

THE EFFECTS OF SOCIAL CONTEXT ON THE PROCESS OF SCIENTIFIC INVESTIGATION: EXPERIMENTAL TESTS OF QUANTUM MECHANICS

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Introduction

This paper deals with a case-study from the recent history of physics, and the general aim of the paper is to illustrate the role of a wide variety of factors, other than purely internal technical factors, in determining the outcome of this piece of scientific practice. My approach is what has been described as 'naturalistic' (1); that is, I do not wish to presuppose the existence of a code of conduct, or a set of norms, which is sufficient to account for the social behaviour of scientists. (Mulkay (2) provides a powerful critique of such presuppositions.) By the same token, a sceptical attitude will be taken towards scientists' *own* accounts for their actions; that is, such accounts will be treated as data rather than as complete explanations of behaviour.

In order to identify the effects of social context on scientific investigation, at the 'micro-level' of the individual scientist and the reception of his work, it is necessary to look in detail at the particular situation in which that scientist finds himself. Knorr has explicitly recognised the context-dependent nature of scientists' strategy, in deciding such things as what topic to study and how to present their results. Instead of postulating a specific code of behaviour, Knorr argues that scientists are fundamentally concerned with 'success', a concept similar to what some other authors have called 'making out'. The type of success desired, Knorr claims, depends entirely on the local context of the scientist:

What counts as success is determined by the field and by the agent's position in the field . . . the notion of success is an *indexical* expression which refers us to the context of a local, idiosyncratic situation . . . success is *by and for* an agent at a particular *time and place*, and carried by local *interpretations*. (3)

This view suggests that the social and cultural, as well as the technical, context in which a scientist finds himself will influence not only the style, timing and presentation of his work but also (at least in principle) its content.

Although these views clearly open up a whole realm of possible enquiry, they also raise what seems to be a serious methodological issue. Having cast doubt on the accounting procedures of scientists, what are we to make of our own data, when we read scientists' writings and talk with them in interviews? For example, if a scientist tells us that he did X because he thought it would lead to Y, this 'reason' is itself an account, given in the (rather artificial) context of an interview and no doubt framed within the requirements of that context. It may be interpreted as a legitimisation, a rationalisation, or a rewriting of history by hindsight. The same question asked by the scientist's colleague in an informal context, or asked by the interviewer some years hence, may well yield a different answer. Which is the 'real' reason?

There are at least two ways to avoid this problem. One is to concede that there are no real reasons, and that sociologists themselves are simply in the business of generating accounts according to their own criteria of reasonableness. Another, and to my mind more satisfactory, reply is to appeal to our own skills, as sociologists and as inhabitants of the everyday world, in interpreting other people's statements. Unless we accept a rather extreme solipsism, we are led to believe that it is possible to make sense of the world. True, the work of ethnomethodologists (4) suggests that this 'making sense' is itself a complex interpretive process — an active accounting procedure — and thus threatens to return us to the position discussed above. But at the very least, this work also points out that the problem of interpretation is not ours alone. I am well aware of the fact that I have not solved the problem. Nevertheless, it is important that we are aware of its existence. At least this awareness may stimulate our critical faculties. In the last analysis, of course, it is the reader who must judge the plausibility of my own account in what follows.

In this case-study, I shall try to make two major points. I shall first provide evidence in favour of Knorr's notion of the indexicality of success. By examining a group of physicists who became involved in a particular topic, I shall argue that these physicist's aims and motivations can only be satisfactorily accounted for if we include detailed discussion of their local social context.

Secondly, and perhaps more importantly, I shall examine the response to

one particular physical experiment, both by the experimenter involved and by his colleagues within the speciality. This experiment produced rather surprising results which are now interpreted as straightforward error. What I hope to show is that this interpretation was by no means an abstract logical deduction. Instead, it came about by a complex interplay of psychological, social and technical factors. In order to account satisfactorily for this small episode in the recent history of science, I believe that we must take note of the particular context in which this experiment took place, and in which the experimenters' notions of 'success' were framed. In the last section of this paper, I shall discuss the possibility that the very content of scientific knowledge may be affected by the social context in which this knowledge was generated.

The Background to the Experiments

My case-study concerns a series of experiments which were performed over the last ten years, and which were designed to test the theory of Quantum Mechanics. Quantum Mechanics (hereafter QM) could fairly be described as a cornerstone of modern physics. Its impact and scope has been characterised in the following terms:

Never in the history of science has there been a theory which has had such a profound impact on human thinking as QM; nor has there been a theory which scored such spectacular successes in the prediction of such an enormous variety of phenomena (atomic physics, solid state physics, chemistry, etc.). Furthermore, for all that is known today, QM is the only consistent theory of elementary processes. (5)

Despite its great empirical success, many of the philosophical implications of QM have been a source of disquiet to some physicists, theologians, and philosophers since the development of QM in the 1920's. Some of these issues, such as the wave-particle duality of matter and light, and Heisenberg's uncertainty or indeterminacy principle, are well-known. Although the mathematical *formalism* of QM led to no contradiction with experimental evidence, the *interpretation* of that formalism was for some years a major focus of dissension.

