

THEORETICIANS AND THE PRODUCTION OF EXPERIMENTAL ANOMALY: THE CASE OF SOLAR NEUTRINOS

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Introduction

In-depth studies of the development of particular pieces of scientific knowledge form the hallmark of recent work in the sociology of science (1). Broadly informed by a relativist approach, the authors of such case studies have attempted to explain scientific development in a fully sociological manner. That is, the main explanatory weight is given to the social world rather than to the natural world.

The emphasis of these case studies has tended to be on either experimental or theoretical controversies. However, it is also important to consider cases where both theoreticians and experimentalists are involved. In particular we need to know what role is played by theoreticians when a controversial experimental result appears which does not fit their theories. It is this type of question which I address below by drawing on a study made of solar-neutrino science — an area where there is an experimental result which conflicts with theoretical expectation.

Theoreticians and Experimenters

Theoretical and experimental work, in physics anyway, is largely carried out by two different and clearly identifiable groups with different work environments, publication patterns, career structures and rewards (2). Although there are some exceptions, large areas of modern physics have a well defined division of labour between theory and experiment.

On a recent round of interviews of American scientists working in the field

of solar neutrinos some of these differences were apparent (the differences noted here may only be local to this branch of physics – high-energy physics, for instance, has much larger teams of experimentalists and theoreticians). I noticed, for example, that I was more likely to encounter a theorist in the privacy of his office on the upper floors of the physics building, whilst an experimentalist was usually to be found on the ground floor or basement, either near or in his laboratory (many theoreticians ‘look down’ on experimentalists simply because the large and complex pieces of apparatus used by experimentalists are more conveniently housed in the lower levels of buildings). Experimenters were more likely to be surrounded by post docs., graduate students and technicians, whilst the theoreticians seemed to favour working alone in the sanctity of their offices. Interviews with experimenters were often conducted amidst an assortment of instruments and odd pieces of hardware in various stages of assembly. This contrasted with the array of books, periodicals, computer programmes and the blackboard covered with formulae which greeted me when I entered a theorist’s office – these were of course, the instruments of *their* trade.

It is clear that theorising and experimentation are very different activities and require quite different competences. The theorist constructs and manipulates the esoteric formalisms characteristic of most modern physical theory, whilst the experimentalist constructs and manipulates the sophisticated pieces of measuring apparatus found in today’s physics laboratories. Yet, despite obvious differences in the two types of activity, at some point they must interact – and the exact nature of that interaction has been the problem which has been taken up in recent work in the philosophy of science (3). The problem I find with such philosophical work is that the issues are discussed in the abstract. For example, philosophers might consider the relationship between a particular observation and theory without any consideration for the setting in which they were produced. It seems to me that in science theory and observation are brought together not in some abstract world (such as Popper’s ‘World three’ (4)) but in a particular scientific and social setting.

The approach that I favour for investigating the interaction of theory and experiment is to trace the parts played by theoreticians and experimenters in the development of a concrete area of science – in this case solar-neutrinos. By outlining the activities of the two groups and in particular their relationship,

hopefully we will gain some insights as to their contributions to the production of scientific knowledge.

The Solar-Neutrino Anomaly

Solar-neutrino science has been dominated by one outstanding problem – the solar-neutrino anomaly. This arises from a clash between the theoretical prediction of the flux of neutrinos coming from the Sun and the experimental measurement of this flux.

The possibility of detecting solar neutrinos and thus confirming nuclear fusion as the Sun's motor, first became a realistic enterprise in 1958, when it was pointed out that the sun produced a rare branch of neutrinos whose energy spectrum was sufficient for them to be detected on earth. In 1964, at a cost of \$ 600 000, a detector was built in a gold mine in Lead, S. Dakota (a mile of rock shields out cosmic rays). The detector was based on a large tank of cleaning fluid (perchloroethylene) containing chlorine-37 atoms with which incoming neutrinos would interact to produce radioactive argon-37. This could be extracted and detected by its characteristic Auger electron decay. By 1967 the flux of solar neutrinos was being monitored. The reported detection rate has always been lower than theory predicts. Although there have been fluctuations in both the theoretical prediction and experimental result, the discrepancy has remained and there is now a growing consensus that the result is correct.

It is this discrepancy between theory and experiment which forms the basis of the present study.

The investigation which I undertook had both sociological and historical dimensions. The main sociological data came from tape-recorded interviews with as many of the scientists who had made significant contributions to the field as were accessible geographically and within the logistics of the project (most of the interviews were conducted on one fieldwork trip to the U.S. in November/December 1978). Interviews were also conducted with scientists on the fringes of the field (for instance, those connected with funding). In all forty interviews took place. In addition to this 'oral history', the traditional History of Science sources, provided by the primary and secondary literatures of the field, were used. A substantial amount of correspondence between some of the leading solar-neutrino scientists was also collected, and this provided a further source of data.

The history of the solar-neutrino anomaly can be conveniently divided into two parts around the date of August 1967, when the first experimental result was reported. I will describe the activities of experimentalists and theoreticians and their relationship pre-1967 and post-1967.

PART I

The Activities of Experimentalists and Theoreticians Pre-1967

The pre-1967 period can be further divided, into the time up until 1958, when the main experimenter worked largely alone, the time between 1958 and 1964, when several theoreticians became interested, and, finally, the period 1964–7, which was characterised by an intense interaction between the experimenter and one theoretician. I will discuss each of these periods in turn.

The Early Situation – Very Much an Experimental Problem

Until theoreticians became directly involved in 1958, solar-neutrino detection was largely an experimental problem. The attempt to detect solar neutrinos was preceded by an experiment to measure the flux of neutrinos produced at a nuclear reactor. The chlorine-37 radiochemical technique, which was used in both experiments, was first proposed in 1946 (5), but it was not until 1954 that Raymond Davis, Jr., of the Brookhaven National Laboratory, was able successfully to build and operate such a detector. No earlier attempt had been made because it was thought the background effects would swamp any signal.

The first detector consisted of two 500 gallon tanks and was set up to measure the neutrino flux produced at the Savannah River nuclear reactor in 1956. However, events in nuclear physics overtook the experiment when it was discovered that there were significant differences between the neutrino and its anti-particle, the anti-neutrino. Unfortunately for the experiment it turned out that only anti-neutrinos were produced at reactors, and, as the detector was only sensitive to neutrinos, only background counts were registered. At the same time as Davis, using radiochemical techniques, was getting nothing, Reines and Cowan, also working at Savannah River, but with

a direct counting array, produced the first direct experimental evidence for the existence of anti-neutrinos. Despite the negative result, there was some interest in what Davis had done because it provided a confirmation of the difference between the neutrino and anti-neutrino.

The aim of Davis's research programme was the detection of neutrinos and, after the reactor experiments, he began thinking in earnest about other sources of neutrinos, in particular the Sun.

