems are observed, in other words once they undergo interactions.

112)Of course, by no means all parapsychologists would support either Sarfatti's arguments or his tactics. Collins and Pinch, in their study of experimental parapsychologists, found that, in an effort to gain recognition for their work from orthodox scientists, parapsychologists kept a tight rein on speculations which were not supported by empirical evidence. Sarfatti's audience is, by and large, not scientists but middle-class Californian laymen; this may account, at least in part, for his peculiar style of argument. See H.M.Collins and T.J.Pinch, 'The Construction of the Paranormal: Nothing Unscientific is Happening' in R.Wallis (ed) On the Margins of Science: The Social Construction of Rejected Knowledge (Keele: Sociological Review Monograph No. 27, 1979), 237-70.

113)J.Sarfatti, Letter to the Editor, Psychoenergetic Systems Vol 2 (1976), 1-8, quote at 2.

114)Bob Toben, Space-Time and Beyond (New York: Dutton, 1975), quote at 11.

115)ibid., 134.

116)H.A.Bedau, 'Complementarity and the Relation between Science and Religion', Zygon Vol 9 (1974), 202-24, quote at 207.

117)Quoted by Jammer, op.cit. note 30, 89.

118)ibid., 90.

119)Barbour, op.cit. note 101, 293.

120)ibid., 293.

121)ibid, 292-3, emphasis in original.

122)This view can conveniently be summarized by Wittgenstein's dictum that 'the meaning is the use'.

Footnotes to Chapter Four

1)As discussed in Chapter Two, I shall use the term 'Foundations of QM' (FQM) to refer to the various attempts to reinterpret or modify QM, as distinct from more conventional study of QM, such as applying the accepted formalism of QM in new contexts.

2)The concept of a specialty has been widely discussed in the sociology of science, and detailed references to the appropriate literature are provided in Chapter One. Obviously, the claim that FQM is atypical requires at least an implicit definition of what constitutes a typical specialty. Rather than attempt a detailed critique of the literature, I shall confine myself to a few features which, according to nearly all authors, are characteristic of scientific specialties. These include: a strong degree of cognitive consensus, an organised social structure, with a well-developed communication system (formal and informal), and a training system for introducing neophytes into the specialty. By showing that FQM has none of these features, I hope to avoid the need for specifying any particular definition of a specialty.

3)T.J.Pinch, 'What Does a Proof Do if it Does Not Prove? A Study of the Social Conditions and Metaphysical Divisions Leading to David Bohm and John von Neumann Failing to Communicate in Quantum Physics' in E.Mendelsohn, P.Weingart and R.Whitley (eds) The Social Production of Scientific Knowledge, Sociology of the Sciences Yearbook, Vol. 1 (Dordrecht: Reidel, 1977), 171-215.

4)There are some journals which regularly publish papers on aspects of FQM, notably Foundations of Physics. However, such journals also publish papers on other aspects of 'fundamental' physics, such as cosmology. In addition, for each journal publishing FQM papers, there was a substantial number of FQM workers who told me that this particular journal was not appropriate for their work and was unrepresentative of FQM as a whole.

5)W.O.Hagstrom, The Scientific Community (Carbondale, Ill.: Southern Illinois University Press, 1975), 277.

6)N.R.Hanson, The Concept of the Positron (London:Cambridge University Press, 1963), 99.

7)As discussed in Chapter One, excerpts from interview transcripts will not normally be attributed to any particular individual.

8)See Chapter One for details of the questionnaire.

9)For example, Shimony was a student of Wigner, yet Shimony has publicly criticised Wigner's 'consciousness interpretation'. Although the 'many-worlds' interpretation of QM is sometimes referred to as the 'Everett-Wheeler' interpretation, Everett developed this model independently of his supervisor Wheeler.

These examples are typical. One reason for the lack of continuity between 'generations' in FQM is that there are virtually no institutions which regularly offer undergraduate courses in FQM, and few institutions which have a long-term research programme in FQM. Thus although a student's supervisor may be actively involved in FQM, this is not likely to be the area in which the student is being trained, nor is it likely to be the supervisor's main professional activity. Many people spoke of being introduced to FQM by their former supervisor, but the absence of organised collaboration or agreement between students and supervisors is quite understandable. Typically, once a student has been introduced to the main issues, he then develops his own approach.

10) There are also a number of journals or regular newsletters which have a restricted readership or are less 'public' than Foundations of Physics. For example, the Annals of the Louis de Broglie Foundation is sent to a wide range of locations but was seldom cited by respondents as a useful source of papers on their own area of FQM. The Epistemological Letters of the Institut de la Methode, in Switzerland, is issued free of charge to a fairly small group of people and is largely concerned with locality and the LHV experiments. Neither of these journals is listed in Science Abstracts or the Science Citation Index.

11) D.O.Edge, 'Quantitative Measures of Communication in Science: A Critical Review', History of Science Vol 17 (1979), 102-34.

12) See, for example, M.J.Moravcsik and P.Murugesan, 'Some Results on the Function and Quality of Citations', Social Studes of Science Vol 5(1975), 86-92; and G.N.Gilbert, 'Referencing as Persuasion', ibid. Vol 7 (1977), 113-22.

13) Foundations of Physics was first published in 1971.

14) See, for example, D.Crane, Invisible Colleges (Chicago: University of Chicago Press, 1972).

15) J.Bub, The Interpretation of Quantum Mechanics (Dordrecht: Reidel, 1974), quote at ix.

16) Interview with Basil Hiley, Birkbeck College, University of London, 5th July, 1977.

17) See note 16.

18) The 'double-solution' theory, dating back to 1921, argues that there are two solutions to the wave equation: a continuous wave (ψ) and a singularity (particle).

19) L.de Broglie, G.Lochak, J.A.Beswick and J.Vassalo-Pereira, 'Present, Predicted and Hidden Probabilities', Foundations of Physics Vol 6 (1976), 3-14, quote at 4.

20) Publicity brochure for A.George et al. (eds), Louis de Broglie; Sa Conception du Monde Physique (Paris: Gauthiers-Villars, 1973).

21) L.de Broglie, 'Basic Principles of Wave Mechanics', Comptes Rendus Vol B277 (16th July 1973) 71-3, quote at 73.

22) I obtained a photocopy of this published review from the author. The review was published in Recherche in 1974; I have been unable to locate it more precisely. (My translation)

22a) The journal's stated policy is as follows:
"The Annales are a review of theoretical physics designed, in particular, for the publication of studies put forward in the

Foundation's Seminar and devoted, more generally, to the diffusion of work on wave mechanics within the perspective of the de Broglie school [l'ecole broglienne], as well as research carried out in other directions which contribute to the advancement of fundamental knowledge of microphysics."
(This policy statement is printed on the inside back cover of each issue of the Annales.) (My translation.)

23)Bohm and de Broglie hold rather different views though there is a degree of overlap. See, for example, D.Bohm, 'On the Creation of a Deeper Insight into What May Underlie Quantum Physical Law', in M.Flato et al. (eds) Quantum Mechanics, Determinism, Causality and Particles (Dordrecht: Reidel, 1976), 1-10.

24)I came across references to only two other groups where, according to commentators, a consistent position had been taken for a number of years. These groups were headed by von Weizsäcker and by Ludwig; both are in Germany, and publish mainly in German. I was unable to study these groups in detail, but none of my respondents outside Germany claimed to be a member of either of these groups.

25)For example, Bub and Demopoulos at the University of Western Ontario, Komar and Finkelstein at Yeshiva University, New York, and Prugovecki and van Fraassen at Toronto University.

26)V.Augelli, A.Garuccio and F.Selleri, 'La Mécanique Quantique et la Réalité', Annales de la Fondation Louis de Broglie Vol 1 (1976), 154-73, quote at 154 (my translation).

27)B.S.DeWitt and N.R.Graham, 'Resource Letter IQM-1 on the Interpretation of Quantum Mechanics', American Journal of Physics Vol 39 (1971), 724-38, quote at 724.

28)A detailed account of the early history of LHV will be provided in Chapter Five.

29)B.S.DeWitt, 'Quantum Mechanics and Reality', Physics Today Vol 23 (September 1970), 30-35.

30)M.Sachs, 'An Alternative to Quantum Mechanics', Physics Today Vol 24 (April 1971), 39-41, quote at 40.

31)L.E.Ballentine, 'The Formalism is not the Intepretation', ibid., 36-38, quote at 38.

32)P.Pearle, 'Quantum Theory Fails the Single System', ibid., 38.

33)As we shall see in Chapter Five, the LHV activity has played an important part in highlighting this shortcoming of 'traditional' FQM.

34)DeWitt, op.cit. note 29, 35.

35)Sachs, op.cit. note 30, 41.

36)B.S.DeWitt, 'Replies to Critics', Physics Today Vol 24 (April 1971), 41-44, quote at 42.

37)See P.Pearle, 'Alternative to the Orthodox Interpretation of Quantum Theory', American Journal of Physics Vol 35 (1967), 742-53 and M.Sachs 'Comment on "Alternative to the Orthodox Interpretation of Quantum Theory', ibid., Vol 36 (1968) 463-4.

38)M.Jammer, in <u>The Philosophy of Quantum Mechanics</u> (New York: Wiley, 1974) p 254 criticises Bell for "erroneously" describing Einstien as a supporter of hidden variables. Bell continues to hold this view - J.S.Bell, 'Einstein Podolsky Rosen Experiments' (unpublished mimeo, CERN, Geneva, 1976).

39)For example, as pointed out in Chapter Three, there is a wide variety of opinions on what exactly Bohr meant by complementarity. See Jammer, op.cit. note 38, 86-92.

40)For example, compare the approaches in Chapter Seven with those of Chapter Eight of Jammer, op.cit. note 38.

41)N.Wiener and A.Siegel, 'A New Form of the Statistical Postulate of Quantum Mechancis', <u>Physical Review</u> Vol 91 (1953), 1551-60.

42)Interview with A.Siegel, Boston University, 14th October, 1977.

43)One indication of this is the fact that the Wiener-Siegel theory is never mentioned in Jammer's comprehensive review of FQM (op. cit. note 38).

44)See note 42.

45)DeWitt, op.cit. note 29, 33.

46)B.S.DeWitt, 'The Many-Universes Interpretation of Quantum Mechanics' in Societa Italiana di Fisica (B.D'Espagnat, ed.) <u>Foundations of Quantum Mechanics</u>, Proceedings of the Enrico Fermi International Summer School, Course 49 (New York: Academic Press, 1971), 211-54, quote at 212.

47)ibid., 211 and 222.

48)Allen, a physicist and a supporter of the 'many-worlds' interpretation, explicitly claims that experiments are irrelevant: "Whereas the test of a physical theory is experiment, the test of an interpretation of QM is logical consistency....of the distinct interpretations known to the author....only the Everett interpretation is free from inconsistency." R.E.Allen, 'Consistency of Language and Interpretations of Quantum Mechanics', (College Station, Texas: unpublished mimeo, Physics Department, Texas A&M University, 1977), quoted at 1 and 4.

49)Of course, a flair for public relations can also affect the reception given to a proposal. By his papers in technical and popular science journals, DeWitt provoked interest in Everett's ideas after they had lain virtually ignored for ten years. Citation data confirm DeWitt's role:

Year	1964	65	66	67	68	69	70	71	72	73	74	75	76
Citations of Everett	3	-	1	2	2	1	2	11	3	4	9	8	4
Co-citations of Everett and DeWitt	-	-	-	-	-	-	2	8	1	-	2	3	-

Thus DeWitt's publicising of Everett around 1970 was clearly effective, although it would seem that, once well-known, the original work is then cited independently of the popularising articles. (See also footnote 20, Chapter Two.)