In a series of debates with Neils Bohr, Einstein raised a number of objections to QM; in the course of answering these objections, Bohr developed

what has come to be known as the Copenhagen Interpretation of QM (so called because Bohr and many of his colleagues worked in Copenhagen). Jammer writes:

As is well known, this interpretation is still espoused today by the majority of theoreticians and practicing physicists. Though not necessarily the only logically possible interpretation of quantum phenomena, it is *de facto* the only existing fully articulated consistent scheme of conceptions that brings order into an otherwise chaotic cluster of facts and makes it comprehensible. (6)

Nevertheless, a very large number of alternative interpretations of QM have been proposed. Different authors have perceived different philosophical flaws in QM, so that there is an enormous diversity of approaches towards reinterpretation (7).

The aspect of QM which is directly relevant to the present context is known as 'non-separability' (8). For certain physical systems, such as a pair of particles which are created simultaneously, QM provides predictions concerning the physical parameters of the system as a whole (e.g. its total mass or momentum) but provides very limited information about the properties of each individual particle. When Einstein raised this issue in 1935, arguing that QM therefore provided an incomplete description of reality, Bohr replied that for any real experiment, QM provides a full description of all that we can measure, and that it is meaningless to discuss the values (or indeed the existence) of properties which we cannot measure.

It is possible to frame Einstein's objection to QM in terms of a rival theory, which postulates the existence of 'hidden variables'. These variables would fully characterise those properties of individual particles which Bohr declared to be meaningless. Since these variables permit the analytical separation of the quantum-mechanical 'global' two-particle system into its constituent parts, they are termed 'local hidden variables' (hereafter LHV). Physicists disagree over whether Einstein had this specific sort of theory in mind, but in 1964 J. S. Bell did construct such a theory (9). More importantly, he showed that although the LHVs could not be detected directly (hence their name), they led to predictions which differed in some small respects from those of QM; specifically, QM and LHV predict slightly different correlations between the properties of the constituent parts of a physical system, such as the spin correlation of atomic particles, or the polarisation correlation of photons

(10). Thus, for the first time (11), it seemed possible, at least in principle, to perform an experimental test of a proposed alternative to QM, rather than to limit the evaluation of this alternative to philosophical or mathematical analysis. Since then, a number of such experiments have been performed. The physicists involved in this work are the subjects of this paper.

Effects of Social Context: Who Became Involved with LHV and Why?

To answer such a question fully would require unattainably detailed knowledge about every facet of the individuals concerned. Indeed, as pointed out earlier, one may question the whole concept of 'real' reasons. Nevertheless, I shall try to show that the social *and* technical context of the LHV activity played a major part in selecting these individuals.

Let us examine some of the salient features of this context. In the first place, this work was experimental, and was thus different in kind from all other work on the reinterpretation of QM. Secondly, despite its possible philosophical significance, LHV could hardly be described as a highly *plausible* theory. Although the great success of QM over the past 50 years did not rule out the possibility that LHV was correct (since the theories agree in all but a few of their predictions, and no experiment had thus far probed the area of conflict) nevertheless the likelihood of falsifying QM in these experiments was not perceived by anyone involved to be very great.

This implausibility was not restricted to LHV. Nearly all the work then being done on the philosophical implications of QM, and the numerous attempts to reinterpret the fundamental basis of the theory, were seen by many physicists as having little value. For example, one author, who was himself sympathetic to such investigations, and who had made an important study of the 'measurement problem' (a major focus of interest among students of QM) prefaced his comments by noting that

the problem of measurement in QM is considered as non-existent or trivial by an impressive body of theoretical physicists. (12)

Factors such as these are highly relevant; they determined, to a very large extent, the sort of person (in psychological and sociological terms) who was likely to become involved with LHV. In the first place, the experimental nature of the work excluded a number of people who had a long-standing

interest and involvement in the philosophical and theoretical analysis of QM. Such people had neither the necessary technical training, nor access to the relevant experimental equipment. This in turn meant that these people were faced with an intrusion, into their area of interest, of a new group, working with a new methodology, and obtaining results which philosophers were not fully equipped to discuss (13).

In the second place, again because of the nature of this work, interested physicists had to exert considerable efforts in order to obtain funds and specialised apparatus. In particular, one group of theoreticians had to 'draft in' an experimentalist who would not, by himself, have performed such an experiment. This event is very relevant to my next section, since the results of this experiment strongly disagreed with QM, a most surprising event. It is therefore worth quoting at length from a theorist's account of his actions:

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?' ... I called *P* to discuss the problem ... he said, we have an apparatus at our university 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 ... We explained what was going on, why their apparatus was useful ... We had to do a lot of explaining of the motivation for our experiment ... this type of thing was very far from the concerns of Pipkin and Holt 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. (14)

Clearly, access to the necessary apparatus was an important factor here, and Holt would never have become involved but for this fortuitous coincidence. Holt's recollection of his own early feelings corroborate this view:

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

A third important factor was the low status of 'foundations' work. Many of the experimenters claimed that this constituted a severe problem:

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.

