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THE DEVELOPMENT OF SOLAR-NEUTRINO

ASTRONOMY: THE THEORETICAL-EXPERIMENTAL

NEXUS AND THE SOCIAL DECONSTRUCTION

AND CONSTRUCTION OF KNOWLEDGE

Submitted by TREVOR PINCH

for the degree of Ph.D.

of the University of Bath

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SUMMARY

This thesis is a study within the sociology of knowledge. It takes as its focus developments in solar-neutrino astronomy. The empirical material presented is drawn from interviews, correspondence and scientific articles.

Solar-neutrino astronomy is still in its infancy, and much of the attention over the period covered (1958-78) is centred upon the activities of one experimental and one theoretical group. The detailed study of the interaction of these two groups in their efforts to get a solar-neutrino detector built and in their efforts to interpret and understand the results produced by this detector forms the core of the thesis. Detailed processes of the social construction of scientific knowledge and, in particular, the roles played by theory and experiment, are outlined. An attempt is made to explain how particular pieces of knowledge became accepted as 'true'. In other words, the social processes whereby consensus emerged over the validity of particular scientific findings are investigated.

As well as attempting to show how scientific knowledge gets constructed, it is also shown how what is taken to be 'true knowledge' can be deconstructed such that its roots in the social world rather than the natural world can be recovered. The interpretative flexibility at the heart of both experimental results and theoretical predictions in the domain of solar-neutrino astronomy is revealed. It is shown that scientific knowledge is thoroughly socially constituted.

In addition to attempting to provide some understanding of the social processes of science, the study presents much rich descriptive material on how this particular area of science developed. Such material may be of interest to historians of science.

CONTENTS

N.B. A brief outline of each chapter is given at the start of each

chapter. Summaries of the chapters are to be found pp.34-7 and pp.369-74.

PREFACE

The work presented within this thesis falls within a body of
research which can broadly be termed 'relativist'. In such
research scientific findings are treated as if they are not con-
strained in any way by the natural world. Several caveats about
the nature of the relativist position being argued here may be
helpful.

Firstly, the research has been carried out in the spirit of
'normal science'. That is, it has been assumed that the relativist
position has been defended adequately enough in the literature for
there to be no need to undertake further defensive exercises. In
other words, it is maintained that relativism is not self-
contradictory, and that this has been shown to be so.

Secondly, just as relativism cannot be proved to be false,
neither can it be proved to be true. It is difficult to see how
any position in epistemology could be proved by empirical work.
Thus, this piece of work is not intended as a proof of relativism.
It is a further exemplification of how relativism might give us
increased understanding of the social processes of science.

Although not intended as a proof, it is hoped that this work
does carry some conviction. It is claimed, for instance, that the
relativistic position adopted here is <u>consistent</u>. That is, in
principle, every piece of scientific knowledge could be deconstructed
such that it would appear to have no constraint placed on it by the
natural world. This argument is made on hypothetical grounds. In
other words, it could be shown that results which seem to stem
unproblematically from Nature can be interpreted differently if
actors were sufficiently ingenious. Although the hypothetical
argument could be made in every case and hence would ensure
consistency, it would be worrying if this was the type of argument
upon which we always had to rely. Thus, the relativist position
would engender a sense of unease if the main experimental knowledge
claim encountered in this thesis - Davis's solar-neutrino results -
could only be deconstructed by hypothetical arguments. However,
as will be seen in Chapter 7, Part II, Davis's results can be

deconstructed, not by hypothetical arguments but by the consideration
of the actual technical arguments made by participants (and, in
particular, one participant). Although it is not logistically
feasible to investigate all the knowledge claims encountered in
this research - each claim would require a major study in its own
right - the deconstruction of Davis's claims means that at least
the work may carry some conviction. It cannot, of course, be a
proof.

In common with other peices of work carried out within the
relativist programme it is conventional to refer to most scientific
facts encountered in the course of the study in the same unreflective
way in which they are described by participants. That is, such
facts are described as though they seem to have arisen unproblematically
from the natural world. This use of realist language should not be
misconstrued as evidence of any fundamental inconsistency as in
principle each such fact could be redescribed within the relativist
vocabulary of social deconstruction.