50)See, for example, L.E.Ballentine, 'Can the Statistical Postulate of Quantum Theory Be Derived?- a Critique of the Many-Universes Interpretation', Foundations of Physics Vol 3 (1973), 229-40; and J.S.Bell, 'The Measurement Theory of Everett and De Broglie's Pilot Wave', in Flato et al., op.cit. note 23, 11-17.

51)Pinch, op.cit. note 3.

52)The lack of communication between Bohm and von Neumann himself is much more easily understood. Bohm produced his HV theory in 1952 and shortly afterwards left the US after difficulties with McCarthyites. He went to Brazil and was based there for a number of years. Von Neumann's proof was written in 1932, and he died in 1957.

53)Pinch, op.cit. note 3, 202. We have already seen in Chapter Three how a number of different glosses can be given to concepts such as 'causality' and 'dialectic'.

54)Pinch, op.cit. note 3, 205.

55)N.Georgescu-Roegen, The Entropy Law and the Economic Process (Cambridge, Mass.: Harvard University Press, 1971).

56)Bohm's theory is in fact highly mathematical. In addition, Bell was able to show, in a clear and unequivocal way, that Bohm's theory was incompatible with von Neumann's proof because it did not conform to von Neumann's axioms, which characterised von Neumann's conception of what a HV theory must look like.

57)Pinch, op.cit. note 3, 205-6.

58)Pinch (on p 207) cites Lakatos' findings that mathematicians are deferential towards abstract logical proofs, but then he extrapolates this finding to physicists with no empirical evidence other than the von Neumann - Bohm case itself. In addition, Lakatos' claim about mathematicians is by no means universally accepted. For example, Hagstrom (op.cit. note 5, 189) writes:"Mathematicians assert that logic is not important to them", and that logic's low prestige in mathematics derives from the fact that it is not generally helpful to mathematicians.

59)Pinch, op.cit. note 3, 208.

60)D.Bohm, 'A Suggested Interpretation of the Quantum Theory in Terms of "Hidden Variables", Part I', Physical Review Vol 85 (1952), 166-79, quote at 174.

61)Pinch, op.cit. note 3, 193.

62)These criticisms are all quoted by Pinch, op.cit. note 3, 182.

63)Bohm, op.cit. note 60, 171 and 179.

64)The journal Nature (Vol 190 (1961), 308) published a short biography of Bohm when he was made a professor at Birkbeck College. Bohm's reputation as a physicist was acknowledged, but

was distinguished from his work on FQM, as the following extract shows:
"[he is] well-known among physicists as the originator of the Bohm-Pines theory of collective motion in a plasma.... and well-known to the public for his views on the philosophy of science and his criticisms of the Copenhagen Interpretation."

65)This general argument is part of the theoretical framework described in detail in Chapter One of this thesis.

66)J.S.Bell, 'On the Problem of Hidden Variables in Quantum Mechanics', Reviews of Modern Physics Vol 38 (1966) 447-52, quote at 447.

67)An illustration of the physics élite's attitude to von Neumann's work (of which this proof was only a very small part) is provided in a letter written by Sir Edward Appleton to Professor W. Wilson of Bedford College, London, on 24th September 1945:
"I bought a copy of von Neumann, as you advised years ago, but have had no time to get beyond the first few pages. But it is all very elegant. That I can see."
(Wilson correspondence, Bedford College Library.)

68)F.J.Belinfante, A Survey of Hidden Variable Theories (Oxford: Pergamon Press, 1973), 34.

69)ibid., 24.

70)ibid., 34.

71)Despite the "obviousness" of the error in von Neumann's proof, physicists who are not wholly familiar with recent FQM literature still cite the proof as if it were accepted knowledge. For example, J.Mehra, in his book The Quantum Principle: Its Interpretation and Epistemology (Dordrecht: Reidel, 1974), uses the proof to exclude Bohm's (1952) HV theory. Then in a footnote (on p 64) he includes a letter written to him by Bell, pointing out that "this conclusion may be incorrect". We can of course conclude from this that Mehra is loyally defending the 'arithmetic ideal', but it seems more reasonable to explain this as a result of his lack of familiarity with the literature of a difficult and esoteric subject.

72)B.Wynne, 'C.G.Barkla and the J Phenomenon: A Case-Study in the Treatment of Deviance in Physics', Social Studies of Science Vol 6 (1976), 307-47.

73)Wynne accounts for these different approaches by referring to a more general cultural differentiation which took place in British physics after 1900. My analysis of Bohm's rejection has not invoked such general cultural changes, although an interesting historical treatment of FQM along these lines has been attempted by Brush. See S.G.Brush, 'The Chimerical Cat: Philosophy of Quantum Mechanics in Historical Perspective', Social Studies of Science, Vol 10 (1980) 393-447.

74)D.Bohm and J.Bub, 'A Proposed Solution of the Measurement Problem in Quantum Mechanics by a Hidden Variable Theory', Review of Modern Physics Vol 38 (1966), 453-69. This estimate is given on p 466.

75)C.Papaliolios, 'Experimental Test of a Hidden-Variable Quantum Theory', Physical Review Letters Vol 18 (1967), 622-5. The equation is quoted on p 622.

76)S.J.Freedman, R.A.Holt and C.Papaliolios, 'Experimental Status of Hidden Variable Theories', in M.Flato et al (eds), op.cit. note 23, 43-59.

77)Interview with C.Papaliolios, University of Arizona, Tucson, 7th November, 1977. (Papaliolios normally works at Harvard but was on temporary secondment to Arizona at the time of this interview.)

78)See note 77.

79)See note 77.

80)J.Bub, 'Hidden Variables and the Copenhagen Interpretation: A Reconciliation', British Journal for the Philosophy of Science, Vol 19 (1968), 185-210, quote at 205.

81)Freedman et al., op.cit. note 76.

82)Papaliolios, op.cit. note 75, 624.

83)See note 77.

84)Freedman et al., op.cit note 76, 47.

85)ibid., 47.

86)See note 77. Since Bohm agrees with Papaliolios' description of the origin of this equation, there seems no objection to quoting such apparently critical statements.

87)See note 77.

88)See note 77.

89)R.Wangsness, 'Hidden Variables and Magnetic Relaxation', Physical Review Vol 160 (1967), 1190-2. In a note added in proofs, Wangsness draws attention to Papaliolios' results.

90)See note 77.

91)Bohm and Bub, op.cit. note 74, 454.

92)ibid., 454.

93)ibid., 457, emphasis in original.

94)References to language crop up repeatedly in their paper. See, for example, pages 452, 457, 458, 468 and 469.

95)Bohm and Bub, op.cit. note 74, 469. This view of the role of language in structuring thought is remarkably similar to the 'Sapir-Whorf' hypothesis, which has recently come under strong criticism from psycholinguists. See, for example, I.D.Currie, 'The Sapir-Whorf Hypothesis', in J.E.Curtis and J.W.Petras (eds) The Sociology of Knowledge (London: Duckworth, 1970), 403-21.

96)Bohm and Bub, op.cit. note 74, 466.

97)Interview with David Bohm, Birkbeck College, London, 5th July 1977.

98)See note 77.

99)Interview with Jeffrey Bub, University of Western Ontario, London, Ontario, 29th September 1977.

100)J.Bub, 'What is a Hidden Variable Theory of Quantum Phenomena?', International Journal of Theoretical Physics Vol 2 (1969), 101-23.

101)See note 99.

102)For example, in his 1952 paper (op.cit. note 60, 166-7) Bohm tentatively suggests that QM might become invalid over very small distances but no details of possible experiments are given. Similarly, in their 1966 paper (op.cit. note 74) Bohm and Bub describe possible violations of QM in a tentative, speculative manner. However, in both papers, there is a great deal of discussion about conceptual frameworks. This theme was also stressed by Bohm in his book Causality and Chance in Modern Physics (London: Routledge and Kegan Paul, 1957), especially in Chapter Four, and also in much of Bohm's later work.

103)J.H.Tutsch, 'Collapse-Time for the Bohm-Bub Hidden Variable Theory', Reviews of Modern Physics Vol 40 (1968), 232-4.

104)J.H.Tutsch, 'Simultaneous Measurement in the Bohm-Bub Hidden-Variable Theory', Physical Review Vol 183 (1969), 1116-31, quote at 1116.

105)ibid., 1117.

106)J.H.Tutsch, 'Mathematics of the Measurement Problem in Quantum Mechanics', Journal of Mathematical Physics Vol 12 (1971), 1711-18, quote at 1711.

107)Personal correspondence from Jerald Tutsch to the author, 6th September, 1978.

108)It should be emphasised that Tutsch's attitude to the Bohm-Bub theory seemed as alien to Bohm and Bub as did Papaliolios'. In our interview, Bub told me that whereas he and Bohm treated the theory as a way of exploring alternative conceptual structures, so that the details of this particular theory were insignificant and "almost ad hoc", Tutsch, on the other hand,
"took it too seriously....he felt it might be roughly the correct physical theory....[and] tried to solve the equations....That was even worse than Papaliolios." (See note 99.)

109)Although FQM is not a typical scientific specialty, and does not fit any general model of the growth of specialties, I do not wish to imply that areas like FQM have been completely ignored by students of scientific specialties. For example, Edge and Mulkay draw a strong distinction between relatively stable groups of scientists who work on shared problems for many years, and "transient networks" of scientists who cluster briefly around single problems. Only the former groups fit the general model of specialty formation and growth, although, as Edge and Mulkay note, transient groups may be of great significance at some stages of specialty development. See D.O.Edge and M.J.Mulkay, Astronomy Transformed (New York: Wiley-Interscience, 1976), especially the discussion on page 127. The relatively undeveloped cognitive and social structure of FQM seems to have allowed such transient networks, together with lone individuals, to play an important part in the development of FQM for an unusually long period.

Footnotes to Chapter Five

1)Even this apparently straightforward identification of an 'irrelevant' variable is somewhat problematic. For example, the existence of the draft may have led some young men in the US to pursue academic research as a way of postponing or avoiding military service. I have no evidence that any of the LHV experimenters held this motive.

There is, however, an even subtler possible influence of the draft. Many US young men went to Canada in order to avoid being drafted, and the demand for higher education in Canada consequently increased. I was told, informally, that the University of Western Ontario took on extra staff to cope with this demand, and Holt obtained a post there around this time. If this expansion had not taken place, Holt might have been more likely to consider performing a replication of his experiment; one reason which he gave for not doing so was his preference for long-term employment rather than another short-term research post. Thus, the identification of 'relevant' features of the social context is always a risky, theory-laden interpretive process. In this chapter, I have chosen to exclude factors such as the political context in the US because such factors do not bear specifically on LHV.

2)See, for example, W.Hagstrom, The Scientific Community (Carbondale, Ill.; Southern Illinois University Press, 1975), and J.Gaston, Originality and Competition in Science: A Study of The British High Energy Physics Community (Chicago: University of Chicago Press, 1973).

3)See, for example, R.Merton, 'Singletons and Multiples in Scientific Discovery: A Chapter in the Sociology of Science', Proceedings of the American Philosophical Society Vol 105 (1961), 470-86.