Davis was very much a loner at this time in thinking about the possibility of solar-neutrino detection. The elusive nature of the neutrino (it only interacts weakly with matter) meant that most people neglected it as a possible probe of the Sun. The consensus amongst theoreticians was that the predominant energy cycle in the Sun was the proton-proton cycle of reactions and that it did not produce any neutrinos of sufficient energy to trigger Davis's detector.

Davis's attitude, however, was that it might be worth putting a large tank in a mine just to see what signal there was. However, there were obstacles to doing this as such an enterprise would inevitably be expensive. Given the largely negative attitude towards the possibility of detecting solar neutrinos which prevailed at that time, it is unlikely that he would have got funding for an experiment looking 'on the off chance'. Davis needed some sort of further justification for doing his experiment, and it was towards theory that he looked.

Davis's interest in theory can be seen from his attitude towards one of the theoretical possibilities. This concerned a nuclear reaction in the sun, the conversion of ^3He to ^4Li by proton capture. The ^4Li formed would then decay by the emission of a very energetic neutrino which Davis should be able to detect. Most theoreticians considered this reaction was unlikely to occur. Davis, however, was more enthusiastic about the possibility because it gave him something to test. He explained the situation as follows (6):

Kuzmin [A Soviet physicist] posed this . . . he pointed out it was possible, you don't know – that kind of argument. And Z [a nuclear physicist] said 'Oh hell, lithium-four can't be stable'. He talked about the nuclear structure 'It is absolutely out of the question' . . . So you had these two viewpoints. For me it was interesting because at least someone posed something you could test.

I will break off from my account of the development of the solar-neutrino

problem here in order to outline a schema of analysis which will prove to be useful in describing events.

Cycles of Credibility

Thus far the relationship between the experimental program of neutrino detection and theoretical concerns would appear to be unexceptional. Indeed the activities of the experimentalist seeking a theory to test critically seem to fit well with the Popperian model of science (7). As mentioned earlier, my objection to philosophical approaches (such as Popper's) to the relationship between theory and experiment is that the particular developments under discussion are frequently divorced from their social setting. In order to clarify my criticism of the philosophical approach I will set it against a sociologically informed analysis of science, outlined recently by Latour and Woolgar (8). This approach was developed from an anthropological study of a scientific laboratory (the aspect of Latour's and Woolgar's study drawn on here is not the central thrust of their argument – for that the reader is advised to consult the original work).

In order to make sense of inter-relationships between individual scientists, the laboratory, and other laboratories, within the overall context of the production of scientific facts, Latour and Woolgar found it useful to talk about 'cycles of credibility'. In a departure from the pre-capitalist system of scientific reward developed by Hagstrom (9) and, to a lesser extent, the capitalist economy of scientific authority outlined by Bourdieu (10), the authors conceived of scientists as investors of credibility in a market in which there is a demand for credible information. Credibility can take on different forms and individual scientists are best described as being engaged in a cycle of credibility whereby different currencies of credibility (or capital accumulations) are converted one to the other. The activity of producing scientific results will enhance the overall credibility of the scientist, provided, of course, that he produces credible results. The production of credible results enables the scientist to get more resources, such as funding and equipment, which can, in turn, be converted into more credibility by producing yet more results. Scientists can thus be seen as investing their previously acquired credibility in the hopes of getting a return by producing credible information.

One benefit of this picture of science is that it enables what have traditionally been seen as disparate themes to appear alongside each other; there is a correspondence between, for instance, economic and epistemological terms. It can be seen that it is quite possible for scientists to be making crucial tests of theories *as well as* furthering their own careers. To conceive of the scientific enterprise in terms of merely the testing of theories is as one-sided as to picture scientists as opportunist careerists. Neither explanation is alone sufficient to describe the full richness of scientific activity.

If scientists are engaged in a struggle to maximise their credibility then we can expect them to exploit the specialization of modern science to further their investment strategies. By forming partnerships it is possible for individuals with quite different areas of scientific expertise to come together and make a joint investment. Latour and Woolgar cite many cases where one scientist or group will give money or equipment to another in the expectation that they will produce results that are of use to the first group, thus enabling the first group to boost their credibility. Clearly the recipients of the equipment and money will also boost their own credibility by producing reliable information for the first group. It is this type of joint enterprise which has been important in the development of the relationship between theoreticians and experimenters in the area of solar neutrinos.

If we look at Davis's career in terms of investment strategies we can see that by 1958 he had reached an impasse. He had devoted most of his career to the development of the chlorine-37 neutrino detector. His investment in this form of experimentation had shown some return with the results of his experiments at reactors and, by 1956, he had built up a substantial capital of credibility in this field. However, if he was to further increase his credibility he had to find other sources of neutrinos to detect (after the successful experiments of Reines and Cowan there was little point in continuing with reactor experiments). The only source that might be usefully detectable with this type of equipment was solar neutrinos and hence it would be a natural extension of his investment strategy to explore this avenue. The problem Davis faced, however, was that it did not seem there were enough detectable neutrinos to warrant the rest of the scientific community investing the large sums of money needed for such an experiment. Davis had to persuade others that it was worth investing in his experiment. His hope was that theoretical astrophysicists and nuclear physicists, who studied the nuclear reactions in

stars, might be persuaded to view his experiment as a test of their theories and thereby support his effort. If such a 'deal' could be struck then both groups would benefit. Davis would get the money for his experiment and the opportunity to further his credibility, and the theorists would have an opportunity to test their theories of the nuclear reactions in stars – a test, which if successful, would undoubtedly enhance their reputation as producers of credible information.

One of the merits of using an economic metaphor to describe scientific developments is that scientists themselves often speak in these terms. Latour and Woolgar found many scientists explicitly referred to their investment strategies. In my own conversations with solar-neutrino scientists, I, too, found that economic language was prevalent. In my continuing account of the solar-neutrino problem we will find that the description in terms of credibility cycles is in close resonance with how the participants themselves viewed the situation. This gives us some encouragement that the account offered here is at least not as divorced from real scientific activity as that offered by philosophers.

Let us now return to the solar-neutrino story and see how it was that the other component in the partnership – the theoreticians – became interested in going into business with the experimentalist.

1958–64. The Entrance of the Theoreticians

The theoretical justification that Davis hoped for came about in 1958. The cross-section (probability of nuclear interaction) for one of the nuclear reactions which could possibly occur in the Sun, $^3\text{He} + ^4\text{He} \rightarrow ^7\text{Be} + \gamma$, was found to be much larger than expected.

The significance of this result was that ^7Be was formed, which, by another reaction (the capture of a proton), could go on to produce ^8B , which would in turn decay by emitting a high energy neutrino – a neutrino which could be detected in Davis's proposed experiment. Although the ^7Be proton capture had never been measured, calculations showed that there should be a significant cross-section. Two nuclear astrophysicists, William Fowler and A. G. W. Cameron, made the necessary calculations and in early 1958 they both wrote separately to Davis informing him that he might be able to detect ^8B neutrinos and indeed that he might be doing so already.