CHAPTER ONE

SOCIOLOGICAL APPROACH AND METHODOLOGY OF THE STUDY

In this chapter the aims and methods of the research are
introduced. The sociological approach taken in the study, which
falls within the empirical programme of relativism, is outlined
and some of its origins in the philosophy of science briefly traced.
The goals of the empirical programme of relativism are formulated
by reference to the notions of social 'deconstruction' and 'construction'
of scientific knowledge. Attention is drawn to the importance
of the differing methods which can best be used to achieve these
goals. Some recent studies in the sociology of science are
reviewed and placed in the context of the social deconstruction
and construction of knowledge. It is argued that the development
of solar-neutrino astronomy is a suitable location for the
research given its aims and methods. The major sociological
issues raised by the study and how they are approached in
subsequent chapters are outlined.

Introduction

The piece of research presented in this thesis is an empirical
study within the sociology of knowledge of an area of modern
culture - specifically scientific culture. The location of the
study within the purview of the sociology of knowledge and
culture more generally implies that there is nothing special
about scientific knowledge. For the purposes of this research
scientific knowledge is to be treated in the same way as any
other cultural product.

The case for establishing scientific knowledge as a legitimate
part of the wider sociology of knowledge, was argued by Mannheim

(1936).[1]   However, for many writers (for example, Merton, 1973)
the sociology of science has been restricted to the study of the
normative behaviour of scientists and the institutional organisation
of science rather than the technical products of scientific
activity.   It is only with modern writers,such as Kuhn (1971),
Barnes (1974), Collins (1975) and Bloor (1976),that the full
potential of a sociology of scientific knowledge has started to be
realised.

The assumption that there is nothing special about scientific
knowledge means that it cannot be said to be better or worse than
other types of knowledge in terms of, for instance, rationality,
correspondence with reality, or truth content.   To make such an
assumption is, of course, to embrace epistemological relativism.
That is it implies that no knowledge whatsoever, including the
knowledge produced by the sociology of knowledge, can be grounded
in absolute criteria of rationality, reality or truth.   The
apparent self-defeating nature of relativism and the effect that
it produces of seeming to undermine scientific knowledge as the
canonical authority for all knowledge,have meant that some writers
(notably Kuhn) have backed away from the relativistic consequences
of the new sociology of science.   For others, however (notably
Barnes, Bloor and Collins), relativism has been taken to be a
central tenet of the new sociological approach to science. Such
writers,rather than drawing back from relativism,are often self-
consciously relativist.   The issue of relativism has now become
so central, that for one author anyway (Collins, 1981a) relativism
is the defining characteristic of the new work on science.

The sociological approach followed in this study falls within

such an explicit form of relativism. However, it is not the intention here to yet again defend relativistic sociology of scientific knowledge as a viable sociological enterprise. Enough has been said elsewhere on this topic.[2] It is clear that the relativist approach has already generated several major sociological studies of scientific knowledge (see below) and it is this body of empirical work and the possibility of carrying out further work within the relativist programme which should now be the major focus of attention. What is envisaged in this study is, pace Kuhn, a piece of 'normal' relativism.

Although the work presented here is first and foremost a contribution to the relativistic sociology of scientific knowledge, it is intended to be more than just another demonstration of the relativist thesis. The major interest (and perhaps novelty) of the study lies in the detailed social processes of modern science which are documented. In particular, the interaction between theorists and experimenters in the construction of scientific knowledge, which forms the core of the empirical findings, is of great interest in terms of our understanding of the connection between theorising and experimentation in science. Indeed, the research location was deliberately chosen in order to investigate such microprocesses of knowledge construction.

Relativistic Sociology of Scientific Knowledge - The Impact of Philosophy of Science

The origins and aims of the relativist programme have recently been outlined by Collins. The origins lie in modern philosophy of science; as he notes:

> Modern philosophy of science has allowed an extra dimension - time - into descriptions of the nature of scientific knowledge.