Ph.D 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.

I do not claim that the LHV physicists were severely persecuted for daring to question the validity of QM. After all, the experiments themselves were *technically* respectable, and the LHV hypothesis had been advanced by a respectable theorist, based at a high-status institution (CERN). One experimenter summed up the position like this:

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 some people who *could* 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.

Clearly, there was no grand conspiracy to suppress these experiments. Nevertheless, I believe it would be fair to say that the LHV physicists deliberately chose to enter a field which was of low prestige compared with other fields, and which was seen as very unlikely to produce surprising or informative results. Evidently, *some* factors must have overridden these disincentives.

A rather facile way of accounting for the involvement of these physicists would be to say that they simply found the topic interesting. Certainly, one would be surprised to find people voluntarily working on a topic which they did *not* find interesting. Indeed, I found in interviews that nearly everyone involved spent some time explaining their involvement in terms of interest. 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 is inadequate. It leaves untouched the notion that scientists are morally superior disinterested seekers after truth. It may be considered slightly sordid to account for behaviour in any other terms, such as self-interest, but I do not think we can ignore other factors.

For example, it seems highly significant that nearly all of the experimenters were Ph.D or postdoctoral students. Scientists in this position may have some special criteria for 'success' not shared by more established scientists. To some extent, their choice of thesis topic is a means to an end; namely, being awarded a Ph.D. None of my interviewees felt that their LHV work had 'type-cast' them. Indeed, they have now gone on to work in fields as diverse as atomic physics, laser fusion, and the psychology of perception (15).

This would appear to leave the field free for a decision in terms of interest alone. Here again, however, we must be cautious. Other criteria are also relevant. For instance, a Ph.D student requires a topic which is not beyond his technical competence, yet which allows him to develop and improve his range of technical skills, and so enhance his future employment prospects. He requires a topic which will yield worthwhile non-trivial 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 desirable. The work is technically feasible, yet it 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 collaborations involving 20 or 30 people, in sharp contrast to fields such as high-energy physics. These factors seem to have been important for the LHV physicists. Their comments, quoted below, portray them as an opportunistic group, for whom an interest in QM may have been a necessary, but by no means a sufficient condition of their involvement:

I was in nuclear physics . . . *I was looking for a thesis project.* 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. *It was just luck* that I was free at the time. Otherwise I'd have done a weak interaction experiment.

I was looking for a postdoc position, or someplace to go when I finished my thesis in astrophysics, and I wanted to do something in the foundations of QM, although I didn't really have anything in mind until I read about LHV.

(Emphasis added in all three quotations.)

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.

The factors discussed above (16) can be subdivided into several groups; each type of factor played some part in selecting the particular group of people who did perform LHV experiments, while at the same time excluded other people. Thus there are institutional factors (access to funds and equipment), technical factors (possession of the necessary experimental skills), theoretical factors (the status of HV theories and the recent history of QM) and sociological factors (the special position of Ph.D students and the criteria by which Ph.D topics are chosen). It is necessarily more difficult to locate people who were *deterred* from doing an experiment because of these factors, since by definition they are unlikely to appear in the LHV literature. However, I do know of several physicists who, at one time, planned to perform experiments, but gave up the idea because of lack of access to the necessary apparatus (17), or became involved with other commitments.

In principle, it would be possible to predict the sort of person likely to become involved with any given topic, by identifying the social and technical factors which select such people. In practice, the complexity of most social contexts makes this difficult, and the analysis must, for now, be retrospective. Nevertheless, such an analysis supports Knorr's general contention that scientists are motivated by a whole series of issues which impinge on their local situation.

Connections can also be made between the social context of LHV and other features of the LHV activity. For example, let us consider the way in which the LHV experimenters presented their work in papers addressed to a wider audience. Much stress is placed on the differences between this work and virtually all previous work on the interpretation of QM: the latter is described as "a vast inconclusive body of literature" (18) and "the concern of philosophy of science rather than of actual physics" (19), whereas the LHV

work constitutes “a dramatic change in the state of affairs” (20) and “a decisive experimental test of the entire family of LHV theories” (21). Certainly, the LHV work was different; however, it seems clear that references to ‘decisiveness’ were used at least partly as a rhetorical device, serving to distance the experiments from the earlier low-status work. The experimenters themselves recognise that their results are in fact less than completely decisive, since a number of assumptions had to be made in order to compensate for the limitations of the available apparatus (22). In fact, the LHV group took great pains to spell out and analyse these assumptions. Such rigour is admirable, but again it cannot be wholly isolated from the context; as one experimenter put it: “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.”