4)The earlier paper is J.S.Bell, 'On the Problem of Hidden Variables in Quantum Mechanics', Reviews of Modern Physics Vol 38 (1966), 447-52; the later paper is J.S.Bell, 'On the Einstein-Podolsky-Rosen Paradox', Physics Vol 1 (1964), 195-200. It will be seen that the 'earlier' paper was actually published after the later paper. This was because the earlier paper 'got lost' in the editorial offices of Reviews of Modern Physics and was delayed by about two years.

5)D.R.Inglis, 'Completeness of Quantum Mechanics', Reviews of Modern Physics Vol 33 (1961), 1-7; and T.B.Day, 'Demonstration of Quantum Mechanics in the Large', Physical Review Vol 121 (1961) 1204-6.

6)R.Friedberg, 'Verifiable Consequences of the Einstein-Podolsky-Rosen Criterion for Reality' (Columbia University, New York: unpublished mimeo, 1970).

7)B.D'Espagnat, 'The Quantum Theory and Reality', Scientific American (November 1979), 128-40; and B.J.Hiley, 'Ghostly Interactions in Physics', New Scientist (6th March 1980) 746-9.

8)See, for example, J.Sarfatti, 'Towards a Quantum Theory of Consciousness, the Miraculous and God' (Physics/Consciousness Research Group:unpublished mimeo, PCRG, San Francisco) and B.Toben, Space-Time and Beyond (New York: Dutton, 1975).

9)H.P.Stapp, 'Bell's Theorem and World Process', Nuovo Cimento Vol 29B (1975), 270-6.

10)P.M.Clark and J.E.Turner, 'Experimental Tests of Quantum Mechanics', Physics Letters Vol 26A (1968), 447. This one-page paper contains several errors. In particular, the authors confuse local HVTs (discussed by Bell) with nonlocal HVTs (discussed by authors such as Bohm and Bub), by arguing that Bell's result is relevant for the Bohm-Bub theory. They also claim, erroneously, that Bell proved that all HVTs are non-local; in fact, Bell's point was that local HVTs might exist, but they would disagree with the predictions of QM.

11)L.R.Kasday, The Distribution of Compton Scattered Annihilation Photons and the Einstein-Podolsky-Rosen Argument (unpublished PhD thesis, Columbia University, New York, 1972), quote at 6.

12)Interview with John Clauser, Livermore, California, 31st October, 1977.

13)Personal correspondence from Abner Shimony to the author, 24th March 1977.

14)This was the Wu-Shaknov experiment, as analyzed by Bohm and Aharonov. See D.Bohm and Y.Aharonov, 'Discussion of Experimental Proof for the Paradox of Einstein, Rosen and Podolsky', Physical Review Vol 108 (1957), 1070-6.

15)C.A.Kocher and E.D.Commins, 'Polarization Correlation of Photons Emitted in an Atomic Cascade', Physical Review Letters Vol 18 (1967), 575-7. This experiment was performed without knowledge of Bell's work and insufficient readings were taken for a test of Bell's predictions.

16)See note 13.

17)See note 13.

18)Interview with Richard Holt, University of Western Ontario, London, Ontario, 28th September 1977.

19)See note 13.

20)J.F.Clauser, 'Proposed Experiment to Test Local Hidden-Variable Theories', Bulletin of the American Physical Society Vol 14 (1969), 578.

21)As discussed in Chapter One, interviewees were promised anonymity if they wished it. Consequently, many of the interview extracts, particularly those dealing with sensitive issues, are not attributed to any particular individual.

22)Personal correspondence from G.Nussbaum to the author, 15th May, 1978.

23)Interview with Donald Scarl, Polytechnic Institute of New York, Farmingdale, NY, 21st September 1977. The experience of the

actual LHV experimenters, most of whom are now in secure jobs, suggests that Scarl exaggerated the employment difficulties. However, the statement quoted is a clear expression of Scarl's own perception of the social context.

24)Holt, Freedman, Thomson and Lamehi-Rachti all performed LHV experiments as part of their PhD research; Clauser, Ullman, Fry and Aspect were all in temporary post-doctoral posts at the time of their LHV experiments.

25)Hagstrom found a similar lack of concern about type-casting among PhD physicists. See Hagstrom, op.cit. note 2, 160.

26)This is of course a general methodological problem, and was discussed in Chapter One.

27)Two caveats are necessary. First, I do not claim that I have discussed every relevant feature of the social context. Second, and more important, the LHV physicists were by no means passive pawns in this process. For example, as discussed earlier, Clauser made strenuous (and successful) efforts to find a laboratory with the necessary equipment, moving 3000 miles to do so.

28)For example, Fry failed to get funds when he applied for grants in 1970 and 1971.

29)The stylistic conventions of scientific writing, and the reasons for such conventions, have been widely discussed. For two rather different views, see G.N.Gilbert, 'Referencing as Persuasion', Social Studies of Science Vol 7 (1977), 113-22; and P.B.Medawar, 'Is the Scientific Paper a Fraud?', The Listener (12th September 1963), 377-8.

30)R.A.Holt, Atomic Cascade Experiments (unpublished PhD thesis, Harvard University, Cambridge, Mass., 1973), quote at II, 3-4.

31)S.J.Freedman, R.A.Holt and C.Papaliolios, 'Experimental Status of Hidden Variable Theories', in M.Flato et al (eds), Quantum Mechanics, Determinism, Causality and Particles (Dordrecht: Reidel, 1976), 43-59, quote at 44.

32)E.S.Fry and J.H.McGuire, 'An Experimental Test of Local Hidden Variable Theories' (unpublished proposal to Texas A&M University Research Council, November 1971), quote at 4.

33)M.Lamehi-Rachti, Mécanique Quantique et Théories des Variables Cachées Locales (unpublished PhD thesis, Université de Paris-Sud, 1976), quote at 2. (My translation.)

34)J.F.Clauser, M.A.Horne, A.Shimony and R.A.Holt, 'Proposed Experiment to Test Local Hidden-Variable Theories', Physical Review Letters Vol 23 (1969) 880-4, quote at 880. This small paper, the first published proposal, contains five references to 'decisive tests' and 'decisive experiments'.

35)As we shall see in Chapter Six, some authors have argued that Bell's analysis is not fully general; this is, however, very much a minority view.

36)Of course, if such an experiment were to refute QM, many more experiments would be required in order to discriminate between the many possible LHV theories and so identify the 'correct' one.

37)Another 'anomalous' experiment, that of Faraci and his group, produced results which did not provide clear evidence in favour of LHV, yet which clearly violated the QM predictions. This borderline case will discussed in more detail in Chapter Six.

38)R.A.Holt, op.cit. note 30.

39)S.J.Freedman and J.F.Clauser, 'Experimental Test of Local Hidden-Variable Theories', Physical Review Letters Vol 28 (1972) 938-41. (Freedman was a PhD student who collaborated with Clauser.)

40)See note 12.

41)See note 18.

42)See note 18.

43)See note 18.

44)See note 18.

45)These negotiations illustrate how an informal communication network had been constructed in LHV by 1974. Holt discussed a possible experiment at Princeton with Freedman (Clauser's ex-collaborator) who had moved there to work with Wigner. Holt also had discussions about a possible experiment in Paris with Vigier, who was a friend of Shimony's. The third location was Brown University, Providence, RI, where Holt had a temporary post after leaving Harvard.

46)See note 18 and note 1.

47)R.B.Blumenthal, D.C.Ehn, W.L.Faissler, P.M.Joseph, L.J.Lanzerotti, F.M.Pipkin and D.G.Stairs, 'Deviation from Simple Quantum Electrodynamics', Physical Review Letters Vol 14 (1965), 660-3.

48)Given Holt's insistence that there was an error in his apparatus, and his equally strong insistence that this error has never been identified, one might still wonder whether someone else would have been able to locate the error. To this extent, a question mark remains concerning Holt's competence. However, most of the LHV physicists recognised the difficulties involved in such error-tracing.

49)While Holt's response was 'successful' in this sense, it was not the only possible 'good' response. Some interviewees claimed that if they had been in Holt's position they would have gone about things slightly differently. For example, some said that they would have published the results; others said they would have scrapped the apparatus and started again. Despite this, it seems clear that most observers accepted Holt's tactics and that in their eyes the outcome was satisfactory if not maximal.

50)J.F.Clauser, 'Experimental Investigation of a Polarization Correlation Anomaly', Physical Review Letters Vol 36 (1976) 1223-26; E.S.Fry and R.C.Thompson, 'Experimental Test of Local Hidden-Variable Theories', Physical Review Letters Vol 37 (1976)

465-8. Thompson was a PhD student who assisted Fry in the later stages of his experiment; he was not involved in the planning and funding of the experiment.

51)As discussed in footnote 45, Freedman also investigated the possibility of repeating Holt's experiment.

52)For a critical discussion of the norms, see Chapter One .

53)The evidence for this apparently rather strong claim will be presented in Chapter Six. One important feature is that LHV physicists commonly argue that, because of the effects of errors in correlation experiments, a single result in favour of QM is enough to vindicate the theory. It seems very likely that, if no further experiments had been done, this argument would have been given greater prominence.

54)Clauser, op.cit. note 50, 1223.

55)Interview with Clauser (performed by H.M.Collins and T.J.Pinch) Lawrence Livermore Laboratory, Livermore, California, 4th April 1977.

56)Interview with E.S.Fry, Ann Arbor, Michigan, 19th October, 1977.

57)The Research Corporation, Burlinghame, California.

58)The main improvement was the method of excitation: whereas earlier experiments had excited their atoms thermally or by collisions with electrons, Fry used a tunable dye laser. This allowed a much more precise excitation of the atoms, so that only the desired photons are emitted. Additionally, data could be generated at a much higher rate, and the level of isotopic purity was much less critical.

59)See note 56.

60)See note 56.

61)See note 56.

62)E.S.Fry, 'Two-Photon Correlations in Atomic Transitions', Physical Review Vol A8 (1973), 1219-27.

63)Fry and Thompson, op.cit. note 50, 467.

64)See note 56.

65)Fry moved from Texas A&M University to the University of Michigan, where he had previously obtained his B.S., M.S., and PhD degrees.

66)For a brief account of such parity-violation experiments, see P. Sanders, 'Can Atoms Tell Left from Right?', New Scientist (31st March, 1977), 764-6.

67)See note 56.

68)Obviously, if QM had been refuted by the experiments, their plans might well have been changed.

69)For example, Shimony brought together Holt, Horne and Clauser; he also visited Geneva and Paris, and was largely responsible for introducing Aspect to LHV.

70)A.Shimony, 'Experimental Test of Local Hidden-Variable Theories' in Societa Italiana di Fisica (B.D'Espagnat ed), Foundations of Quantum Mechanics, Proceedings of the Enrico Fermi International Summer School, Course 49 (New York: Academic Press, 1971), 182-94, quote at 191-2.

71)B.S.DeWitt, 'Reply to Critics', Physics Today (April 1971), 41-4, quote at 42.

72)A.Shimony, 'The Role of the Observer in Quantum Theory', American Journal of Physics Vol 31 (1963), 755-73.

73)J.Hall, C.Kim, B.McElroy and A.Shimony, 'Wave-Packet Reduction as a Medium of Communication', Foundations of Physics, Vol 7 (1977), 759-67.

74)Shimony, op.cit. note 72, 772.

75)Hall et al., op.cit. note 73, 761.