> Theories are now seen as linked to each other, and to
> observations, not by fixed bonds of logic and correspondence,
> but by a network, each link of which takes <u>time</u> to be
> established as consensus emerges and each link of which
> is potentially revisable - given <u>time</u>. (Collins, 1981a:3).

The failure of correspondence theories of truth and the lack

of compulsion of the natural world in settling scientific disputes

has, as Collins states, been increasingly recognised by philosophers

of science. For instance, the Popperian school, and in particular

Lakatos (1970), have granted that no experimental claim can un-

problematically establish a fact of the natural world. This is be-

cause accompanying every experimental claim is a <u>ceteris paribus</u> clause

which contains all the unstated assumptions made in the experiment.

In principle, the decision what to include and what not to include

in the <u>ceteris paribus</u> clause is arbitrary and there is nothing

to stop anyone challenging an experimental claim on the grounds

that they did not accept the <u>ceteris paribus</u> clause. This has become

known as the Duhem-Quine thesis and it has drawn increasing

attention to the theory-ladenness of observations (we shall return

to the Duhem-Quine thesis below). Theoretical interpretation

and experimental evidence seem to be inseparable. The theory-

laden character of observations is a familiar theme from the work

of Feyerabend (1975) and Hesse (1974) also.

Although these philosophers of science have opened the door for

a sociological analysis of science, their own approaches do not take

us very far in terms of a relativistic sociology of scientific

knowledge. Lakatos, for instance, claimed, that in the last analysis,

there were rules of scientific rationality whereby the progress

of one research programme over another could be determined

objectively.[3] Hesse too, seems to be closer to the traditional

concerns of philosophy of science when she attempts to formulate

rules of inductive inference and talks, in her network model of

science, about the 'coherence' of the networks of theory and

observation. Feyerabend is more radical, but ultimately his rela-

tivism degenerates into an anarchistic individualism which leaves

very little room for social processes.

Perhaps the most influential writer in modern philosophy of

science has been the historian of science,Thomas Kuhn.[4]   Both in

his paper,'The Function of Measurement in Modern Physical Science'

(Kuhn, 1962),and his seminal book,The Structure of Scientific

Revolutions (Kuhn, 1971),Kuhn outlined ideas which seemed to imply

that science was similar to other areas of social and political

life. He also seemed to endorse a relativist position in respect

to scientific knowledge. Statements such as the following one,

concerning Dalton's work in chemistry, seemed to imply very little

compulsion to the natural world:

> But it is hard to make nature fit a paradigm...after accepting
> the theory they had still to beat nature into line......When it
> was done... The data themselves had changed. That is the last
> of the senses in which we may want to say that after a
> revolution scientists work in a different world. (Kuhn,1971:135)

Despite the clear resonance with the goals of relativism and

his explicit embrace of sociology of science,it has turned out to

be difficult to develop Kuhn's ideas systematically (but see Collins

and Pinch, 1982) . This is not only because of a certain lack of

rigor in the original ideas but also because Kuhn himself has

frequently reinterpreted his earlier ideas (Pinch, 1979;

1981b). Furthermore, Kuhn has recently made it explicit that the

sociological consequences of his ideas are not particularly

radical,[5] and, as mentioned earlier, he now seems to renounce

relativism altogether.[6]

The impact of this philosophical tradition, and Kuhn in particular, has probably been greatest on British sociologists of science.[7] Of particular interest are the sociologists who have interpreted Kuhn in the light of their own phenomenological, ethnomethodological, and Wittgensteinian-Winchian leanings. As mentioned in the introduction, the work of Barry Barnes (1974) and David Bloor (1973, 1976) has been particularly influential here in terms of setting out the terrain for a fully fledged relativistic sociology of knowledge. Barnes and Bloor have not, in the main, however, set about showing how such a programme could be carried out empirically - especially for the study of modern science.[8]

The empirical work on modern science carried out within the relativist approach has largely been initiated by Collins (1975, 1976) and others following similar methods (e.g.,Harvey, 1980, 1981; Pickering, 1980, 1981a; Travis, 1980 a,b,1981). Collins (1981a) has recently brought the goals of these studies together in terms of an 'empirical programme of relativism'. It is this programme which forms the starting point for considering the goals of the present research.