The importance of the new result to Davis was, of course, that now he had

good reasons to do the experiment. However, he did not plan the experiment in the expectation that he would exactly confirm the theoretical prediction. As he told me:

I guess this is an experimentalist's attitude. What do you use the theory for? . . . You use the theory for guidance as to whether it makes sense to do the experiment. . . . And so you say at least I've got the theoreticians telling me that now's a good time to do the experiment. But if you do the experiment you may or may not find what they say.

Davis's primary goal was the detection of solar neutrinos. This is what he hoped would give him credibility in his field. He could achieve this goal by testing astrophysical theories. However, the test, as such, was for him only a means to an end. For the nuclear astrophysicists, however, the test itself was the crucial thing. If the test of nuclear astrophysics was successful they would boost their own credibility. Thus the performance of a solar-neutrino experiment served the interests of both groups.

Of the two scientists who wrote to Davis in 1958 pointing out the Boron-eight possibility, the most significant for the development of the experiment has undoubtedly been Fowler. Although Cameron continued to be interested in the solar-neutrino possibility and calculated some neutrino fluxes, his interest came more from astrophysics as a byproduct of his general concerns with stellar evolution. Fowler's interest, on the other hand, came more from nuclear physics. Fowler, who heads a group of nuclear-astrophysicists at CalTech, orientated his research program around the slogan that 'more can be learnt about stars by studying nuclear reactions than by looking down telescopes'. One outcome of this programme was a celebrated paper, published with Hoyle and the Burbidges (11), in which they produced their schema for the synthesis of the elements by tracing the evolution of stars through the various stages of nuclear burning. By the early 60's, Fowler thus already had considerable scientific achievements, and the detection of solar neutrinos, which would provide the first direct test of the nuclear reactions thought to occur in stars, would have been, as one respondent put it, 'the icing on the cake'.

Clearly if the nuclear astrophysicists were to take an interest in Davis's work they had to think he was capable of producing reliable information concerning the solar-neutrino flux. It can be seen that Fowler, anyway, had a high regard for Davis's work. An extract from his letter to Davis in 1958 reads (12):

Permit me to conclude by expressing my great admiration for your beautiful work on this problem.

That Fowler should have a good opinion of Davis is almost a precondition for his interest in the experiment as a test of nuclear astrophysics. Clearly if a fruitful partnership was to be entered into each party had to regard the other as capable of producing credible information. The nuclear astrophysicists would have nothing to gain from an incompetent test of their theories, in the same way Davis would be left with a costly white elephant (an olympic swimming pool of cleaning fluid a mile below the Earth's surface) if it turned out there were no ^8B neutrinos to detect.

The aim of the partnership developing between Davis and the astrophysicists was, like in all business agreements, to produce a return for the investments of both parties. Such returns would only come when a successful solar-neutrino experiment was under way – it was towards this goal that both parties now worked.

With the theoreticians telling him there was a detectable flux of neutrinos from the Sun, Davis proceeded to 'convert' his apparatus into a solar-neutrino detector. He did this by moving his tanks from the Savannah River nuclear reactor to the Barbarton mine in Ohio, where the rock would provide the necessary cosmic-ray shield. At the same time Fowler's group at CalTech started to make the first experimental measurements of the crucial ^7Be -proton capture cross-section. As it turned out this cross-section was lower than expected and it seemed unlikely that Davis's 500 gallon tanks would be big enough to see anything. Davis's move to the Barbarton mine was, however, important because it enabled him to demonstrate the feasibility of this sort of detection technique for a solar-neutrino experiment.

Out at CalTech Fowler continued to make calculations of the solar-neutrino flux. He got a stellar-model specialist, R. D. Sears, to make some computations but it was becoming increasingly obvious that whether or not there was a detectable flux of Boron-eight neutrinos depended on very detailed and complex calculations. And working through the problem in the detail required was going to be a fairly major undertaking. It was here that Fowler's influence was important because he drew Davis's attention to a physicist who might be capable and willing to work on the problem. This physicist was John Bahcall. If solar neutrinos were to be Fowler's

'icing on the cake', they were for a while, any way, to be John Bahcall's 'bread and butter'.

Bahcall's interest was stimulated by a letter from Davis. He was at the University of Indiana and was acquiring a reputation as an expert in the calculation of stellar nuclear reactions. Davis wrote to Bahcall in the hope that he would be willing to make some calculations. John Bahcall described his reaction to the letter as follows (13):

The reason I decided to work on it, is the result of a conversation with [a colleague] . . . We got into a discussion about this letter I got from Davis, and I had to decide whether I would spend the time, once I saw that it was some time to calculate . . . so we got into a discussion about how unique or fundamental the experiment would be . . . I became convinced that it was . . . really a unique way of testing an otherwise very fundamental theory.

It can be seen that Bahcall had to weigh up whether to make the necessary investment in the venture. Clearly doing the required work would be demanding and time consuming, but it is equally clear that, after receiving advice from his colleague, he thought the scientific rewards (which would be co-extensive with a boost to his own capital of credibility) provided by a test of stellar evolution, made the proposition put to him by Davis worthwhile.

Having decided to make the necessary calculations, Bahcall kept in touch with Davis by letter, reporting on his progress. Although it turned out that this particular calculation did not show any great promise for the feasibility of Davis's experiment, Bahcall was now enthusiastic about making further calculations. He was able to do so by virtue of a post. doc. place at CalTech in Fowler's group.

It was now in the late summer of 1962 that intense theoretical work on the solar-neutrino problem started at CalTech. In order to make a sufficiently detailed calculation, several different types of theoretical expertise were needed. It first of all required someone who could calculate all the nuclear reaction rates (both in the Sun and in the experiment) – this was Bahcall's particular expertise. Experimental values of the nuclear reaction rates, where they were obtainable, were also required – these were provided by Fowler and the nuclear physicists working with him in the CalTech, Kellogg Radiation Laboratory group. Finally a detailed model of the Sun was required, into which the nuclear reaction rates could be plugged. The production of solar models is a complex theoretical activity in its own right and requires a massive

computer program. A sub-routine of this program gives the neutrino fluxes. The generation of such models for stars in general had become a well established branch of astrophysics and two CalTech scientists, R. D. Sears and Icko Iben, were experts in this kind of computation. It was Bahcall's job to put all the different parts of the calculation together – not an easy task as the stellar-model scientists could see little point in working out the Sun in the detail required and were more interested in the late stages of stellar evolution. Indeed Bahcall required Fowler to use his institutional position as head of the group in order to set things in motion.

Eventually the detailed calculations were made by Bahcall in collaboration with Fowler, Iben and Sears, but again the results did not look over promising for the solar-neutrino experiment. As Bahcall wrote to Davis (14):

These results suggest the experiment is extremely difficult. Do you think it is possible?

Davis's reply was more optimistic (15):

It would be possible to observe a rate as low as this . . . However, an experiment of this magnitude would be quite expensive, a rough estimate would be \$200,000 . . . I have started some exploratory discussions on the possibility of carrying out the experiment.