76)L.de Broglie, 'The Reinterpretation of Wave Mechanics', Foundations of Physics Vol 1 (1970), 5-15.

77)See, for example, D.Bohm and B.J.Hiley, 'On the Intuitive Understanding of Nonlocality as Implied in Quantum Theory', Foundations of Physics Vol 5 (1975), 93-109.

78)Editorial, Foundations of Physics Vol 1 (1970), 2-3, quote at 2.

79)For a useful collection of papers on NCRT, see L.Mandel and E.Wolf (eds) Coherence and Quantum Optics, Proceedings of the Third Rochester Conference on Coherence and Quantum Optics, June 1972 (New York: Plenum Press, 1973)

80)J.F.Clauser, 'Experimental Limitations to the Validity of Semiclassical Radiation Theories', Physical Review Vol 6A (1972) 49-54. Essentially, the argument is that NCRT, being classical, is also a fundamentally local theory, whereas QM is non-local.

81)Kocher and Commins, op.cit. note 15.

82)Freedman and Clauser, op.cit. note 39.

83)J.F.Clauser, 'Experimental Distinction between the Quantum and Classical Field-Theoretic Predictions for the Photoelectric Effect', Physical Review Vol 9D (1974), 853-60.

84)ibid., 859.

85)ibid., 855.

86)The exception is Aspect, whose experiment will be discussed in Chapter Six.

87)See note 55.

88)Interview with Philip Pearle, Hamilton College, Clinton, NY, 4th October, 1977.

89)Interview with Stuart Freedman, Stanford Unviersity, 1st November, 1977.

90)Of course, labelling an idea as 'good' is not an unproblematic judgement. As when interviewees described LHV as a 'good thesis experiment', it is likely that such evaluations are influenced by a large number of social and cultural factors.

91)Interview with Costas Papaliolios, University of Arizona, Tucson, Arizona, 7th November 1977.

92)Interview with Ed Fitchard, Stirling University, 14th April 1977.

93)P.Pearle, 'Hidden-Variable Example Based upon Data Rejection', Physical Review Vol D2 (1970), 1418-25.

94)See note 88.

95)See note 88.

96)M.Mugur-Schachter, 'The Quantum-Mechanical One-System Formalism', in J.L.Lopes and M.Paty (eds) Quantum Mechanics, a Half Century Later (Dordrecht: Reidel, 1977), quote at 145.

97)See, for example, B.R.Frieden, 'Uncertainty Product for a Subensemble of Particles', International Journal of Theoretical Physics Vol 15 (1976), 389-91.

98)D.Matthys, An Experimental Approach to the Uncertainty Principle (unpublished PhD thesis, Washington University, St Louis, Missouri, 1975). This experiment did not actually test the principle, because it could not produce accurate enough data.

99)ibid., 11 and 14.

100)E. Jaynes in Mandel and Wolf, op.cit. note 79, quote at 78-9. We should perhaps be slightly sceptical about these remarks: Jaynes habitually writes in a rather dramatic style, and by 1977, when the next conference in this series was held, Jaynes conceded that NCRT had many difficulties, but Clauser's results were not even mentioned as one of them (E.T.Jaynes, unpublished paper presented at Fourth Rochester Conference on Coherence and Quantum Optics, June 1977).

101)M.Paty, 'Recent Attempts to Verify Quantum Mechanics' in Flato et al (eds), op. cit. note 31, 261-89, quote at 287.

102)J.Bub, 'On the Possibility of a Phase-Space Reconstruction of Quantum Statistics: A Refutation of the Bell-Wigner Locality Argument', Foundations of Physics Vol 3 (1973), 29-44, quote at 38.

103)S.Freedman and E.Wigner, 'On Bub's Misunderstanding of Bell's Locality Arguemtn', Foundations of Physics Vol 3 (1973), 457-8.

104)M.Flato, 'Quantum Mechanics and Determinism', in Flato et al (eds) op.cit. note 31, 19-31, quote at 29.

105)Freedman et al, op.cit. note 31, 57.

106)R.Maiocchi, Review of Flato et al (op.cit. note 31), Scientia, Vol 111 (1976), 505-9, quote at 507.

107)ibid., 507.

108)Interview with R.Blohm, McGill University, Montreal, 26th September, 1977.

109)D.Bohm, 'On the Creation of a Deeper Insight into What May Underlie Quantum Physical Law', in Flato et al, op.cit. note 31, 1-10, quote at 2.

110)ibid., 2..

111)ibid., 10.

112)ibid., 6.

113)L.de la Peña, A.M.Cetto and T.A.Brody, 'On Hidden-Variable Theories and Bell's Inequality', Nuovo Cimento Letters Vol 5 (1972), 177-84, quote at 180. This conclusion is not accpeted by the LHV group.

114)R.Fox, 'Low-Energy Proton-Proton Scattering as a Test of Local HVT', Nuovo Cimento Letters Vol 2 (1971), 565-7; R.Fox and B.Rosner, 'Proposed Experiment to test Local HVTs', Physical Review Vol D4 (1971), 1243-4.

The first of these proposals was taken up by Lamehi-Rachti and Mittig, and an experiment was performed (see note 33). However, Fox's original design was modified, and the experimenters had no personal contact with Fox.

115)R.Opher, 'Are Quantum Processes Cosmologically Induced?', Foundations of Physics Vol 5 (1975), 309-21.

116)ibid., 320.

117)Editorial, International Journal of Theoretical Physics, Vol 1 (1968), 1.

118)Editorial, Journal of Philosophical Logic, Vol 1 (1972), 1.

119)Editorial, Physical Review Vol D8 (1972), 357.

120)ibid.

121)L.E.Ballentine, 'The Statistical Interpretation of Quantum Mechanics', Reviews of Modern Physics Vol 42 (1970), 358-81.

122)Editorial, Reviews of Modern Physics Vol 42 (1970), 357.

123)ibid.

124)ibid.

125)Since many LHV papers were published in Physical Review and Physical Review Letters, and since interviewees also reported a favourable response to their experiments from their colleagues, Clauser and the rest of the LHV group have apparently succeeded in gaining 'respectability' for this activity, now matter which definition of respectability we use.

Obviously, the different usages of the term 'respectability' are closely related. For example, acceptance by Physical Review requires acceptable methodology; conversely, the fact that such a journal is prepared to accept papers in this field is itself likely to enhance the status of this activity.

126)It would be naive to take editorial policy statements as wholly accurate or complete descriptions of the conditions which govern the acceptance or rejection of papers. Nevertheless, such statements do provide one way for prospective authors to assess the likely reception afforded to their manuscripts.

127)See, for example, N.Storer, The Social System of Science (New York: Rinehart and Winston, 1966) and W.O.Hagstrom (op.cit.note 2).

Footnotes to Chapter Six

1)As Collins puts it, the culture of science "legitimates and limits the parameters requiring control in the experimental situation, without necessarily formulating, enumerating, or understanding them". H.M.Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon or the Replication of Experiments in Physics', Sociology Vol 9 (1975), 205-24, quote at 207.

2)It must be conceded that a group may accept the truth of an idea while admitting that the idea is 'hard to swallow'. QM itself is often described in such terms by its supporters. However, when a QM supporter describes QM as 'implausible', he is actually saying that it may seem implausible to a layman. The insider 'knows' that QM is a very elegant, comprehensive and accurate theory and that it is actually very plausible. Indeed, such a usage supports my own arguments, since it recognises that plausibility is context-dependent and that a plausibility structure is built up through socialization processes which the layman has not undergone.

3)Pickering draws an interesting distinction between 'socially acceptable' and 'socially accepted' concepts. Both are permissible in scientific accounts, though the latter refers to concepts which are 'known' to be valid while the former refers to those which are not yet accepted as proper descriptions of reality but which are permissible because they have already been the subject of scientific activity. See Andrew Pickering, 'Constraints on Controversy: the Case of the Magnetic Monopole', Social Studies of Science Vol 11 (1981), 63-94.

LHV may not be typical, in that the whole notion of hidden variables was relatively implausible, but I was impressed with the ease with which physicists entertained hypotheses which were definitely not 'socially accepted' nor even, in Pickering's terms, particularly 'acceptable'. The distinction between 'acceptable' and 'accepted' is fairly clear, but it seems very easy for a concept to go from 'unacceptable' to 'acceptable' simply by judicious citation and (as in LHV) by a fairly tolerant approach to speculative ideas.

4)J.S.Bell, 'On the Einstein-Podolsky-Rosen Paradox', Physics Vol 1 (1964), 195-200.

5)J.F.Clauser and A.Shimony, 'Bell's Theorem: Experimental Tests and Implications', Reports on Progress in Physics Vol 41 (1978), 1881-1927, quote at 1889.

6)For a recent discussion of Hillman's critique of conventional electron microscopy, see R.G.A.Dolby, 'Controversy and Consensus in the Growth of Scientific Knowledge' (Canterbury, Kent: Unit for the History, Philosophy and Social Relations of Science, University of Kent, unpublished mimeo.)

7)J.F.Clauser, 'Philosophical Motivations of Bell's Theorem and the Experimenter's Problem' (Berkeley, Calif.: Lawrence Berkeley Lab, University of California, unpublished mimeo LBL-5418, April 1976), 2.

8)A.Shimony, 'Experimental Test of Local Hidden-Variable Theories' in Societa Italiana di Fisica (B.D'Espagnat, ed.) Foundations of Quantum Mechanics, Proceedings of the Enrico Fermi Summer School, Course 49 (New York: Academic Press, 1971), 182-94, quote at 190.

9)J.F.Clauser, M.A.Horne, A.Shimony and R.A.Holt, 'Proposed Experiment to Test Local Hidden-Variable Theories', Physical Review Letters Vol 23 (1969), 880-4.

10)Clauser, op.cit. note 7, 3, emphasis added.

11)L.R.Kasday, The Distribution of Compton Scattered Annihilation Photons and the Einstein-Podolsky-Rosen Argument (unpublished PhD thesis, Columbia University, NY, 1972).

12)Some years later, another high-energy experiment was performed by Lamehi-Rachti and Mittig, using protons instead of photons. The assumptions required for such an experiment were generally held to be even stronger than for high-energy photons: "Auxiliary assumptions similar to those required for the [high energy photon] experiments are required ....it should be noted that their geometry requires an additional assumption not necessary for the [photon] experiments." (Clauser and Shimony, op.cit. note 5, 1918.)

However, such an experiment was justified by its authors on the grounds that it was important to test QM using particles other than photons and interactive forces other than electromagnetic (proton scatter involves the strong nuclear force). See M.Lamehi-Rachti and W.Mittig, 'Quantum Mechanics and Hidden Variables', Physical Review Vol D14 (1976), 2546. In this case, the choice of experiment was made long after the first LHV experiments were under way - unlike Kasday's choice - so the argument about complementary types of experiment would seem to have played a large part in their choice.

13)Shimony, op.cit. note 8, 190-91.

14)Clauser et al., op.cit. note 9, 883-4.

15)See, for example, J.F.Clauser and M.A.Horne, 'Experimental Consequences of Objective Local Theories', Physical Review Vol D10 (1974), 526-35.

16)See note 1.

17)S.J.Freedman and J.F.Clauser, 'Experimental Test of Local Hidden-Variable Theories', Physical Review Letters Vol 28 (1972) 938-41.

18)R.A.Holt, Atomic Cascade Experiments (unpublished PhD thesis, Harvard University, 1973).