The Empirical Programme of Relativism - The Three Stages

In his paper setting out the 'Stages in the Empirical Programme of Relativism', Collins argues that revealing the lack of influence of the natural world in settling knowledge claims is only the first part in a three-stage programme. The second stage involves the demonstration of the mechanisms by which the potentially endless debate over knowledge claims is limited. Such mechanisms are the

social processes whereby consensus is reached that a certified fact
of the natural world has emerged. As we shall see below, I have
few quarrels with the first two stages of Collins's programme,
although I prefer to talk about the two stages in a slightly
different language.   However, Collins goes on to suggest a third
stage of the programme - a stage which no case study of modern
science has yet addressed.   This is the need to relate essentially
local debates over knowledge claims in science to wider social and
political structures and processes. Furthermore, Collins argues
that it would be most satisfying to see this third stage carried
through for a piece of knowledge belonging to mainstream science
with substantial institutional autonomy.

Whilst agreeing with Collins that recent case studies carried
out within the relativist programme have shown that the 'consensual
interpretation of day-to-day laboratory work is only possible
within constraints coming from outside that work' (Collins, 1981a:7),
I would disagree with his implication that such constraints are
ultimately to be found in the wider political and social structure.
Such constraints,I would argue, are located mainly, although not
exclusively, within the internal workings of science. That is
consensus is usually reached by social processes within science
which are to a degree autonomous of the wider social and political
structure.[9] Whilst wider social and political factors may have
impinged directly on scientific knowledge more often before the
professional autonomy prevalent in modern science emerged, to show
the influence of such factors on a concrete piece of knowledge
within an area such as modern physics is a very unpromising task.
This is not to deny that such cases might, in principle, be found,

but it would be foolish to orientate research around the search for such factors.[10] In other words,Collins's third stage is largely redundant for the study of an area such as modern physics. There is, of course, always the possibility of some weak sense of external constraint in that science <u>as an institution</u> is dependent upon the wider social and political structure. This last point, however, is largely uncontentious and presumably is <u>not</u> what Collins had in mind.

The position being advocated here is thus very similar to that favoured by Kuhn (1971) who suggests that, once a mature science is set upon its developmental cycle of paradigms, then it is largely immune to extraneous social factors.[11] I am somewhat more of an externalist than Kuhn, however, and reserve the possibility that new fields of scientific endeavour and present areas of science, earlier in their history, could be permeated by wider social and political factors. Modern physics, though, is not the area where such factors are likely to be found to be important,except perhaps in exceptional circumstances.[12]

The argument against the relevance of the third stage of the empirical programme in the context of a study of modern physics places more emphasis on the second stage - that is the demonstration of the mechanisms which limit scientific debate in the absence of external social and political constraints. In other words, these mechanisms alone should usually be sufficient to explain the emergence of a consensual fact of the natural world. The new emphasis on the second stage leads to a reformulation of the programme in terms of a slightly different vocabulary from that favoured by Collins. I prefer to describe the aims of the first

stage as the task of <u>social</u> <u>deconstruction</u> of knowledge. That is

the sociologist must 'deconstruct' what appears to be an immutable

fact of the natural world and show that it is socially contingent.

In the second stage, the aim is to show how this socially contingent

fact has been <u>socially</u> <u>constructed</u> as a seemingly immutable

scientific fact.[13]

These new terms of 'social deconstruction' and 'social con-

struction' are the language with which I shall describe the tasks

of the first two stages of the empirical programme in the relativ-

istic sociology of knowledge. The terms will be elaborated upon

further below but first it will be explained why it is convenient

for the purposes of this research to reformulate the first two

stages of Collins's programme in this way.