Effects of Social Context: The Response to Anomalous Results

In this section, I wish to advance a stronger claim: namely, that the context of these experiments had an important bearing on the manner in which the results of the experiments were interpreted. I shall focus on one experiment, performed by Holt, which apparently contradicted QM, and I shall argue that the response to this experiment, both by Holt and by others, cannot be explained solely in terms of technical factors.

Holt’s experiment was similar in design to another experiment, performed by Freedman and Clauser (23), which was completed a few months prior to Holt’s, and which 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 rather hoped to falsify QM. 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?

Holt did not respond to his own result in this straightforward empiricist manner. Apparently he had strong reservations about the validity of his results as soon as they began to appear. Clauser and Freedman published their results, whereas Holt did not — nor did he try to. Holt, and his Ph.D

supervisor, Pipkin, took a long time to decide whether or not to submit their results to a journal.

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.

Their final decision was to produce an unpublished manuscript describing the experiment and the results (24).

In addition, although Holt's first results appeared in 1971, he did not submit his Ph.D thesis until April 1973, the intervening period being spent in a (fruitless) attempt to isolate a source of error in his 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. This would seem to be Holt's response to the different audiences and aims of these two accounts. He told me:

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 was 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 that 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.

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 virtually inevitable, 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 the 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.'

Holt also stressed the fact that he had performed two other experiments during his Ph.D research which had absolutely nothing to do with hidden variables.

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.

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. However, around 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 *X* 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.

Let me summarise the alternatives available to Holt. He could have publicly disowned his result, claiming that it was due to an error — but this would not reflect well on his competence, at a stage in his career when he was looking for employment as an experimental physicist. He could have simply presented his results as he found them, and moved on to a new field — but given the surprising nature of his findings, disbelief would have been inevitable. Could this Ph.D student really have found a flaw in QM, when another group, with a very similar experiment, claimed to have found none? The possibility of error in Holt's apparatus could not be overlooked. In addition, the assessment of the work by Holt's Ph.D committee also counted for a great deal. Even after his exhaustive search for errors, Holt still expected 'a hard time' from them.

Thus, Holt's actual response was clearly a sensible one. Faced with a result which he did not believe, his actions seemed to him to be a way of minimising any damage which could be done, and of optimising the outcome of what

could have been a very embarrassing episode. By showing that the error was persistent 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. 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 skeptical manner (25).

However, another option *was* available to him: he could have published his results, claiming or implying that they were valid. Undoubtedly he would have received a great deal of publicity for this, and *if* he were to be 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, Clauser and Freedman's result – all these factors strongly suggest that Holt's results were *not* likely to be corroborated by future experiments. In interview, Holt dismissed this possibility, claiming that he intuitively doubted his results. Given such doubts, there was little to be gained by sensationalism.

There was yet another reason for adopting a low profile: a reason which was particularly relevant for Holt's local situation. His 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. 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? His result was still on record, and was known to the small group of physicists actively involved with LHV. Why risk having to make a retraction at a later date of a result which he doubted anyway?

In the light of his social and cultural context, it seems clear that Holt did indeed behave rationally (26). But it is not my aim to assess the rationality of physicists. The important point to be gained from this discussion, I feel, is that unless we are prepared to consider the context in which behaviour occurs, we can neither fully appreciate that behaviour, *nor* can we perceive the possibility of alternative behaviour under different circumstances. At first sight, Holt's behaviour takes on almost an air of inevitability which, I would

argue, is not warranted. Faced with similar problems, other individuals might well have behaved differently. By identifying the factors which apparently guided Holt's actions, we simultaneously construct a set of parameters which may, in some sense, have played a part in determining the outcome. Different settings of these parameters may lead to different outcomes. I shall return to this point in my final section, which by necessity will be somewhat speculative.

But let us return to the *actual* outcome. As things turned out, Holt's virtual capitulation led almost inevitably to universal disbelief in his results. Over and over again I was told in interviews "Holt himself doesn't believe his result, and if the person who actually does the experiment doesn't believe it, why should I?" Scientific controversies, at least those most often studied by sociologists of science, are typically structured in terms of two opposing viewpoints; the combatants attack the work of their opponents, and defend their own work against such attacks. In so doing, arguments are elaborated, new experiments are performed, and a variety of resources are employed in an attempt to win over the scientific audience (27). By his choice of presentation, Holt virtually ensured that a controversy of this sort would not arise. What *was* the effect of Holt's result on the LHV group? Let us now turn to the response of *other* physicists to this anomalous result.