19)J.F.Clauser, 'Experimental Investigation of a Polarization Correlation Anomaly', Physical Review Letters Vol 36 (1976), 1223-6; E.S.Fry and R.C.Thomson, 'Experimental Test of Local Hidden-Variable Theories', Physical Review Letters Vol 37 (1976), 465-8.

20)See Clauser and Shimony, op.cit. note 5, for a discussion of these experiments.

21)See footnote 12, and M.Lamehi-Rachti, Mécanique Quantique et Théories des Variables Cachées Locales (unpublished PhD thesis, Université de Paris-Sud, 1976).

22)Clauser and Shimony, op.cit. note 5, 1919.

23)Lamehi-Rachti, op.cit. note 21, 24 (my translation).

24)ibid., 122-3.

25)A.A.Ross-Bonney, 'Does God Play Dice?', Nuovo Cimento Vol 30B (1975), 55-79, quote at 76.

26)A.Baracca, D.J.Bohm, B.J.Hiley and A.E.G.Stuart, 'On Some New Notions Concerning Locality and Nonlocality in the Quantum Theory', Nuovo Cimento Vol 28B (1975), 453-65, quote at 460.

27)P.H.Eberhard, 'Tests of Unitarity' (Berkeley, Calif.: Lawrence Berkeley Lab, University of California, unpublished mimeo LBL-4885, May 1976), 290.
Other papers which cite Freedman and Clauser but which neglect Holt include J.Edwards and L.E.Ballentine, 'Sufficient Conditions for Objective Local Theories, Part I', Nuovo Cimento Vol 29B (1975), 100-10 and 'Part II', ibid., Vol 34B (1976), 91-6; K.Eppley and E.Hannah, 'The Necessity of Quantizing the Gravitational Field', Foundations of Physics Vol 7 (1977), 51-68; and R.Opher, 'Are Quantum Processes Cosmologically Induced?', Foundations of Physics Vol 5 (1975), 309-21.

28)R.A.Holt and F.M.Pipkin, 'Quantum Mechanics Versus Hidden Variables: Polarization Correlation Measurement on an Atomic Mercury Cascade' (Cambridge, Mass.: Department of Physics, Harvard University, unpublished mimeo, 1974). Published reviews were by S.J.Freedman and R.A.Holt, 'Tests of Local Hidden-Variable Theories in Atomic Physics', Comments in Atomic and Molecular Physics Vol 5 (1975), 55-62; and S.J.Freedman, R.A.Holt and C.Papaliolios, 'Experimental Status of Hidden Variable Theories', in M.Flato et al. (eds) Quantum Mechanics, Determinism, Causality and Particles (Dordrecht: Reidel, 1976), 43-59.

29)Shimony, op.cit. note 8, 192.

30)R.B.Blumenthal et al., 'Deviation from Simple Quantum Electrodynamics', Physical Review Letters Vol 14 (1965), 660-9. Some years later, Pipkin said of this experiment, "They.... reported a large deviation from QED. Later experiments have shown that this result was spurious". F.M.Pipkin, 'Quantum Electrodynamics at Small Distances' in G.K.T.Conn and G.N.Fowler (eds) Essays in Physics, Vol 2 (London: Academic Press, 1970). This rather off-hand dismissal does not do justice to the original paper, which generated a great deal of theoretical and experimental activity.

31)There is an interesting contrast here with Wynne's study of Barkla. Barkla originally claimed to have discovered a 'J-series' of radiation, but later withdrew this claim, while continuing to argue in favour of the (quite separate)J-phenomenon. However, studying the published comments on Barkla's later work, Wynne notes that "all the papers refer to Barkla's previous mistake as if this were evidence of his being mistaken again". See

B.Wynne, 'C.G.Barkla and the J Phenomenon: A Case Study in the Treatment of Deviance in Physics', Social Studies of Science Vol 6 (1976) 307-47, quote at 327. In Pipkin's case, no mention of his earlier mistake ever appeared in the LHV literature. Pipkin's more lenient treatment is probably due to two factors: first, the small number of people involved in LHV made it possible for knowledge of his mistake to be circulated quite effectively by informal means. Second, since Pipkin himself rejected Holt's result, there was no need to discredit Pipkin in order to reject Holt's experiment.

32) This comment is taken from my personal notes taken during a conference of LHV experimenters, at which Pipkin gave a talk. Not surprisingly, this comment does not appear either in Holt and Pipkin's unpublished manuscript, or in the published abstract of Pipkin's conference paper.

The lack of agreement on any specific cause of Holt's error is virtually irrelevant, given that there is agreement on the important point, namely that Holt did make an error. A similar situation existed with D.C.Miller's 'ether-drift' experiments, which disagreed with the results of Michelson and Morley. Despite the fact that over 20 years elapsed before the cause of Miller's error was agreed, his results were not accepted. See L.S.Swenson, 'The Michelson-Morley-Miller Experiments Before and After 1905', Journal for the History of Astronomy Vol 1 (1970) 56-78.

33) Interview with Holt, University of Western Ontario, 28th September 1977.

34) Interview with Freedman, Stanford University, 1st November 1977.

35) Clauser and Shimony, op.cit. note 5, 1919.

36) ibid., 1910.

37) Holt and Pipkin, op.cit., note 28, 26.

38) Freedman et al., (1975), op.cit. note 28, 61; Freedman et al., (1976), op.cit. note 28, 57.

39) Clauser, op.cit. note 19, 1223.

40) Lamehi-Rachti, op.cit. note 21, 122.

41) See note 33 (emphasis added).

42) See Clauser, op.cit. note 19 for details of technical differences between the experiments. Since Fry, unlike Clauser, did not attempt even a partial replication of Holt's experiment, Fry's apparatus is very different from Holt's.

43) In other disciplines, such as psychology and medicine, the effects of conscious or unconscious experimenter bias are well known, and techniques such as 'double-blind' trials have been developed in an attempt to cope with such phenomena. Parapsychologists often make similar claims, for instance, concerning the effects of sceptical observers. One consequence if this is that the concepts of replication, and of similar/dissimilar experiments, are often at the root of disputes in parapsychology. See

H.M.Collins, 'Upon the Replication of Scientific Findings: A Discussion Illuminated by the Experiences of Researchers into Parapsychology', Proceedings of the 4S/ISA Conference on Social Studies of Science, Cornell University, November 1976 (School of Humanities and Social Sciences, University of Bath, unpublished mimeo). Wynne has also noted how Dunbar's "replication" of Barkla's work was by no means a replication from Barkla's point of view, because it ignored certain features which, to Barkla, were crucial. Yet Dunbar's experiment was portrayed as a replication, suggesting to Wynne that "it was merely 'rubber stamping' attitudes that were by then already deeply ingrained". Wynne, op.cit. note 31, quote at 330.

44)J.F.Clauser, 'Measurement of the Circular-Polarization Correlation in Photons from an Atomic Cascade', Nuovo Cimento Vol 33B (1976) 740-6.

45)ibid., 743.

46)ibid., 743.

47)Despite the fact that Clauser presents this result as being in favour of QM, no other author, to the best of my knowledge, has quoted this result as evidence against LHV. However, it is also true that no-one has criticised Clauser's presentation, let alone used this result to support LHV.

48)It is fairly well established that scientific papers are written in a conventionalised style, which bears little relation to the details of the research process. However, my findings may appear to conflict with those of Pinch. See T.J.Pinch, 'The Sun-Set: The Presentation of Certainty in Scientific Life', Social Studies of Science Vol 11 (1981), 131-58. Pinch found that his interviewees maintained a rather formal posture, and were mostly unwilling to make the sort of frank confessions of uncertainty, hunch and bias which one might expect in an informal context. However, as Pinch points out, his interviewees were aware that there was substantial public interest in their field, and he feels that they perceived him as a representative of this wider public audience. In addition, there remains genuine controversy in Pinch's field of study.

In LHV, on the other hand, many interviewees seemed surprised and flattered that an outsider would be interested in their work. The issues involved were also much more clear-cut, providing a solid base of 'certainty' to underpin informal, seemingly exaggerated statements. Other factors, such as the demeanour of the interviewer, may presumably also affect the sort of statements produced.

49)G.Zukav, The Dancing Wu Li Masters (London: Fontana, 1980); H.P.Stapp, 'Bell's Theorem and World Process', Nuovo Cimento Vol 29B (1975), 270-6. The only exception is that Aspect's experiment, and the hypothesis he proposes to test, is usually discussed by such authors. This is quite consistent with the argument I will develop below, namely that Aspect has altered the plausibility of the hypothesis he is testing.

50)According to Collins, this is fairly typical; he argues that only the 'Core-Set' of scientists who are actively involved in a controversy are aware of the inconclusive nature of much experimental data. Scientists further from the actual research process, he claims, are more likely to perceive this process as unproblematic. See H.M.Collins, 'The Role of the Core-Set in Modern Science: Social Contingency with Methodological Propriety in Discovery', History of Science Vol 19 (1981), 6-19.

51)G.Faraci, S.Gutkowski, S.Notarrigo and A.R.Pennisi, 'An Experimental Test of the EPR.Paradox', Nuovo Cimento Letters Vol 9 (1974), 607-11.

52)L.Kasday, J.D.Ullman and C.S.Wu, 'Angular Correlation of Compton-Scattered Annihilation Photons and Hidden Variables' Nuovo Cimento Vol 25B (1975), 633-61.

53)Although the effects concerned were very different from those in Holt's experiment, it will be recalled that Holt had also been criticised on the grounds that stress effects might be distorting his results.

54)Kasday et al., op.cit. note 52, 659.

55)G.Faraci and A.R.Pennisi, 'Polarization of the Annihilation Photons of Triplet Positronium', Nuovo Cimento Vol 31B (1976), 289-95.

56)ibid., 289.

57)J.S.Bell, 'The Theory of Local Beables', Epistemological Letters Vol 9 (1976) 11-24, quote at 21.

58)Faraci and Pennisi, op.cit. note 55, 293.

59)First Session of the Thinkshops on Physics: Experimental Quantum Mechanics, organised by J.S.Bell and B.D'Espagnat, held at the Ettore Majorana Centre for Scientific Culture, Erice, Sicily, 18th-23rd April, 1976. Abstracts of contributions published in Progress in Scientific Culture Vol 1 (1976) 439-60.

60)S.Notarrigo, 'Polarization Correlation of Annihilation Radiation' op.cit. note 59, 452-3, quote at 453.

61)A.R.Wilson, J.Lowe and D.K.Butt, 'Measurement of the Relative Planes of Polarization of Annihilation Quanta as a Function of Separation Distance', Journal of Physics Vol G2 (1976), 613-24; M.Bruno, M.D'Agostino and C.Maroni, 'Measurement of Linear Polarization of Positron-Annihilation Photons', Nuovo Cimento Vol 40B (1977), 142-52.

62)I was unable to travel to Bologna. As for the Birkbeck group, Dr. Wilson died shortly after completing his experiment, and Mrs Lowe and Dr. Butt did not agree to cooperate with my research. Neither of these groups was represented at the experimenters' conference in Sicily.

63)Interviews with B.Hiley and D.Bohm, Birkbeck College, University of London, 5th July 1977.

64)This proposal, the so-called 'Furry hypothesis', suggests that when correlated pairs of photons become separated by a large enough distance, 'spontaneous localization' - a breakdown of the correlation - may occur. As well as providing support for LHV, Faraci's results seem to support this hypothesis. However, neither Wilson nor Maroni found any evidence for spontaneous localization. For further details of this hypothesis, see Clauser and Shimony, op.cit. note 5, 1922 and Baracca et al., op.cit. note 26.