The reactions of Bahcall and the CalTech group can be seen from Bahcall's next letter to Davis (16):

We were all pleased to learn . . . that you have started exploratory discussions . . . Please keep us informed of your progress and let us know if we can be of any help.

It can be seen that the experiment, although expensive, was now perhaps feasible.

The joint venture that Davis and the CalTech group had entered, like all business ventures, required financing. Solar-neutrino detection is a costly business, especially as it results in only one number. Whereas the CalTech nuclear astrophysicists had a powerful and influential figure in Fowler as head of their group and were able to command the necessary resources for their side of the venture, Davis, who in any case required much more funding, was not in as good a position. He was based in a chemistry department and this type of experimentation was far more costly than the normal run-of-the-mill chemistry. A special application for funding to the Atomic Energy Commission

would have to be made. Fowler was able to help Davis with this application by writing to the chairman of Davis's department, who happened to be an old friend and former colleague of Fowler's. Fowler not only wrote in support of Davis but also offered to assist further up the funding chain.

It would be wrong to see these negotiations over funding as in any way scurrilous. All parties were acting with the highest scientific ideals. The point is that the struggle for scientific credibility cannot be divorced from the struggle to gain funding. It is the nature of modern science that attempts to further scientific knowledge appear alongside what might seem to the uninitiated as fairly crude money wrangles.

Bahcall continued to spend a large amount of time on the solar-neutrino calculations. In the summer of 1963 he visited the Niels Bohr Institute, in Copenhagen, and it was there that he made the breakthrough that was to prove decisive for the future of the experiment. Working with an idea given him by Nobel Laureate, Ben Mottelsohn, Bahcall was able to show that there was a special state in the chlorine-37 system – the analogue state – which would greatly enhance (by a factor of about twenty) the probability of boron-8 neutrinos interacting with chlorine. Bahcall described what happened next:

I went back to CalTech and calculated things very accurately . . . I gave a seminar . . . and people got very excited about it, so I called Davis. He then invited me to come to Brookhaven to give a seminar there. I think that was the first time we met.

Bahcall's new calculations were, of course, important because they boosted the number of neutrinos which Davis could expect to detect. The analogue-state discovery was also important for other reasons, as can be seen from Bahcall's account of what happened when he came to Brookhaven to give the seminar:

He [Davis] had arranged for us to meet the director of Brookhaven . . . we had to sell [the director] or else we wouldn't get the experiment. This nuclear-physics trick [the analogue state] would be something that [the director] would be turned on about, because he was himself a very bright nuclear theorist amongst other things . . . so we decided to sell him the experiment based mainly on this. Then more or less that worked.

The significant thing here was that the support of the director of Brookhaven was needed before any application for funds could be made. And Bahcall, being a nuclear theorist, could assist Davis in convincing the director of the merits of the experiment.

Shortly after this there was a conference held in New York where Bahcall presented his calculations, based on yet more computations by Sears, and Davis gave a short account of the experimental possibilities. These presentations formed the basis of the first major papers stemming from the collaboration. Again Fowler's influence was important; he was pulling strings in the background to try to get the experiment funded and he advised Bahcall and Davis to get something into print quickly in order to draw the attention of the rest of the scientific community to the proposed experiment. Eventually two papers, an experimental one by Davis and a theoretical one by Bahcall, were published 'back to back' in *Physical Review Letters* in March 1964 (17). I asked Davis why they preferred to publish separate papers rather than producing a joint publication; he told me:

See the topics were very different . . . I can't say anything about solar models, and the cross-sections . . . To do the experiment, how to do the experiment, I know all about that.

As we shall see below this publication format is especially significant. This is because, although reflecting the common-ends of the joint venture, it also maintains a boundary between the two very different types of expertise involved.

1964–7. The Years of Close Collaboration

Shortly after the appearance of the articles, two events of significance happened. The calculations made by Bahcall of the analogue state were indirectly confirmed by measurements made on the ^{37}Ca system (18) and the necessary funding for the experiment was made available by the Atomic Energy Commission.

Davis now had to choose a mine and develop plans for constructing the apparatus. There were lots of experimental problems to be solved and contractual details to be sorted out. Eventually, after much negotiation, excavations were started in early 1965 in the Homestake gold mine, in Lead, South Dakota.

On the theoretical front there was plenty of work to be done in refining the calculations. Several new experimental measurements of the relevant input physics were being made and these new results had to be fed into the

model predictions. Other theoreticians, such as Cameron, were also making calculations, so Bahcall was faced with the task of comparing these calculations with his own and sorting out any differences which appeared.

Relationships between Bahcall on the west coast and Davis on the east coast required delicate handling as each felt his way into the collaboration. This can be seen from their reaction to two episodes where reporting of the collaboration gave the impression that only one of the two groups was responsible for the experiment. In a letter to Bahcall, written in early 1964, Davis expressed concern about a report in *Time* magazine (19):

The article, in *Time* magazine, seemed quite good but I was disappointed that there was no mention of your paper and the CalTech contribution. I, of course, like the idea of the experiment being a joint effort.

Later it was Bahcall's turn to express concern when a report appeared in *Scientific American* which seemed to attribute the whole project to him — the author of the article being under the impression that Davis was the technician supervising the building of the experiment! After Bahcall complained to the Editor, *Scientific American* published an *erratum* (20). Perhaps the somewhat delicate nature of the relationship between Bahcall and Davis at this stage can be seen from Bahcall's recollection of the Editor's comment when they first met:

He said it was unique in his experience at *Scientific American* that somebody complained about getting too much credit!

As the construction of the experiment neared completion, the contact between Bahcall and Davis increased. As Bahcall told me:

I went to visit the site . . . I didn't have much useful to say but I wanted to be involved in that . . . I used to go back continually to talk to him about theoretical things, advise him how often to take samples, current levels of theory . . . and in the language I was the house theorist.

Bahcall was now clearly committed to the experiment and was even giving advice on matters of experimental detail. Arrangements were made for Bahcall to become a Brookhaven consultant. Bahcall was thus truly the 'house theorist' — he was paid for his services by the same house!

The relationship between Bahcall and Davis now encompassed more than mere technical matters. As Bahcall told me:

I was also the guy that encouraged him . . . We were good friends, he's much older than I but we had both more or less staked our careers on this. I had staked my career on my ability to predict the response of the instrument, that the instrument would work and be sensitive in the way I said it, and he in spending his major, almost his entire, effort in building the equipment.

It can be seen that career investments and personal relationships appear alongside each other — they are all integral parts of scientific activity. Bahcall's comment about having 'staked his career on this' shows the degree to which he had invested his credibility in the enterprise. It was not unnatural for Bahcall to feel increasingly apprehensive as the date when Davis would make measurements drew near. He recalled his attitude:

I can remember being enormously nervous before the results came out . . . I was then a young research fellow whose emotions and scientific advancement depended in a large part on my correctness in what I was asserting.