The Response of Other Physicists to Holt's Result

The discussion in the previous section might have led the reader to expect that Holt's result would have been totally ignored. However, this is not what happened. A number of physicists took an interest in Holt's work; some visited his laboratory to examine his apparatus, and some performed other experiments in an attempt to resolve the disagreement between the experiments of Holt and of Freedman and Clauser. At first sight, this may seem slightly odd. It is certainly true that the two 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 sort of reply would invoke the norm of 'organised skepticism' — i.e. this reply would argue that not only are scientists constitutionally skeptical about novel results,

but they are equally skeptical about any attempt to dismiss such results, even if the dismissal is made by the person who actually obtained these results.

There are several difficulties with this sort of explanation. For example, it does not account for the frequency with which, as already mentioned, Holt's own dismissal was cited by interviewees as a justification for their own rejection. In addition, if such a norm of skepticism were to be applied universally, it is difficult to see how scientists would find the time to generate new results, since all errors would have to be followed up by an independent observer. 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 limits on skepticism.

I wish to put forward an alternative explanation: namely, that belief or disbelief in Holt's result was by no means a crucial factor in the strategy of the LHV physicists. 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 is similar to the above explanation; both accounts rest on the premise that science is a puzzle-solving activity. 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 in a complex one, and I would argue that 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 this Ph.D might not have been awarded and his competence as a physicist might have been questioned. Why did *other* physicists choose to devote their time to this problem?

I believe that the answer is that they had nothing to lose and a lot to gain. They had nothing to lose because 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 experiments would be resolved. To the best of my knowledge, no-one involved in LHV favoured Holt's result, and if no further experiments had been done, it seems clear that QM would have been vindicated. Nevertheless, it cannot be denied that it would be more satisfying to have corroborating evidence.

A second factor was the credibility of the LHV enterprise. As stated earlier, some rather extravagant claims were made for these experiments. For the first time, a decisive answer to an interpretive problem would be available. Holt's result clearly does not go well with such claims. Clauser, at least, was apparently acutely aware of this fact. He investigated Holt's result by performing a similar experiment, whose results agreed fully with QM. Clauser explained 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.

A third function to be served by further investigation of Holt's result was that, like any challenging piece of work, it offered the chance to develop skills and techniques which could then be applied to any number of topics. To enlarge on this point, I shall discuss another of the LHV experimenters, Fry.

Fry first took an interest in LHV in 1970, soon after he heard about the first published proposal for an experiment. Together with a co-worker, he applied for a number of grants in 1970 and 1971 in order to purchase the necessary equipment. Unfortunately, they discovered that the apparatus they were able to obtain could not overcome certain technical problems (connected with the hyperfine structure of the mercury atom). In addition, the results of the first experiment, by Freedman and Clauser, appeared at this time. Both of these factors contributed to Fry's decision 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.

In fact, when Fry reapplied for funds in 1974, he was given money by an organisation which had rejected his application in 1970. Admittedly, his second application included substantial technical improvements, 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.

Holt's result, irrespective of whether Fry believed it to be valid, thus gave him the opportunity of negotiating for funds.

Holt's experiment provided a basis from which I could argue to get some more money.

I certainly do not wish to imply that Fry had no interest in the LHV issues other than 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.

Nevertheless, I think there is some basis for claiming that Holt's result was not a crucial part of Fry's motivation, but rather provided a resource by which he could fulfill some wider aims in his work. For example, in 1973 Fry published a paper (28) 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.

A further point: after publishing his experimental results, which were "in excellent agreement with the QM prediction . . . and in clear violation of the LHV restriction" (29), Fry made a further request 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. To quote Fry,

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.

Fry's improved experiment would be more satisfying from the point of view of experimental design, but it would not tell us anything we don't already know about the status of LHV theories. It therefore seems plausible to conclude that a major part of Fry's strategy was to develop experimental techniques *per se*, and then apply them to a number of quite different empirical problems.

A further piece of evidence is relevant. Shortly after completing his LHV experiment, Fry moved temporarily to another university in order to take part in another 'fundamental' experiment, this time to study parity violation. Like many of the LHV physicists, Fry expressed a *general* preference for such fundamental experiments rather than more routine work:

May people just go out and measure things . . . but I think it's much more exciting to do things like the hidden-variable experiment or the parity experiment.

It is important to remember that none of the LHV experimenters planned to spend their entire professional life doing LHV experiments. Indeed, many of them claimed that once it became clear that the results of the existing experiments favoured QM, it was difficult to imagine anything being gained by further tests. None of the numerous alternatives to the Copenhagen Interpretation of QM had led to realisable experimental predictions which could provide genuine tests of these alternatives. (This is also true of David Bohm's well-known HV theories — see Note 11.) Most of the experimenters therefore described their LHV work as "a one-off job". The LHV group were experimental physicists first and foremost, and hidden-variable investigators second. Their reference group, to whom they looked for recognition and future employment, was the mainstream physics community, and not the diffuse and fragmented collection of physicists and philosophers who spend a large proportion of their time on the interpretation of QM.