65)Clauser and Shimony, op.cit. note 5, 1917.

66)F.M.Pipkin, 'Atomic Physics Tests of the Basic Concepts of Quantum Mechanics' in D.R.Bates and B.Bederson (eds) Advances in Atomic and Molecular Physics Vol 14 (New York: Academic Press, 1978) 281-340, quote at 316.

67)Personal correspondence from Maroni to the author, 30th March 1977.

68)See note 67.

69)Bruno et al., op.cit. note 61, 146.

70)G.Faraci and A.R.Pennisi, 'Polarization Correlation of a Photon Pair', Physics Letters Vol 66A (1978), 15-16, quote at 15.

71)G.Faraci and A.Pennisi, 'Polarization of Triplet P_s Photons in Assymmetrical Decay', Nuovo Cimento Vol 55B (1980), 257-63.

72)Personal correspondence from Faraci to the author, 26th June 1977.

73)See note 72.

74)D.Gutkowski, G.Masotto, and M.V.Valdes, 'On the Sufficiency of Bell's Conditions' Nuovo Cimento Vol 50B (1979), 323-43, quote at 324.

75)Again, Wynne's study of Barkla (op.cit. note 31) provides a similar case of a scientist who is able to construct a self-consistent defence of his views yet who is judged to be wrong by the cultural conventions of the physics community of the time.

76)Other non-technical factors may also have been involved in this assessment of quality. These may have included the prestige of Columbia University compared with a Sicilian university, and the personal reputation of Professor Wu, the leader of Kasday's group. Also, in informal conversation, some US physicists displayed a certain degree of anti-Italian bias. It is certainly true that Italian physicists generally hold more favourable views of Faraci's work.

77)Letter from J.H.McGuire to J.F.Clauser. The copy I obtained was undated, but a reference to a 'recently published' paper indicates that it was written in 1970.

78)J.S.Bell, 'Introduction to the Hidden-Variable Question', in Societa Italiana di Fisica, op.cit. note 8, 171-81; Clauser and Horne, op.cit. note 15; Clauser and Shimony, op.cit. note 5.

79)Bell, op.cit.note 4, 199.

80)Clauser et al., op.cit. note 9.

81)Shimony, op.cit. note 8, 191.

82)Clauser and Horne, op.cit. note 15.

83)This point has been discussed by Clauser (op.cit. note 1). To give an example, morphologically dissimilar objects such as thermocouples and mercury thermometers are perceived as doing roughly the 'same' thing because of our adherence to certain theoretical ideas about heat and thermometry.

84)When unpolarized light is reflected from glass, it is partially polarized, and the same is true for transmitted light. The effect varies with the angle of incidence and is a maximum at an angle known as the Brewster angle. When oriented at this angle, a glass plate behaves like a sheet of polaroid, and the effect is enhanced by having a whole series of parallel plates. Calcite crystals are birefringent, which means that unpolarized light entering a crystal splits up into two differently polarized components. If already-polarized light enters either of these two devices, its transmission coefficient can be varied by rotating the device. In this way, the polarization of the light can be measured.

85)This is a generalisation. Recall that Gutkowski et al (op.cit. note 74) did argue in favour of some unknown parameter to account for Holt's anomalous results. However, this is the only occasion on which such views have been expressed in print, and in my fieldwork I found no support for such a proposal.

86)A.Aspect, 'Proposed Experiment to Test the Nonseparability of Quantum Mechanics', Physical Review Vol D14 (1976), 1944-51.

87)Clauser and Shimony, op.cit. note 5, 1914 and 1921.

88)Aspect, op.cit. note 86, 1949.

89)Interview with Aspect, Erice, Sicily, 22nd April, 1976.

90)A.Aspect, P.Grangier and G.Roger, 'Experimental Tests of Realistic Local Theories Via Bell's Theorem' (Institut d'Optique Theorique et Appliquee, Universite de Paris-Sud, Orsay: March 1981, unpublished mimeo.)

91)Bell, op.cit. note 57.

92)A.Shimony, M.A.Horne and J.F.Clauser, 'Comment on "The Theory of Local Beables"', Epistemological Letters Vol 13 (1976), 1-8, quote at 4.

93)ibid., 5.

94)This is a well-known thought-experiment, discussed by James Clerk Maxwell, which apparently violates the Second Law of Thermodynamics by presenting an isolated physical system in which entropy decreases as time passes. See M.W.Zemansky, Heat and Thermodynamics (New York: McGraw-Hill, 1968), 271 for a good account of this paradox.

95)J.S.Bell, 'Free Variables and Local Causality', Epistemological Letters Vol 15 (1977), 79-84, quote at 79.

96)ibid., 83.

97)ibid., 83.

98)Interview with Michael Horne, MIT, Cambridge, Mass., 13th October, 1977.

99)This list is not intended to be a comprehensive review of the wide variety of conclusions which have been drawn about LHV. For further details of the views quoted, see D.Bohm and B.J.Hiley, 'On the Intuitive Understanding of Nonlocality as Implied by Quantum Theory', Foundations of Physics Vol 5 (1974), 93-109; J.Sarfatti, 'Towards a Quantum Theory of Consciousness, the Miraculous and God' (1977) and various other unpublished mimeos from the Physics/Consciousness Research Group, San Francisco; L.E.Ballentine, 'A Survey of Hidden Variable Theories' (Book Review) Physics Today (October 1974), 53-5; V.Augelli, A.Garuccio andF.Selleri, 'La Mécanique Quantique et la Réalité', Annales de la Fondation Louis de Broglie Vol 1 (1976), 154-73; H.P.Stapp, op.cit. note 49; and T.A.Brody and L. de la Peña-Auerbach, 'Real and Imagined Nonlocalities in Quantum Mechanics', Nuovo Cimento Vol 54B (1979), 455-62.

100)See for example L.de la Peña, A.M.Cetto and T.A.Brody, 'On Hidden Variable Theories and Bell's Inequality', Nuovo Cimento Letters Vol 5 (1972), 177-84; D.Kershaw, 'Is There an Experimental Reality to Hidden Variables?' (College Park, Maryland: Univesity of Maryland, unpublished mimeo, 1973, Technical Report 74-034); G.Lochak, 'Has Bell's Inequality a General Meaning for Hidden-Variable Theories?', Foundations of Physics Vol 6 (1976), 173-84; L.de Broglie, G.Lochak, L.A.Beswick and J.Vassalo-Pereira, 'Present, Predicted and Hidden Probabilities', Foundations of Physics Vol 6 (1976), 3-14.

101)Italian theorists in particular have been involved in this activity. See, for example, D.Gutkowski and G.Masotto, 'An Inequality Stronger than Bell's Inequality', Nuovo Cimento Vol 22B (1974), 121-30; D.Gutkowski et al, op.cit. note 74; L.Schiavulli and F.Selleri, 'Further Consequences of Einstein Locality', Foundations of Physics Vol 9 (1979), 339-52; R.Livi, 'New Tests of Quantum Mechanics for Multi-Valued Observables', Nuovo Cimento Letters Vol 19 (1977), 189-92.

102)Interview with John Clauser, Lawrence Livermore Laboratory, Livermore, California, 31st October 1977.

103)Ironically, parapsychologists have become involved in the LHV activity, but they now argue that the results in favour of QM provide evidence to support their views. See, for example, J.Sarfatti, op.cit. note 99.

104)This is of course a very general argument, discussed by Duhem, Quine, Feyerabend, Lakatos and others. See Chapter One for a fuller treatment of this argument.

105)Apart from differences in terminology, this conclusion seems quite consistent with those reached by Collins (op.cit. note 1), Pickering (op.cit. note 3), and many other authors. Naturally, not all who share this view would agree on whether to stress the constraining influence exerted by culture, or the manner in which physicists freely choose to employ those conceptual categories which their cultural background makes available as resources, in order to account for experimental data. In the next chapter, I shall expand on this point, and I shall also indicate the particular advantages of my own terminology.

Footnotes to Chapter Seven

1) For examples of historical studies of the role of science in social debates, see R.M.Young, 'Evolutionary Biology and Ideology: Then and Now', Science Studies Vol 1 (1971), 177-206; C.E. Rosenberg, 'Scientific Theories and Social Thought', in S.B. Barnes (ed) Sociology of Science (Harmondsworth: Penguin, 1972) and D.A.MacKenzie, Statistics in Britain 1865-1930 (Edinburgh: Edinburgh University Press, 1981). Other references to empirical studies were provided in Chapter One.

2) One important feature of this public/private subdivision of science is the distinction between expert knowledge (facts) and internal disagreements between scientists. In many cases, this distinction has broken down, leading to a quite different public conception of the expert. See, for example, A.Mazur, 'Disputes between experts', Minerva Vol 11 (1973), 243-62; S.J.Reiser, 'Smoking and Health: The Congress and Causality', in S.Lakoff (ed) Knowledge and Power (London: Collier-Macmillan, 1966), 293-311; and D.Nelkin, 'The political impact of technical expertise', Social Studies of Science Vol 5 (1975), 35-54.

3) For a useful summary and synthesis of empirical studies of the growth of specialties, see M.Mulkay, 'Sociology of Science in the West', Current Sociology Vol 28 (1980), 1-168, especially pp 18-22. For a more detailed critical review of such studies, see D.O.Edge and M.Mulkay, Astronomy Transformed, (New York: Wiley, 1976), especially Chapter Ten.

4) It is important not to confuse 'failed fields' with 'dead fields'. For example, Fisher has studied Invariant Theory in mathematics, which 'died' early this century. See C.S.Fisher, 'The Death of a Mathematical Theory: a Study in the Sociology of Knowledge', Archives for the History of the Exact Sciences Vol 3 (1966), 137-59. Invariant theory was at one time a central part of mathematics. The mechanism of its decline is therefore not directly relevant for FQM, which has never 'taken off'. However, Fisher's account of the reasons for decline, such as the breakdown of teacher-pupil links, does seem in agreement with my own findings. Edge and Mulkay (op.cit. note 3) have criticised Fisher for underemphasising intellectual factors, such as the feeling that the main problems in Invariant Theory had been solved. Such factors are indeed important in FQM: to its critics, the problems which it deals with are merely pseudo-problems.

5) Mulkay (1980), op.cit. note 3, 19.

6) See Chapter One for a discussion of the methodological problems posed by the peculiar social and cognitive structure of FQM.

7) As pointed out in Chapter Four, Edge and Mulkay have suggested that 'transient networks', which do not fit the typical description of scientific specialties, may be more common, and more important, than we realise at present. See Edge and Mulkay, op.cit. note 3, especially p 127.

8) Thus, norms are used to provide ex post facto rationalisations of conduct. Garfinkel has suggested that jurors use 'official'

concepts of evidence, proof, and guilt to account for their verdict:
"jurors did not actually have an understanding of the conditions that defined a correct decision until after the decision had been made. Only in retrospect did they decide what they did that made their decisions correct ones. When the outcome was in hand they went back to find the 'why', the things that led up to the outcome....in order to give their decisions some order, which namely, is the 'officialness' of the decision."
H.Garfinkel, Studies in Ethnomethodology (Englewood Cliffs, NJ: Prentice-Hall, 1967), quote at 14.

I have also argued that we should view the use of von Neumann's proof against Bohm's theory in just the same way.