It can be seen that even the psychological state of the researcher is tied up with the enfolding drama.

By the stage Davis was ready to make his first measurements we can see that a lot of time, effort, and money had been invested in the experiment. In particular, it was the partnership between theoreticians and experimentalists which was so important in preparing the way for the first measurements. In a sense it was Bahcall's and Davis's scientific careers which were as much at stake as the flux of neutrinos.

PART II

1967—Today. The Appearance of the Anomaly

In this part of the paper I will look at how the solar-neutrino anomaly became established. That is how the consensus developed that this experimental result, which stood out against theoretical expectation, was worth taking seriously. Obviously results can only be anomalous in a given theoretical context and if theory could be brought into agreement with experiment, then we would no longer have an anomaly. In the case of solar neutrinos, however, the attempts to accommodate the result within standard theory have so far

been unsuccessful. Although I will be paying close attention to the attitude of the theoreticians to the experiment. I will not here be discussing theoretical developments *per se*.

In my discussion of the reception of Davis's experiment, as in my account of earlier developments, I will try and set events in a wider context. I, thus, do not share the view that the success or otherwise of experiments can be understood independently from the wider scientific and social setting in which they appear. In this particular case my main concern is to outline why Davis's experiment has managed to achieve credibility in the face of a hostile theoretical climate. The question is particularly pertinent when we consider that other experiments which clash with theory (the most relevant example here is perhaps Weber's claimed measurement of large fluxes of gravitational radiation) have lost credibility and are not generally believed (21).

In discussing the reception of the experiment it is useful to distinguish three groups – firstly, nuclear chemists and radio chemists familiar with the particular technique Davis used; secondly, astrophysical neutrino detection experts with other techniques; and finally, the nuclear astrophysicists whose work is directly impinged on by Davis's result.

The Reaction of the Chemists

When Davis got his first indication of a low result in August 1967 his initial task was to convince himself that he had made a good measurement. As he told me:

The thing that concerned me most was whether I was getting the right answer, . . . was there something wrong with the apparatus?

Davis was particularly worried because of his relative isolation. He worked alone, and, although he had assistance from two scientists in the design and the construction of the experiment, there was no other experimental scientist directly involved as he started to make measurements (there were, of course, technical staff). Thus, one of the first things he did was to get two of his immediate colleagues from Brookhaven to check over his work.

As Travis has stressed it is important for scientists to be seen to be acting correctly (22). Many respondents have commented to me that they have been most impressed by Davis in this respect. He has all the qualities of an

ideal experimenter — he appears to be careful, modest, and very open with his results. Indeed Davis has told me that he has made it a deliberate strategy to be open with his results because it is unlikely the experiment will ever be repeated. He has not only been open to suggestions but also has attempted to check many of the possible uncertainties in the experiment (possibilities which he himself might regard as highly unlikely) by running more tests of parts of the experimental technique.

After Davis had received the blessings of his immediate colleagues he felt confident enough to notify a wider audience of his results. He presented his initial results to a meeting of the American Chemical Society in September 1967.

The reactions of chemists to Davis's result has been that as far as they are concerned he is just doing standard chemistry — admittedly on a slightly larger scale than is usual — but, nevertheless, standard chemistry. Also the chemistry of argon is considered to be particularly simple.

Two other reasons may be important in understanding the acceptance which Davis's result has gained amongst chemists. Firstly, his result can be seen as posing very little difficulty for chemistry — it conflicts with astrophysics not chemistry. Secondly, Davis had been doing experiments of this sort (but on a smaller scale) for the previous decade and, thus, it is unlikely that chemists should only now be concerned about his techniques after being quite happy with them earlier. In other words Davis's previously acquired credibility in this field may have been enough to see him through.

The Reaction of the Astrophysical Neutrino Experimenters

Apart from Davis, the other main astrophysical neutrino experimenter is Fred Reines, of the University of California, Irvine. Reines had built two solar-neutrino detectors in the mid-sixties (23). One of these experiments, located in a mine in S. Africa, was actually operating in August 1967. Reines favoured direct counting techniques, similar in principle to those which he had used to detect neutrinos at a nuclear reactor. Reines and Davis were not only the two principal astrophysical neutrino detection experimentalists, they were also close friends.

When Davis got his low result, Reines got in contact with him to see whether it was worth pursuing his own solar-neutrino detection program. His

detector in S. Africa was already encountering a very large background and he had to decide whether to do a lot more work, perhaps building a shield, to try to get down to the sensitivity which Davis had achieved. After a lengthy discussion with Davis, Reines decided there was no point in continuing this experiment. The other detector that he had built, in the expectation of a much larger solar-neutrino flux, was never put into operation, again because it was unlikely that he could get down to the necessary sensitivity (24).

Reines has a very high regard for Davis and his assessment of this particular experiment was that it 'has been done with exquisite care, thoughtfulness and humility' (25). Again it is important to bear in mind that Reines was familiar with Davis's techniques from his earlier reactor work. Davis's Savannah river result, although a negative one, was consistent with Reines's own positive result. Thus Davis had already established his technique as far as Reines was concerned and Reines had every reason to trust his solar-neutrino results.

In view of the conflict between Davis's results and astrophysical theory, it might seem surprising that other experimenters have not placed a greater emphasis on checking Davis's result by an independent experiment (the experiment has never been repeated although the Soviets are building a similar detector). Clearly the huge cost and time-consuming nature of the experiment is an important constraint. It was also pointed out to me by many respondents that there is little scientific reward to be gained from a 'me too' negative result. Such reasoning seems to me, however, to beg the question, because it assumes Davis is correct. If he had made a mistake, and there were neutrinos there to detect, then another experiment could be highly significant. Another factor in the reluctance of experimenters to embark on replications may be their sceptical attitude towards astrophysical theory. As indicated already, Davis himself saw no reason why he should see exactly what the theoreticians predicted. This attitude was also shared by Reines who considered it to be up to the theoreticians to bring their theory into line with the experimental result.

In general then the attitude of other experimentalists to Davis's result has been that they are confident he is correct — a confidence which has grown stronger over the years as he has continued to report a low result and make further improvements and checks on his experiment. The clash with astrophysical theory does not seem to have been a reason for undue scepticism.

The attitude of the nuclear astrophysicists, whose theory would seem to be in conflict with Davis's result, has, however, been somewhat different.

The Reaction of the Nuclear Astrophysicists

I will start by looking at the reaction of the most involved theorist, John Bahcall. I will then go on to consider the attitude of the wider community of theorists.