It is difficult to *disprove* 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. It is for this reason that I have taken the response to Holt's experiment as a problematic

outcome which requires explanation. Both Holt and the other physicists involved could have reacted differently, and I believe that it is only by reference to the particular context in which these people operated that we can provide a detailed account of their practice and of the actual outcome. To claim that different courses of action lead to different pay-offs may seem obvious. What I have tried to show here is that such pay-offs cannot be assessed solely by reference to general notions of scientific methodology; instead they are largely determined by the local context in which decisions are made.

Of course, it is by no means easy to give a *complete* description of the 'local context', and any particular reading of this context, such as my own, could always be challenged. But this is no reason to give up all attempts to understand the detailed mechanics of scientific practice; nor does it undermine the validity of the general claim that such practice is heavily influenced by non-technical, and specifically social, factors.

Social Context and the Creation of Scientific Knowledge: Some Tentative Comments

By this point, I hope to have convinced the reader that the particular social, historical and cultural context in which the LHV experiments took place had a major effect on many features of these experiments – their location, the people who performed them, the way in which they were presented, and the response to anomalous results, both by the originator of these results and by other LHV physicists.

The real question, of course, is whether or not all this is ultimately trivial. Could the outcome of the experiments – the data themselves and the conclusions drawn from them – be affected by anything other than technical criteria?

Ultimately, the answer depends on what one regards as trivial. For example, if Holt's experiment had been performed by a person of higher status, or by someone who aggressively defended its validity, this might have no effect at all on the data obtained in subsequent experiments, but it might well have led to a quite different style of debate and a different sequence of events. It is interesting to speculate about what might have occurred 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.' This in itself might have made a substantial difference to the outcome. As discussed earlier, Holt's virtual capitulation led almost inevitably to widespread disbelief in his results. Had these results been vigorously defended by their originator, a whole range of hypotheses, 'ad hoc' or otherwise, might have been put forward to explain the apparent clash between the rival experiments. We simply have no way of knowing what *might* have happened.

One clue is available to us. Soon after Holt's result became known, he was contacted by a member of what might be termed an 'alternative science' institution in California. This group was closely involved in the parapsychology movement, and expressed interest in Holt's work. Holt did not follow up this contact — indeed his supervisor, Pipkin, informed me that one reason why they were reluctant to publish their results was because they did not wish to give encouragement to unorthodox groups who might exaggerate the significance and credibility of Holt's result. If Holt *had* been willing to invoke parapsychology to account for his anomalous results, the involvement of such groups would be classed by some observers as anything but trivial (30).

Obviously, any discussion of how things *might* have turned out, had circumstances been different, must be speculative. The justification for such speculation is that unless we are prepared to consider alternatives to the *actual* outcome, that outcome can easily be interpreted as inevitable and unproblematic. The whole process of social change within science then becomes either inaccessible to sociological analysis, or else describable only as the straightforward application of a generalized scientific methodology. Where there is controversy in science, it is relatively easy to show the existence of alternative interpretations of empirical data, each with its own sort of plausibility. Where there is no controversy, as in the LHV case, the sociologist is forced into the role of a devil's advocate, however immodest this may appear, if the scientists' interpretation is to be rendered problematic. In what follows, I shall adopt such a role, not in order to argue that the LHV group were in any sense mistaken, but to illustrate, once again, the role of the social and cultural context in determining the actual outcome.

Virtually all the discussion which took place concerning Holt's experiment was a discussion about where the error in his work might lie. As Pipkin put it

We couldn't understand why the experiments clashed. Clearly, the reasonable thing was that one of them was wrong.

For many excellent reasons, it was concluded that it was Holt's experiment which was wrong. But this decision took place within a context of shared theories and mechanisms and, equally importantly, within a context of almost total disbelief in LHV. Anyone who wished to defend the validity of Holt's result, particularly after the results of the later experiments became known, would almost certainly have had to reject some section of contemporary physics. The fact that this did not happen does not necessarily imply that it could not have happened.

For example, Clauser published a paper in 1976 (30a) in which he describes an experimental investigation of Holt's 'anomaly'. In this paper, he points out several 'possibly significant' differences between Holt's apparatus and Clauser's own partial replication. One of these is that different sorts of polarisation analysers were used: Holt employed calcite crystals, while Clauser (and all the other experimenters) employed sets of glass plates, arranged in parallel rows and aligned at the 'Brewster angle'. Now morphologically, a lump of crystal and a set of 13 or so glass plates are quite dissimilar, but functionally they are held to be identical (or at least sufficiently alike to avoid the necessity of including calcite crystals in the replication). This functional identity is derived from our current theories about the nature of polarised light; in other words, the identity is defined by the context.

(The practical equivalence of these devices was defined by the social context in a much more direct way. Clauser's lab operated on a tight budget. He already had a set of glass plates from his earlier experiment, and would not have been able to purchase a pair of calcites even if he wanted to, which apparently he did not.)