<u>The Different Methodological Imperatives of the First and Second</u>

<u>Stages</u>

The shift in vocabulary indicates a different emphasis in the

way that science is to be approached methodologically within the

relativist programme. The importance of methodology can be seen

from a closer examination of the first two stages of Collins's

programme. One of the paradoxes of the programme (which seems,

thus far, to have gone unnoticed) is that the first two stages are

best carried out with the use of different methodologies.

In order to show the lack of compulsion of the natural world -

the task of stage one - there is little doubt that the most powerful

methodology is that of the contemporaneous study. For instance,

by going out in the field and interviewing scientists as they argued

over knowledge claims, Collins, and others, have been able to show

that experimental outcomes are socially contingent. However, the

second stage of the programme - the investigation of the mechanisms whereby the potentially endless debate is settled - is, I would claim, better suited to the historical, rather than the contemporaneous study. This is simply because, as Collins himself points out, each link (in the network of scientific theories and observations) 'takes time to be established as consensus emerges and each link of which is potentially revisable - given time' (Collins 1981a: 3). In other words, consensus takes time to form, and if we wish to elucidate the mechanisms whereby this consensus has been arrived at, then it seems inevitable that we should look at a time span of scientific events - that is we should do history.

Now clearly the division between historical methods and contemporaneous sociological methods implied here is not absolute. For instance, the contemporaneous study might produce significant clues as to what the relevant historical processes were and anyway could be repeated every few years so that, in effect, chronological cuts along the path of some scientific development were taken. In this way the mechanisms whereby consensus was reached could be elucidated as they emerged with each new set of data (this seems to be the method which Collins himself has employed in the gravity-wave case).[14] Indeed, such a series of cuts would actually entail a historical method for, by the time consensus emerged, the earlier interview data would constitute historical records in their own right.[15] However, since most investigators do not possess the resources for such repeated surveys (or cannot wait around long enough for consensus to emerge) it is perhaps methodologically more parsimonious to rely on other historical evidence which covers the period of interest.

The two stages of the programme can be said to display the traditional tension between history of science and sociology of scientific knowledge. The standard criticism made against the historian of science is that his/her methods fail to reveal the interpretative flexibility of scientific knowledge because, by the time the historian arrives on the scene, consensus has already emerged. On the other hand, the historian's riposte to the sociologist is that sociological methods are not capable of elucidating the historical processes (processes which occur over a period of time) which shape scientific knowledge. In view of this tension within the empirical programme we need to be careful about which stage of the programme any particular piece of research addresses, and how best the aims of the research may   be achieved given the available methods.

The argument presented earlier was that the second stage of the programme was the part to which greatest emphasis should be attached in the context of the present research. The identification of the mechanisms of consensus formation is probably the outstanding problem of the relativist programme. And, as argued immediately above, the goals of the second stage are best suited to investigation with the use of historical methods. However, this does not mean that we should embrace orthodox history of science with the attendant danger of losing sight of the aims of stage one of the programme. In order to avoid such a danger, we should, it is claimed here, self-consciously choose a particular type of historical method suited to the overall concerns of the relativist programme. As it is not a type of history which has been carried out by traditional history of science, and indeed most historians of science would probably not recognise it as history, its aims will be spelt out explicitly.

A Methodological Compromise

Inevitably any real research involves compromise, including compromises in methods. It has already been pointed out that the first stage of the programme is best suited to contemporaneous methods. The claim here is that a feasible compromise between con-temporaneous and historical methods can be reached such that neither task of the relativist programme is neglected. This compromise depends on the choice of a particular location for the research.

In order to see where such research can best be located let us first ask what conditions must be met in order to carry out research on the social construction of scientific knowledge (i.e., the goal of stage two) - which is the primary aim of the present research. Then, having seen what restrictions this places on the choice of the research site, we can see how such a study might also approach the social deconstruction of scientific knowledge.

There seems to be only one important condition to be met in choosing the location for research on the social construction of knowledge and that is that we must choose a case sufficiently distant (in terms of time) for consensus to have emerged.[16] If such a consensus has not yet emerged, then it seems difficult to see how we could make any compelling statements about the processes of knowledge construction, since we will not know how the consensus we wish to explain has turned out.[17]

If this is the only constraint then there is nothing to stop us locating the research in the immediate past. This is because consensus (in modern physics, anyway)[18] can emerge very quickly. It seems to be at the most a matter of years or one or two decades.