Bahcall, unlike Davis, took the theoretical prediction very seriously. To him Davis's result was not just an anomaly, it was a personal disaster. He told me the following anecdote which perhaps illustrates the depth to which Davis's result affected his scientific life:

Ray Davis . . . came to CalTech and gave an informal seminar and Richard Feynmann was there . . . it was clear there was just absolute conflict and afterwards I was enormously depressed . . . I think that was really the low point of my feeling about science . . . after that seminar Feynmann . . . when he saw I was pretty much destroyed by this . . . was really very nice and told me 'Don't worry, you've done nothing wrong, nobody has found anything wrong in your calculations . . . if there's a discrepancy it's all the more important' . . . Even though he spent a lot of time with me, it took me quite a while to get over that . . . well it was a big blow . . . that it came out wrong. I think now that I was mistaken for the reasons that he said . . . the result is more important because it is in conflict, but at the time I was expecting something very different.

Bahcall's dismay is perfectly understandable. His ability to produce credible information was in doubt. Of course, as Feynmann pointed out, in the long term the failure of the theory might turn out to be to his advantage if a major change was needed. To have gone for a major, possibly revolutionary, change in the theory (what Bourdieu refers to as a subversion investment strategy (26)) would have entailed a substantial risk. After all, major changes in physical theory are comparatively rare events. It is clear that Bahcall himself preferred the safer bet of an incremental gain in credibility provided by a confirmation of the theory (a succession investment strategy) rather than the possible fame and glory of instigating a radical change. In any case it is not clear that there is much credibility to be gained from merely showing that a theory is incorrect, and any subsequent revolutionary change would, of course, throw the whole previous economy of credibility into disarray.

In view of Bahcall's commitment to his prediction, was he sceptical of

Davis's results? He could not recall for me his immediate reaction, but he told me that it would not have been unreasonable to have shown some initial scepticism:

I can remember being certain that the experiment was right a few years later, expressing myself publicly that way . . . I think it would not have been unnatural . . . at the time, to have wondered if I had invested the crucial years of my research development on a project that was going to go away . . . that I'd spent the four years which I had to prove myself, before I got a good assistant professorship or a poor assistant professorship doing a problem that was irrelevant . . . If you couldn't do the chemistry or couldn't get the atoms out or whatever, then I would have made a bad professional choice and I would have been much further out of the stream than I was . . .

Bahcall certainly seems to have gone over Davis's work with a fine-tooth comb. For instance, in the context of another anecdote about his relationship with Davis, he told me:

He came to CalTech . . . and I talked to him so forcefully and at such length about analysing his data statistically in detail . . . he got a headache, and my wife told me I had better lay off him!

Davis also recalled the attitude of Bahcall when he first learnt of the result:

I remember going to CalTech and giving a seminar on the result . . . going to John Bahcall's house . . . and he used to argue why I was so sure, and what are the arguments, and how did I know that . . . the nuts and bolts of the whole thing.

Bahcall still had worries in early 1968 as Davis prepared the first major publication of his result. Again this was to be an experimental paper alongside a theoretical paper by Bahcall and his collaborators. Davis sent Bahcall a copy of his paper; in the accompanying letter he wrote (27):

I tried to answer your worry about chemical trapping of ^{37}Ar .

This refers to the possibility that the argon was formed in the tank in the expected amounts but was not properly extracted because it bound with something in the tank and was hence 'trapped'. Most chemists thought this was unlikely; however, a number of theoretical physicists, including Bahcall, has worried about the possibility.

Having sent his paper to Bahcall, Davis spent the following weekend at CalTech. Bahcall then sent a letter to Davis on the Monday, in which he expressed further worries about the experiment; it commenced (28):

I am even more convinced . . . that the simplest explanation of your results is that the background counter was hotter than the counter which contained the sample.

There were several pages of accompanying calculations where Bahcall attempted to establish this point. The worry here was that the background rate was really lower than Davis measured it to be and hence that the signal in the counter from ^{37}Ar was really much larger because too many background counts were being subtracted from the total signal.

This claim, it seems, was soon answered by Davis to Bahcall's satisfaction and no further reference to it appears in the correspondence to which I had access. Eventually Bahcall's worries were all allayed and he was able to express public confidence that the result was a good one.

Bahcall's initial concern over the correctness of the result is again perfectly understandable when we consider what he had invested in it. As he himself has pointed out he had put the key years of his scientific career into making the necessary calculations and helping ensure that the experiment was funded. He had a lot to lose if it turned out that some experimental error was responsible. His careful checking of what Davis had done was facilitated by the joint nature of the project. He was able to put his worries to Davis directly without having to go through more formal channels — a process which might well cast suspicion on the experiment. Also he did not have to stick his neck out immediately and embrace the result. The publication format of separate papers that he and Davis had established allowed him to reserve judgment. In the theoretical paper that accompanied Davis's 1968 paper (29) nothing directly is said about the validity of the result — the ultimate responsibility for the correctness of the experiment was left in Davis's hands. But, as time went on and Bahcall continued to do theoretical work associated with the experiment, and thus continued to make further investments, it was increasingly likely that he would stand by the result in public, as was eventually the case.

Other theoreticians, apart from Bahcall, have been interested in the result of the solar-neutrino experiment. Many of these have connections with CalTech, which reflects the influence which Fowler's group have had. The particular importance of convincing Fowler and the CalTech group of the correctness of his experiment was recognised by Davis. When discussing scientists who had examined the experiment, he stressed the significance of the visit of CalTech nuclear physicist:

I always welcomed someone from their lab coming. You see he has a lot invested in this, Fowler, . . .

I wanted to be sure that he was thoroughly satisfied that we were doing things right.

Although many scientists, especially those at CalTech, had made investments in the experiment, none came close to Bahcall in the amount invested (with the possible exception of Fowler). It would thus appear that the rest of the nuclear astrophysicists had little to lose if the experiment became discredited and further it would save any embarrassment in the domain of theory. The attitude of the wider group has somewhat mirrored that of Bahcall in that most of them at one time or another have indeed been highly suspicious of the experiment. However, as yet, they have not been able to make any of their suspicions hold water. Many of them have been content to voice general worries in informal settings such as seminars, or the informal solar-neutrino conference held at U. C. Irvine in 1972 (30), but few have been prepared to come out with a public attack on the experiment.

The impression to be got is that the theorists' comments have been put forward in the guise of friendly criticism rather than hostile attacks. This may again have been important for the reputation of the experiment. Openly antagonistic criticisms in the mainstream literature would almost certainly have damaged the experiment. The antagonisms and, on occasions, open feuding, which have accompanied the Weber episode, have largely been avoided in this field. Davis's good relationship with Bahcall and the CalTech group, facilitated by their joint investments, may have played the decisive part here.

There has only been one instance of which I am aware when this largely informal way of dealing with criticism has broken down. In 1974–5, Davis received three letters from astrophysicists concerned about the possibility of ^{37}Ar being trapped in the tank (the same worry that Bahcall had back in 1968). Davis replied to these astrophysicists pointing out why such a possibility was unlikely and what sort of test he was prepared to do to check it. Two of the astrophysicists were satisfied, but the third one, Kenneth Jacobs, went ahead and published his objections in *Nature* (31). Jacobs noted that the detection chemistry was 'one of the more neglected aspects of the problem'. He put forward the hypothesis that 'the solar-neutrino problem is solely an artefact of the chemistry of the detection technique'. It is my understanding that, in the original version of his article, Jacobs had compared Davis's experiment with other famous cases in physics where there was only

one experiment near the noise level which, on replication, was found to be in error. It took Jacobs a year to get his paper into *Nature* and he eventually had to drop any comparison between famous errors and the solar-neutrino experiment.