I do not wish to claim that LHVs only manifest themselves in the presence of calcite crystals, any more than I wish to claim that LHVs only show up in the presence of experimenters whose surnames begin with 'H'. Nevertheless such claims are clearly not impossible to make. Their reasonableness, or lack of it, derives from a set of tacit conventions. The risks involved in rejecting these conventions vastly outweighed the possible benefits – in this particular case. All I wish to point out is that functional equivalence is dependent on theory. The fact that, for example, mercury thermometers appear to do the

'same' job as alcohol thermometers or (less obvious) as thermocouples, only derives facticity from our shared theories of thermometry (31). The apparent solidity of material artefacts should not obscure their links with abstract theories. Let me provide another argument from the LHV activity. One LHV experiment, proposed by Aspect, is still in progress (32). It is designed to test what seems a rather radical hypothesis. The experiments to date are all static experiments: the apparatus is set up and a series of readings then taken. Such experiments do not rule out the possibility that some sort of signal is transferred from one part of the apparatus to another: this signal might contain information about the configuration of the polarisation analysers, and the photon source might then modify its output in order that the results *appear* to agree with QM. The proposed experiment would be able to detect any such subterfuge.

The results of this proposed experiment are awaited with interest by the LHV group. The hypothesis which it tests is evidently taken seriously, despite the fact that it hypothesises a new mechanism not presently known to physics. It does not account for Holt's results, but I think it does indicate that it is possible to suggest that LHVs, which by definition are unknown quantities, have special properties which make their detection less clear-cut than we might suppose.

I wish to present a final piece of evidence. This concerns the response of a number of LHV physicists to a suggestion such as the above. I asked these physicists whether there was any effect which could be present in all the experiments *other than Holt's*, and which could account for the disagreement between Holt's results and the results of the other experiments. At first, this possibility was denied emphatically, but when pressed they replied in terms such as:

I can't think of an example, but I haven't really thought about it, though.

Oh, you can always mock up a very perverse or psychotic type of mechanism.

The errors argument explains my prejudices, if you like. Obviously, someone could come up with a systematic error which goes the other way.

Some interviewees *did* suggest errors which could be present in all the other experiments, and they were indeed 'psychotic' and 'screwy' in terms of current physical theories. Since these effects would have to be present in a number of experiments, and would have to have gone unnoticed by several

different people, it is not surprising that no-one takes them at all seriously. I certainly do not claim that people should. All that I am trying to show is that arguments cited by physicists as reasons for rejecting an anomalous experiment do not take the form of logically-compelling deductions from a set of timeless axioms. Science is not like that. Scientists are continually faced with decisions about where to invest their time. They study problems in which they feel there is some likelihood of gaining worthwhile results. LHV had had its chance, and it had failed to show results. Everyone agreed that it was time to move on. The possibility that Holt's results were correct was simply not plausible. But the assessment of plausibility is a complex judgement. In a different context, where someone had felt strongly in flavour of LHV, or where someone had been ingenious enough to construct an alternative account of the experimental results, the case might not have closed quite so quickly.

What role should we give to Nature in all this? Might it not simply be that QM is a correct description of the world, and that LHV is not? Indeed it could. But we do not have direct access to Nature. We approach reality through experimental practices into which we are socialised (33), practices which are located in a social context. We describe experimental data in terms of observational categories which are heavily dependent on current theories (34). Thus our knowledge about the world cannot be isolated from the social context in which that knowledge is generated.

Notes and References

1. See, for example, S. B. Barnes, *Scientific Knowledge and Sociological Theory*, Routledge and Kegan Paul, London, 1974; and David Bloor, *Knowledge and Social Imagery*, Routledge and Kegan Paul, London, 1976.
2. Michael Mulkay, *Science and the Sociology of Knowledge*: Allen and Unwin, 1979.
3. K. D. Knorr, 'Producing and Reproducing Knowledge: Descriptive or Constructive? Towards a Model of Research Production', *Social Science Information* 16, 670-74 (1977).
4. Woolgar has applied an ethnomethodological analysis to science itself. Steven Woolgar, 'Writing an Intellectual History of Scientific Development: The Use of Discovery Accounts', *Social Studies of Science* 6, 395 (1976).
5. Max Jammer, *The Philosophy of Quantum Mechanics*, Wiley, New York, 1974, p. v.
6. Max Jammer, *The Conceptual Development of Quantum Mechanics*, McGraw-Hill, New York, 1966, p. vii.
7. For a comprehensive review of alternative interpretations of QM, see Jammer, *op. cit.*, 1974, Note 1.