9)Of course, Aspect's results will also have an important effect on the plausibility of the timing hypothesis.

10)Detailed references to studies by Collins, Pickering, Pinch, Travis, Wynne and other authors can be found in Chapter One.

11)H.M.Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', Sociology Vol 9 (1975), 205-24; and H.M.Collins, 'Son of Seven Sexes: The Social Destruction of a Physical Phenomenon', Social Studies of Science Vol 11 (1981), 33-62.

12)Collins (1981), op.cit. note 11, quote at 54, emphasis in original.

13)This includes not only philosophical critiques of the concept of falsifiability, but also historical studies of 'rejected knowledge' which indicate that there is nothing inherent in such knowledge which necessitates its rejection or otherwise distinguishes it from 'good' science. See Chapter One for a more detailed discussion of these points.

14)For example, Michalos writes:
"In the worst of all possible worlds, every term, and therefore every report, would be theory-laden. So it would be impossible to administer an unbiased test of any theory."
A.C.Michalos, 'Philosophy of Science: Historical, Social and Value Aspects', in P.T.Durbin (ed) A Guide to the Culture of Science, Technology and Medicine (London: MacMillan, 1980), 197-281, quote at 231.
Historically, the existence of mutually exclusive, incommensurable theories in science is by no means unknown. Michalos seems to feel that it is possible to break out of this theory-laden universe by referring to 'facts', a term which he employs unproblematically:
"When psychologists test alternative theories about the behaviour of rats....there is typically no question about the observable facts....The whole point of the exercise is to try to provide a satisfactory account of the facts. If the facts changed to suit every theory, the exercise would effectively lose its point. Similarly, demographers, epidemiologists, criminologists and geographers typically have to dip into the very same pool of statistical time series - i.e. facts - in order to test, confirm, or disconfirm their theories." (ibid., 231)
Not only does this account neglect empirical evidence about the

multiplicity of ways in which 'raw data' can be interpreted, but by listing a number of disciplines which use the 'same' facts in very different ways, it points very clearly to the ways in which facts are mediated by social processes. In his next sentence, Michalos virtually completes the case in favour of relativism:
"Moreover, becoming a demographer, et cetera, implies learning to interpret such common pools of facts in the proper ways." (ibid., 231.)

15)H.M.Collins and R.G.Harrison, 'Building a TEA Laser: The Caprices of Communication', Social Studies of Science Vol 5 (1975), 441-50; H.M.Collins, 'The TEA Set: Tacit Knowledge and Scientific Networks', Science Studies Vol 4 (1974), 165-85; Collins (1975) op.cit. note 11; Collins (1981) op.cit. note 11; H.M.Collins, 'Upon the Replication of Scientific Findings: A Discussion Illuminated by the Experiences of Researchers into Parapsychology' Proceddings of the 4S/BSA Conference on Social Studies of Science, Cornell University, November 1976 (unpublished mimeo, University of Bath).

16)Figure 1 is taken from Collins (1976), op.cit. note 15.

17)Collins (1975) op.cit. note 11, quote at 220.

18)Collins (1976), op.cit. note 15.

19)Although Figure 1 represents the extreme case of open-ended controversy, it should be stressed that this is an idealized case. Most controversies are much less symmetrical than this figure would suggest. In particular, it should be noted that the high-flux gravity wave case study, discussed by Collins, does not really fit Figure 1 because, according to contemporary theory-based expectations, gravity waves (in the great quantities implied by Weber's claims) were not plausible.

20)Collins (1975), op.cit. note 11, quote at 220.

21)H.M.Collins, 'Stages in the Empirical Programme of Relativism', Social Studies of Science Vol 11 (1981) 3-10, quote at 4, emphasis in original.

22)This rule should always be applied tentatively, and should not be used to draw epistemological conclusions. In a footnote in Chapter One, I criticised Collins for claiming that we could use the concept of 'incorrect method' if all actors agreed that the method was incorrect. I argued that consensus has no special epistemological status within a relativist analysis, so that we should avoid evaluative terms such as 'incorrect' in these contexts.

I may seem to be falling into the same trap here. However, I would stress that the identification of 'raw data' is always tentative and contingent on the actual course of the debate. There is, in principle, no aspect of the data which could not be made into a point of dispute. Thus, 'raw data' may not always be raw. Nevertheless, at any point in time, some aspects of the data will be uncontentious. These aspects, while they remain uncontentious, act as boundaries within which negotiation takes place. As we have seen in Holt's case, this still leaves plenty of scope for manoeuvre for the 'determined advocate of hidden variables'.

23) T.S.Kuhn, The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1970).

24) Wynne has suggested that Barkla's 'deviant' theories were tolerated by the physics community, rather than made the focus of controversy, because of Barkla's prestige. Like myself, Wynne emphasises the existence of a range of reactions to deviance, from simple admission of error to full-scale controversy. He argues that our accounts of disputes must be flexible enough to encompass all such possibilities:
"It is important to treat the patterns of social control in science as flexible from situation to situation, and to be sensitive to wider contextual factors which influence social control as a process of interaction between groups or individuals."
B.Wynne, 'Between Orthodoxy and Oblivion: The Normalisation of Deviance in Science', in R.Wallis (ed), On the Margins of Science: The Social Construction of Rejected Knowledge, Sociological Review Monograph No. 27 (Keele: University of Keele, 1979), 67-84, quote at 69.

25) In their 'anthropological/ethnomethodological' study of biochemists, Latour and Woolgar present a very chaotic, rapidly-changing picture of science. See B.Latour and S.Woolgar, Laboratory Life (Beverley Hills: Sage, 1979). Latour has written specifically on the issue in question here. See B.Latour, 'Is it Possible to Reconstruct the Research Process?: Sociology of a Brain Peptide', in K.D.Knorr, R.Krohn and R.Whitley (eds), The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook, Vol 4 (Dordrecht: Reidel, 1980), 53-73. However, the particular scientific activity under study here seems *atypically* open: experimenters in principle could choose to produce any of $2{\cdot}6 \times 10^{22}$ substances, and their choice was, to a considerable extent, arbitrary. Each new substance led them to redefine their strategy in order to give their choice, and their past choices, a gloss of rationality. Areas such as LHV, where experiments take over a year to complete, seem almost static by comparison.

26) Compare Collins (1975), op.cit. note 11, and Collins (1981), ibid.

27) The undeniably special *sociological* status of science (as a professionalized body and as our society's major arbiter of valid knowledge) has often been associated with epistemological demarcation criteria such as falsifiability and rationality. One does not have to be a relativist to question such associations. For an intermediate view of the relationship between science and other forms of belief, see Robin Horton, 'African Traditional Thought and Western Science', in M.F.D.Young (ed) Knowledge and Control: New Directions for the Sociology of Education (London: Collier-MacMillan, 1971), 208-66.

28) Although in this thesis I have discussed plausibility only in qualitative terms, it should be possible in principle to carry out a meaningful quantitative (or at least systematic)

assessment of plausibility by sociometric means.

29) As this thesis was being typed, Aspect's experimental report, cited as a preprint in Chapter Six, appeared in print. See A.Aspect, P.Grangier and G.Roger, 'Experimental Tests of Realistic Local Theories via Bell's Theorem', Physical Review Letters Vol 47 (1981), 460-3.
This experiment, which agreed with QM but which did not include a test of the 'timing hypothesis', was reported in The Times (28th August 1981, p 2) under the heading 'Random events overrun Einstein'. (I am grateful to David Edge for this reference). This article, attributed to 'the Staff of Nature', makes the fairly common mistake of failing to distinguish between local and non-local hidden variables. Thus, the authors imply that a refutation of LHV means that "there can be no hidden variables to account for the randomness of QM". In addition, the authors misunderstand the implications of this experiment for the timing hypothesis; they claim that this result rules out all HVTs which obey the theory of relativity. Certainly, non-local HVTs seem to conflict with relativity, but until the timing hypothesis is tested, it remains possible that signals are transmitted at speeds less than that of light, causing no conflict with relativity.

An article in The Times cannot be considered to represent the reaction of the physics community as a whole. It is too early to judge the reaction to Aspect's result among more informed physicists. Nevertheless, it would be quite in keeping with my own account to find that Aspect will be credited with refuting the timing hypothesis without having even tested it!

Appendix A

The Questionnaire

NOTE: Throughout this questionnaire, all work dealing with criticism of the 'orthodox interpretation' of quantum mechanics, counter-proposals, and objections to such counter-proposals, is for convenience defined as 'work on the interpretation of QM'.

It would be very useful for us to know details of the careers of physicists who have worked on the interpretation of QM. Accordingly, we would greatly appreciate it if you could send a curriculum vitae and a bibliography of your published work when you return this questionnaire.

To make our survey more comprehensive, we wish to learn of as many of the people in this field as possible. Also, one of the aspects we are studying is the communication between people working in this area. For both these reasons, we would be very grateful if you could also send a list of people to whom you send preprints or offprints, and, if possible, a list of those from whom you receive such material.

SECTION ONE: Biographical Details

1) How did you first become interested in the problems of the interpretation of QM?

(Please tick one or more of the options)

Talking to colleagues ☐

Influence of a supervisor or teacher ☐

Reading papers in specialist journals ☐

Reading less specialist books or journals ☐

Reading popular scientific books and journals ☐

Reading books or journals in the philosophy of science ☐

Attending a conference ☐

Other (Please specify)

....

If you can recall a single person or event as giving an important stimulus, please give details:

2) /

L66

2) At what stage in your career did you first take an interest in work on the interpretation of QM?

(Tick one)

Before university/college ☐

As an undergraduate ☐

As a graduate student ☐

As a professional scientist. ☐

Other (specify) ....

3) At any stage of your career, have you been a student (undergraduate or graduate) of other physicists with an active interest in these problems?

Yes ☐

No ☐

If yes, please give details:

4) At any stage in your postgraduate career, have you worked in the same department, or in close proximity, with such physicists?

Yes ☐

No ☐

If yes, please give details:

5) In which areas have you worked? (Please tick one or more.)

Local hidden variable experiments	☐	Local hidden variable theory	☐
Nonlocal hidden variables	☐	Hidden variables in general e.g. 'impossibility proofs'	☐
Statistical interpretation	☐	Quantum Logic	☐
Measurement Theory	☐	Philosophy of QM	☐

Others (specify) ....

What is your main interest in this field?

6)/

6) How much of your professional time is taken up with work on the interpretation of QM?

(Tick one only)

All of it, i.e. full-time ☐

A major part (more than 50%) ☐

A minor part (less than 50%) ☐

Only an occasional paper, but keeping up-to-date in reading ☐

A paper or two some time ago, and since then have neglected the field. ☐

7) Is this proportion changing as time goes on?

Yes ☐

No ☐

If no, go on to Question 10.

8) How is this proportion changing?

Increasing ☐

Decreasing ☐

9) Please explain why this is so (e.g. change of interest, other commitments, completion of an experiment, etc.)

10) Apart from the interpretation of QM, what are your professional interests?

11) Have you ever taught an undergraduate or postgraduate class in quantum mechanics?

Yes ☐

No ☐

If yes, go on to Question 13.

12)/

12) If no, how would you deal with problems of interpretation if you were to teach such a class?

Ignore them ☐

Mention them in passing ☐

Stress their importance ☐

Offer a separate course on them ☐

Do you have any other comments about the teaching of QM?

Now go on to Question 15.

13) Which textbooks do you use?