Jacobs, who had carried out an extensive search of the chemistry literature and, who had even done some experiments with chemists, despite his astrophysics background, suggested several mechanisms for trapping and also produced arguments as to why previous checks run by Davis were inadequate. A reply to these criticisms soon appeared in *Nature* (32) and Jacobs in turn published a reply (33), concluding that 'my chemical trapping solution to the solar-neutrino problem remains a viable alternative'.

It is not my intention here to go into the details of the arguments and counter-arguments between Jacobs and his critics. In any case for most respondents the issue has been resolved by yet another check which Davis has carried out (33a). Jacobs's criticisms were, in effect, brought to an end in 1976 when he failed to get tenure at the University of Virginia (an episode uncorrected with his work on solar neutrinos).

The significance of the Jacobs contribution is that it shows that it was possible for plausible arguments against the experiment to be mounted in public. Indeed Jacobs's attempt to discredit the experiment by comparing it with notorious examples of past mistakes has been a successful debunking ploy in other cases (34). On this occasion, however, it is the debunker who has the minority view.

Jacob's criticisms probably came too little and too late to have any substantial effect. In view of his marginal position and the difficulty he had in getting his article accepted, he was always fighting an uphill battle against the emerging consensus that Davis's result could be trusted.

The majority of theoreticians now seem prepared to stand by the result. In giving reasons why they believe Davis, many draw attention to his modesty. By this they mean that he has not made any great theoretical claims for his results. The theoreticians' embarrassment has thus, to an extent, been saved.

This again can be seen as stemming from the joint enterprise. Just as it was important for Bahcall, in view of his past investments, that the experiment should remain credible, Davis too would have little to gain from stressing the conflict with astrophysics. The publication format further facilitated this because Davis could happily leave the theoretical consequences of his result

with Bahcall. Any theoretical criticisms fell on Bahcall's shoulders, thus ensuring Davis kept his smooth relationship with the rest of the theoretical community.

The Importance of Partnership

The theme of this paper has been that in order to understand the relationship between theory and experiment we should take account of the social relations between experimenters and theoreticians. In this case I have tried to show the importance of the joint investments of credibility undertaken by both parties. It is only in the light of these investments that we can understand the reception accorded to Davis's result.

In understanding the outcome of other scientific controversies it is perhaps important to look at the relationship between experimenters and theoreticians in these terms. From my own interview data I have one other case that is suggestive. One of my respondents, J, is an experimentalist, but he also likes to do his own theory when possible. J had recently made some observations and had been approached by a theorist, L, who offered to do the theoretical analysis in return for speedy access to the results. Rather than welcoming such collaboration, as Davis had done, J refused; subsequently the credibility of his observations had come under attack. J, himself, thought that this was because of his refusal to co-operate with the theoreticians. He told me:

I'm sure that if I had gone to L, I would have had holy water sprinkled on us and thus would all be in good shape.

J took a dim view of Davis's relationship with the theoreticians, as the following extract of interview material shows:

J: I think first of all Davis is the kind of guy that can be worked on, he can be worked on by Fowler, he can be worked on by Bahcall, OK?

Pinch: Worked on in what sense?

J: They treat him just as a technician, he does measurements, they are going to do the theory. OK, I won't accept that.

We should be cautious in interpreting J's comments in too literal a manner — he has a reputation for paranoia. However, it is clear that his experience has been very different from the smooth partnership entered into by Davis and Bahcall.

Davis has always been quite happy to accept the division of labour between experiment and theory entailed within the partnership; as he told me:

This all started out as a kinda joint thing . . . and if you start that way you tend to leave these little boundaries in between. So I stayed away from forcing any strong opinions about solar models and they've never made much comment about the experiment . . . You see it's done in a broader way in high-energy physics.

It is interesting that Davis draws here on the analogy of high-energy physics where arguably there exists the most rigid division between experimentalists and theoreticians. However, in high-energy physics the division is well institutionalised and it is possible for the theorist and experimentalist to go their separate ways. In this case there was no equivalent of the pre-existing accelerator which the experimentalist could use to produce data for the theoreticians to interpret. Both groups had to work closely together to bring the detector into existence at all. This required the special partnership which I have outlined. The difference between the solar-neutrino project and other projects from the theoreticians' point of view can be seen from Bahcall's comment that:

First of all the feasibility of the experiment was a joint project. . . . A lot of times you do theory and someone else goes away and does the experiment but in my case I invested such a large amount in the experiment and . . . I felt that, as I continued to do that, that I wanted to sell the experiment, that is to get funding for it so it would happen.

The lack of a pre-existing apparatus meant the theoretician had, in this case, to engage in more activities than usual. He had, not only to make the theoretical predictions, but also to 'market' the experiment to ensure it got funded. The economic metaphor adopted by Bahcall to describe his activities ('selling') is particularly appropriate in this case. He, and Davis, had to persuade the wider community that it was worth putting up the necessary half-million dollars.

Conclusion

Experiments such as that carried out to detect solar neutrinos are probably exceptional. Most experiments of this 'one-off' type are not as expensive. However, the special nature of the experiment and the concomitant social relationships is useful for illustrating aspects of scientific activity not usually

visible in other cases. In particular the joint investments made have become explicit. There is no reason why, in principle, the investments documented here should not happen in much more mundane pieces of science. Indeed Latour and Woolgar (35) document several such cases in their study.

What I think the solar-neutrino case illustrates well is the connection between the conditions for the production of scientific knowledge (the Popperian context of discovery) and the epistemological status of the end-product (the Popperian context of justification). The virtue of the notion of credibility is that it combines what have traditionally been seen as epistemological concerns with the concerns of sociology and psychology. If scientists are regarded as attempting to maximise their credibility by making investments then the outcomes of their investments are likely to be influenced by the pre-existing investment strategies (36). Scientists, like good businessmen, choose their investment strategies carefully in the expectation of a favourable outcome which will enhance their credibility. The production of credible information – information which, in epistemological terms, is some approximation towards scientific truth – can be seen as stemming from the previous investments of credibility.

This relationship is particularly clear in the solar-neutrino case because of the huge size of the investments made by all parties (the experimenter, the theoretician and the wider community who put up the money). It seems *a priori* unlikely, with such large amounts invested in the project, that it could be allowed to fall into disrepute. The credibility of the experimental result was in a sense guaranteed by the previous investments of credibility (37).