8. Bernard D'Espagnat, *Conceptual Foundations of Quantum Mechanics*, Benjamin, London, 2nd. edn., 1976, pp. 75–158.
9. J. S. Bell, 'On the Einstein Podolsky Rosen Paradox', *Physics*, 195 (1964).
10. In this paper, I concentrate on those experiments which involved the use of optical photons produced by atomic cascades. For details of the experiments, see J. F. Clauser and A. Shimony, 'Bell's Theorem: experimental tests and implications', *Reports on Progress in Physics* 41, 1881 (1978).
11. This is not strictly correct. An earlier experiment by Papaliolios had tested a quite different hidden-variable theory, but the test was never claimed to be decisive. For details, see Jammer, *op. cit.*, 1974, Note 1.
12. D'Espagnat, *op. cit.*, 1976, Note 1, pp. 161–163.
13. This in turn led to some interesting results. Some of the theorists attacked the whole experimental methodology, and argued strongly in favour of an alternative, explicitly speculative methodology. For details, see my forthcoming Ph.D thesis, University of Edinburgh.
14. Personal correspondence with Abner Shimony, 1977. All the statements made in this paper by LHV physicists were obtained from correspondence and interviews over a two year period. In some cases, particularly when discussing sensitive issues, anonymity was requested, and some of my quotations are therefore unattributed. The sources for all other quoted statements are made clear in the text.
15. Hagstrom found a similar lack of concern about type-casting among Ph.D physicists. See W. O. Hagstrom, *The Scientific Community*, Southern Illinois University Press, Carbondale, 1975, p. 160.
16. This list of factors may not be exhaustive. Another possible factor was the small chance of a large pay-off if QM were to be refuted. In retrospect, few of my interviewees claim to have taken this possibility at all seriously, but it is difficult to know whether this claim is coloured by what they now know about the experimental results.
17. Of course, these physicists were not merely passive pawns in all this. For example, Clauser made strenuous (and successful) efforts to find a laboratory with the necessary equipment. He moved 3000 miles, took up a post at the same institution, and slowly worked his way into the lab concerned.
18. E. S. Fry and J. McGuire, unpublished grant proposal, 1971, p. 4.
19. M. Lamehi-Rachti, *Mécanique Quantique et Théories des Variables Cachées Locales*, unpublished Ph.D dissertation Université de Paris-Sud, 1976, p. 2.
20. R. A. Holt, *Atomic Cascade Experiments*, unpublished Ph.D dissertation, Harvard University, 1973, pp. II, 3–4.
21. J. F. Clauser, M. A. Horne, A. Shimony, R. A. Holt, 'Proposed Experiment to Test Local Hidden-Variable Theories', *Physical Review Letters* 23, 880 (1969).
22. For a detailed discussion of the assumptions, see Clauser and Shimony, *op. cit.*, 1978, Note 10.
23. S. J. Freedman and J. F. Clauser, 'Experimental Test of Local Hidden-Variable Theories', *Physical Review Letters* 28, 938 (1972).
24. The initial status of this manuscript was still unclear. In later publications by Holt and co-authors, the manuscript is cited as 'R. A. Holt and F. M. Pipkin (to be published)'.
25. 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 might have been able to find the error. To this extent, a question mark remains. However, most of the LHV physicists recognised the difficulty involved in such error-tracing.

26. This does not necessarily mean that Holt *maximised* his success. Some of my interviewees claimed that in Holt's position they would have gone about things slightly differently. This might have led to a slightly more or less favourable outcome. What does seem certain is that Holt *was* concerned with gaining a favourable outcome.
27. See, for example, B. Wynne, 'C. G. Barkla and the J Phenomenon: A Case Study in the Treatment of Deviance in Physics', *Social Studies of Science* 6, 307 (1976), and H. M. Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology* 9, 205 (1975).
28. E. S. Fry, 'Two-Photon Correlations in Atomic Transitions', *Physical Review* A8, 1219 (1973).
29. E. S. Fry and R. C. Thompson, 'Experimental Test of Local Hidden-Variable Theories', *Physical Review Letters* 37, 465 (1976).
30. Ironically, parapsychologists *have* become involved, but they now argue that the results *in favour of* QM provide evidence to support their views. See, e.g. Jack Sarfatti, 'Towards a Quantum Theory of Consciousness, the Miraculous and God', unpublished paper, Physics/Consciousness Research Group, San Francisco.
- 30a. J. F. Clauser, 'Experimental Investigation of a Polarisation Correlation Anomaly', *Physical Review Letters* 36, 1223 (1976).
31. See H. M. Collins, *op. cit.*, 1975, Note 27, p. 216; and Bill Harvey, 'Cranks and Others: Science as a Sociological Phenomenon', *New Scientist* 77, 739 (1978) (March 16th, no. 1094).
32. A. Aspect, 'Proposed Experiment to test the nonseparability of quantum mechanics', *Physical Review* D14, 1944 (1976).
33. P. L. Berger and T. Luckmann, *The Social Construction of Reality*, Penguin University Books, Harmondsworth, 1971.
34. See Bloor, *op. cit.*, 1976. Note 1.