Are there any textbooks which you dislike and would definitely not use?

What do you object to in these textbooks?

14) In your teaching, how do you deal with problems of interpretation of QM?

Ignore them ☐

Mention them in passing ☐

Stress their importance ☐

Offer a separate course on them ☐

Do you have any other comments about the teaching of QM?

15)/

15) Have you ever supervised graduate students whose research has been on the interpretation of QM?

Yes ☐

No ☐

If yes, may we contact you for further details?

Yes ☐

No ☐

SECTION TWO: Your impression of the work done in this field, and of the people who do it.

16) It has been suggested that "a growing number of physicists today declare themselves to be dissatisfied with the interpretation of the Copenhagen School". (Recherche, 5, p. 653, 1974)

Do you personally feel that the number of physicists interested in this field is increasing or decreasing?

Increasing ☐

Decreasing ☐

Constant ☐

Please give a brief explanation for this phenomenon.

Do you know of any evidence, numerical or otherwise, of this trend?

Yes ☐

No ☐

If yes, may we contact you for further details?

Yes ☐

No ☐

17)/

17) In your opinion, what does the physics community in general think of this field? (e.g. 'a waste of time', or 'interesting but irrelevant' or 'very important', etc.)

Do you have any evidence (reported comments, correspondence, etc.) on this issue?

Yes ☐

No ☐

If so, may we contact you for further details?

Yes ☐

No ☐

18) Do you think that the physics community in general is more, or less, interested in problems of interpretation than it was 15 years ago?

More interested ☐

Less interested ☐

Interest unchanged ☐

Why do you think this is so?

19) We would like to investigate the factors which motivate people to work in this field. How important are the following ideas as motivating factors in your work?

(Tick one column in each line)

	Strong motive	Some effect	No effect
The need for a homogeneous account of the world (i.e. classical/quantum barrier removed)			
The 'paradoxes' in quantum measurement theory (e.g. Schrödinger's Cat)			
The 'incompleteness' of QM. as discussed by Einstein, Podolsky and Rosen			
The loss of determinism in QM			
The loss of locality in QM			
The existence of dogmatism among some quantum theorists			
The possibility of doing interesting experimental work			
The possibility of doing interesting theoretical work			
The possibility of working with a certain person (viz:)			
The chance of gaining recognition by doing good work			

Others (specify):-

20) Please list 5 papers which you would recommend as the best way of introducing a newcomer to the main ideas in this field.

21) Please list the 5 papers which, in your opinion, have stimulated the greatest interest in this field.

22)/

22) Please list the 5 men who, in your opinion, have contributed most to work on the interpretation of QM.

23) Do you feel that your opinions have changed since you first became interested in the interpretation of QM?

Yes ☐

No ☐

If yes, please explain the way in which they have changed, and the reasons for the change.

24) Do you feel that quantum mechanics has any relevance for the study of parapsychology?

Yes ☐

No ☐

If yes, may we contact you for further details?

Yes ☐

No ☐

25) Have you any knowledge of the experiments performed since 1971 to test for the existence of local hidden variables (LHV's)?

Yes ☐

No ☐

If no, go on to question 26.

How/

25) How much do you know about the experiments of:
(contd.) (Tick one column in each line.)

	Detailed knowledge	Knowledge of main techniques used	The result only	No knowledge
Holt				
Clauser				
Freedman & Clauser				
Fry & Thompson				
Faraci, Gutkowski, Notarrigo & Pennisi				
Kasday, Ullman & Wu				
Aspect				
Others (specify)				

Were you surprised by any of the results actually obtained?

Yes ☐

No ☐

If so, which and why?

Holt and Faraci et al disagree with the other results. Please comment on the following statements:

(Tick one column in each line)

	Agree	Disagree	Don't Know
Holt's result is less likely to be true than Clauser, Freedman, Fry and Thomspon's			
Faraci et al's result is less likely to be true than Kasdny et al's.			
On the whole, the results refute the existence of LHV's			
On the whole, the results prove the existence of LHV's			

The/

25) contd.

	Agree	Disagree	Don't Know
The results lead to no definite conclusion and further experiments are required			
Aspect's experiment with timing will finally settle the issue			
Experiments with timing and ideal detectors would settle the issue			
Our task now is to develop new concepts and a new language to describe the world in non-local terms			
The non-local correlations violate relativity theory by suggesting instantaneous reduction of the wave packet			
There is no clash with relativity because nothing physically real is transmitted instantaneously			
At larger separations, the correlations may give the LHV result			
QM has more problems now than before these experiments were done			

Other comments:-

26) Have you any knowledge of Papaliolios' experiment to test the Bohm-Bub theory?

Yes ☐

No ☐

If no, go on to Question 27.

26. (contd.)

If yes, please comment:

	Agree	Disagree	Don't Know
This experiment disproves the Bohm-Bub model			
This experiment sets a limit on the relaxation time			
This experiment is irrelevant because the relaxation time value is not crucial to the model			
This experiment is irrelevant because of problems with identity of photons			

Other comments on this experiment:-

27) Have you ever attended conferences or meetings to discuss the interpretation of QM?

Yes ☐

No ☐

If yes, please give details:

28) Are there any people with whom you frequently communicate or used to communicate on this subject e.g. by letter or telephone, or in person?

Yes ☐

No ☐

If so, please give details:

Is/was this communication:

To exchange information on experimental techniques ☐

Involved with writing a joint paper ☐

Other (specify)

SECTION THREE: Funding and Publishing, and difficulties encountered.

29) Have you ever applied for a grant or other finance specifically to do work in these areas?

Yes ☐

No ☐

If so, may we contact you for further details?

Yes ☐

No ☐

30) Do you agree with one physicist's claim that "It's no more difficult to get funding for this work than for work in other areas"?

Agree ☐

Disagree ☐

Don't Know ☐

If you disagree, do you have any evidence for this, from your own experience, or the experience of others?

Yes ☐

No ☐

If so, may we contact you for further details?

Yes ☐

No ☐

31) Do you think that the rejection rate for papers in this field is higher than average?

Yes ☐

No ☐

If yes, why do you think this is so?

Larger proportion of 'crank' papers ☐

The need to keep standards high to improve the reputation of this field ☐

Editors and referees are unsympathetic ☐

Other (specify)

Have/

31) (contd.) Have you yourself, or anyone you know, had such a paper rejected for what you feel are poor reasons?

Yes ☐

No ☐

If so, may we contact you for further details?

Yes ☐

No ☐

32) If you were looking for recent papers in this field published in physics journals, which journals would you consult? Please list 5 journals in order of the likelihood of finding such an article published therein:

1

2

3

4

5

33) In 1962 David Bohm expressed the view that "some consideration of theories involving hidden variables is at present needed to help us to avoid dogmatic preconceptions". Other people have claimed that the "orthodoxy" of physics is intolerant of criticism and that unorthodox work is unwelcome and discouraged. Do you feel that physicists who hold unorthodox views about QM have any great difficulties in carrying out their work?

(Tick one only.)

No more than usual ☐

Some more, but nothing drastic ☐

Exceptional difficulties ☐

34) In your opinion, if a young physicist begins his professional life by working in this field, can his career be hampered?

35)/

35) Do you have any evidence of dogmatism or other difficulties faced by physicists who are critical of "orthodox" quantum mechanics?

Yes ☐

No ☐

If so, may we contact you for further details?

Yes ☐

No ☐

36) "It has been claimed that even the most 'Progressive' theoretician believes at the bottom of his heart in a strictly deterministic, objective world even if his teachings categorically deny such a view." (Max Jammer) Others have claimed that the Copenhagen Interpretation is positivistic and does not permit the idea of an objective reality. Do you feel that one's stand on such philosophical issues influences one's opinion about the interpretation of QM?

(Tick one only.)

Philosophical position is a crucial factor ☐

Philosophical position is an important factor ☐

It is a possible factor ☐

One's philosophical stand has no connection with the physical theories one believes ☐

Please explain briefly why you hold this view:

37) In the controversy over hereditary and environmental influence on intelligence, it has been suggested that one's political viewpoint correlates strongly with one's stand on this issue. Do you feel this has any truth in quantum mechanics?

Strong correlation between stand on QM and political stand ☐

Possibly some correlation ☐

No connection at all ☐

Please/

37) (contd.) Please explain briefly why you hold this view:

Do you have any evidence (e.g. correspondence) for this view?

Yes ☐

No ☐

If so, may we contact you for further details?

Yes ☐

No ☐

38) What has been achieved by work done on the interpretation of QM?

(Tick one or more)

Positive contribution to physics ☐

Negative results but useful analysis ☐

Confusing proliferation of theories ☐

Little or no worthwhile results ☐

Any other comments:

39) Lastly, are there any changes you would like to make to the present system of funding, publishing, refereeing, etc., which you think would be an improvement?

Yes ☐

No ☐

If yes, please give details of the proposed changes and your reasons for suggesting them.

Thank/

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Thank you for your cooperation. May we remind you of our request for a curriculum vitae, bibliography, and preprint names and addresses lists. We hope that you have not been inconvenienced by this survey, and that perhaps you found it interesting. It will certainly be of great help for our work.

NAME ....

Appendix B

Details of Interviews

Interviewee	Location	Date
J.S.Bell	Edinburgh	March 4th 1976
J.F.Clauser	Erice, Sicily	April 20th 1976
E.S.Fry	"	"
F.M.Pipkin	"	"
A.Aspect	"	April 22nd 1976
E.Fitchard	Stirling	April 14th 1977
J.Dorling	London	July 4th 1977
D.Bohm	"	July 5th 1977
B.J.Hiley	"	"
D.O'Brien	Dundee	July 11th 1977
T.Tonietti	Edinburgh	August 19th 1977
L.Kasday	New York	September 20th 1977
D.Scarl	Farmingdale, NY	September 21st 1977
A.Komar	New York	September 23rd 1977
A.Petersen	"	"
D.Finkelstein	"	"
J.Ullman	"	September 24th 1977
M.Bunge	Montreal	September 26th 1977
D.Blohm	"	"
R.Holt	London, Ontario	September 28th 1977
C.Hooker	"	September 29th 1977
W.Demopoulous	"	"
J.Bub	"	"
R.Stairs	"	"
S.Prugovecki	Toronto	September 30th 1977
P.Pearle	Clinton, NY	October 4th 1977
M.Sachs	Buffalo, NY	October 7th 1977
C.Smith	Philadelphia	October 9th 1977
S.Kochen	Princeton, NJ	October 10th 1977
S.Reynolds	"	October 11th 1977
E.Wigner	"	"
E.Nelson	"	"
F.M.Pipkin	Cambridge, Mass.	October 13th 1977
M.Horne	"	"
A.Siegel	Boston	October 14th 1977
C.Lo	"	"

Interviewee	Location	Date
H.Margenau	New Haven, Conn.	October 17th 1977
E.Fry	Ann Arbor, Mich.	October 19th 1977
J.F.Clauser	Livermore, Calif.	October 31st 1977
S.Freedman	Stanford, Calif.	November 1st 1977
C.Papaliolios	Tucson, Ariz.	November 7th 1977

Harry Collins and Trevor Pinch also interviewed the following scientists on my behalf, using lists of questions which I had prepared: J.F.Clauser, S.Freedman, N.Herbert, J.Sarfatti, P.Werbos and O.Costa de Beauregard.

Andy Pickering also interviewed John Bell on my behalf, again using a list of questions which I had prepared.

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