Clearly with scientific experiments becoming more and more complicated and expensive the associated investments will be much larger. It is likely that in order to minimise risks only scientists with a comparatively large amount of previously acquired credibility will be encouraged to take part in such projects. With such projects likely to be too expensive to duplicate, scientific advance may increasingly depend on the outcome of single experiments. However, in view of the size of the investments and the lack of competition it is increasingly unlikely that the results produced in such experiments will be subject to dispute. Just as a few multi-national corporations can dominate business markets and challenges to the credibility of their products become harder, so too may the market for credible information become dominated by large and expensive projects which produce almost guaranteed credible

information. Perhaps in the solar-neutrino experiment we can see something of the likely character of the physics of the future.

Acknowledgement

I am grateful to all the scientists who gave up their time to me, especially Ray Davis and John Bahcall.

Notes and References

1. I am thinking here of the work of Collins, Harvey, Pickering and Travis. 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* 9, 205–224 (1975); B. Harvey, 'The Effects of Social Context on the Process of Scientific Investigation: Experimental Tests of Quantum Mechanics', this volume; A. Pickering, 'The Role of Interests in High-Energy Physics', this volume; and G. D. L. Travis, 'Constructing Creativity: The 'Memory Transfer' Phenomenon and the Importance of Being Earnest', this volume.
2. These differences have been documented by a variety of authors. See, for example, M. J. Mulkey and A. T. Williams, 'A Sociological Study of a Physics Department', *British Journal of Sociology*, March 1971, 68–82; J. Gaston, *Originality and Competition in Science*, University of Chicago Press, Chicago, 1973; W. O. Hagstrom, 'Competition in Science', *American Sociological Review* 39, 1–18 (1974); I. Mitroff, *The Subjective Side of Science*, American Elsevier, New York, 1974; and A. Bitz, A. McAlpine and R. D. Whitley, 'The Production, Flow and Use of Information in Research Laboratories in Different Sciences', British Library Report Series, London, 1975.
3. A cross-section of philosophical articles on the connection between theory and observation can be found in R. E. Grandy (ed.) *Theories and Observation in Science*, Prentice-Hall, New Jersey, 1973.
4. See K. R. Popper, *Objective Knowledge*, Clarendon Press, Oxford, 1972.
5. The original idea was proposed by B. Pontecorvo and L. W. Alvarez.
6. Interviews with Ray Davis were conducted at the Brookhaven National Laboratory on October 17, October 22–23 and December 6, 1978.
7. K. R. Popper, *The Logic of Scientific Discovery*, Hutchinson, London 1959.
8. Bruno Latour and Steve Woolgar, *Laboratory Life*, Sage, Beverley Hills, 1979.
9. W. O. Hagstrom, *The Scientific Community*, Basic Books, New York, 1965.
10. Pierre Bourdieu, 'The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason', *Social Science Information* 14, 19–47 (1975). See also, K. D. Knorr 'Producing and Reproducing Knowledge: Descriptive or Constructive?', *Social Science Information* 16 (6), 669–696 (1977).
11. E. M. Burbidge, G. R. Burbidge, W. A. Fowler and F. Hoyle, 'Synthesis of the Elements in Stars', *Reviews of Modern Physics* 29, 547 (1957).

12. Letter from William Fowler to Ray Davis, January 7, 1958.
13. Interviews with John Bahcall were conducted at the Institute for Advanced Study, Princeton, October 20–21, and December 4, 1978.
14. Letter from John Bahcall to Ray Davis, November 20, 1962.
15. Letter from Ray Davis to John Bahcall, December 20, 1962.
16. Letter from John Bahcall to Ray Davis, January 3, 1963.
17. J. N. Bahcall, 'Solar Neutrinos. I. Theoretical', *Physical Review Letters* 12, 300–302 (1964); and R. Davis, Jr., 'Solar Neutrinos. II. Experimental', *Physical Review Letters* 12, 303–305 (1964).
18. See for instance, J. C. Hardy, and R. I. Verrall, *Physical Review Letters* 13, 764 (1964), and P. L. Reeder, A. M. Poskanzer, and R. A. Esterlund, *Physical Review Letters* 13, 767 (1964).
19. Letter from Ray Davis to John Bahcall, January 21, 1964.
20. 'Erratum', *Scientific American* 212, 8 (April 1965).
21. For a discussion of the experimental reception of Weber's work see Collins, 1975, *op. cit.*, Note 1.
22. See Travis, 1981, *op. cit.*, Note 1.
23. For an informative account of the experimental possibilities in 1966 see F. Reines, *Proceedings of the Royal Society of London*, Series A, 301, 159 (1966). Tom Jenkins of the Case Institute, Ohio, had also built a detector. However, this detector never worked properly due to a large background (thought to originate from the instrument itself).
24. Davis's apparatus had been built in the expectation that he would be able to go a factor of ten below the theoretical prediction of 1964.
25. Interview with F. Reines, University of California, Irvine, November 21, 1978.
26. Bourdieu, 1975, *op. cit.*, Note 10.
27. Letter from Ray Davis to John Bahcall, February 16, 1968.
28. Letter from John Bahcall to Ray Davis, February 26, 1968.
29. R. Davis, Jr., D. S. Harmer, and K. C. Hoffman, 'Search for Neutrinos from the Sun', *Physical Review Letters* 20, 1205–1209 (1968); J. N. Bahcall, N. A. Bahcall and G. Shaviv, 'Present Status of the Theoretical Predictions for the ^{37}Cl Solar-Neutrino Experiment', *Physical Review Letters* 20, 1209–1212 (1968).
30. F. Reines and V. Trimble (eds.), *The Irvine Conference on Solar Neutrinos*, 1972.
31. K. C. Jacobs, 'Chemistry of the Solar Neutrino Problem', *Nature* 256, 560–561 (1975).
32. B. Banerjee, S. M. Chitre, P. P. Dvakaran and K. S. V. Santhanam, 'Polymerisation and the Solar Neutrino Problem', *Nature* 260, 557 (1976).
33. K. C. Jacobs, 'Jacobs Replies', *Nature* 260, 557 (1976).
- 33a. This test involved creating ^{36}Ar by the decay of ^{36}Cl in a tank of perchloroethylene doped with ^{36}Cl atoms. ^{36}Ar should behave chemically in the same way as ^{37}Ar , and thus the extraction of the correct number of ^{36}Ar atoms indicates that trapping is ruled out.
34. It should be noted though, that such attacks rarely appear in the literature. Harry Collins informs me that Weber's experiment has been debunked informally in this manner.
35. Latour and Woolgar, 1979, *op. cit.*, Note 8, Chapter 5.
36. A similar argument has been made in T. J. Pinch, 'What Does a Proof Do if it

Does Not Prove?', in E. Mendelsohn, P. Weingart, and R. D. Whitley, *The Social Production of Scientific Knowledge*, Sociology of the Sciences, Vol. 1, Reidel, Dordrecht, 1977, 171–215.

37. I would not like to suggest that a deterministic relationship exists. It is always possible that Davis's experiment could have lost (and indeed still might lose) credibility. The market for credible information, like other markets, can always be subject to social contingency.