This means that if we choose to limit the study to around the period

from when the knowledge claim was first put forward to the moment

when consensus emerged, then it is possible to locate the research

in the recent past rather than distant history.

The advantage of focussing on the recent past in terms of the

deconstruction task (i.e., the first goal of the relativist programme)

is that the period of interpretative flexibility is sufficiently

recent for there to be some hope that it can be recaptured. Perhaps,

for instance, it is possible for the scientists to relive the

episode in the context of an interview. Although consensus has emerged,

it may have emerged sufficiently recently for the scientists to be

used as an interpretative resource for the purposes of showing the

deconstruction of scientific knowledge. Of course the usual

historical records (e.g., correspondence) and publications are

available as well, and these, if interpreted with care, can also

be used to deconstruct knowledge.[19] In other words, with the

choice of this particular location it is to be hoped that enough

material can be gathered to deconstruct scientific knowledge.

It is the focus on the _recent past_ which makes this type of

history novel for historians of science. Even the most modern

period studied within the oral-history tradition seems to be pre-

1960 (in physics, anyway).[20] However, here we are talking about

events of the 1960's and 1970's. The lack of interest shown by

historians of science in the very recent past is surprising for

it would seem that there are greater amounts of primary material

available than for more distant episodes. For instance, nearly

all the scientists involved are still alive and can be interviewed

and their correspondence files copied.[21] Also individuals, who,

in hindsight, might be seen to have a key role to play in the social

processes of consensus formation, but who are not themselves important enough to leave historical records behind, can be interviewed.[22] Other historical records which might soon be discarded or destroyed can be recovered, and scientists' memories can be tapped for information as to where the relevant records are located.[23]   All in all, more, and better, data can be gathered than for a more distant episode.[24]

These advantages in terms of materials[25] are, however, just a bonus; the real purpose of focussing on the recent past, as emphasised above, is that it facilitates the task of social deconstruction.[26]

The reason for the new terminology of 'social deconstruction' and 'social construction' should now be apparent. It has been argued that the emphasis that I wish to place on stage two of the empirical programme is best suited to historical methods. This means that scientific knowledge is approached <u>after</u> consensus has emerged and the task is to show how what appears to be a solid piece of knowledge can be 'deconstructed' in order to recapture its interpretative flexibility. Once this deconstruction has been achieved, the social processes whereby a socially contingent piece of knowledge becomes 'constructed' as a hard-and-fast fact of the natural world can be shown.[27]   With the adoption of this terminology and the above methods there seems to be no insurmountable difficulty to carrying through a piece of research addressed mainly to stage two of the programme but which, nevertheless, does not neglect the first stage of the programme.

Before the location for the present research is considered, the concepts of social deconstruction and social construction of

knowledge will be further illustrated by discussing them in the

context of other recent work in the sociology of scientific knowledge.

Social Deconstruction and Construction of Knowledge - A Review of

Some Empirical Studies and Approaches

The empirical study carried out within the relativist programme

which is of most relevance to the present research is Collins's (1975)

study. I will briefly review this piece of work in terms of the

notions of social deconstruction and construction.

In his 1975 study,Collins monitored the attempts by scientists

to establish a new fact of the natural world.   This purported fact

was the claim made by Joseph Weber in 1969 to have detected large

fluxes of gravitational radiation. When the claim first appeared,

other experimenters also built detectors to search for gravity waves.

By interviewing most of the scientists involved in this experimentation,

Collins was able to show that the experiments alone did not seem able

to offer a conclusive outcome as to whether or not large fluxes of

gravity waves existed. In short, it was found that different experi-

ments were interpreted in different ways by the different scientists

involved. This led him to conclude:

> As far as can be seen there is nothing outside of 'courses of
> linguistic, conceptual and social behaviour' which can affect
> the outcome of these arguments, and yet this outcome decides
> the immediate fate of high fluxes of gravity waves......
> (Collins,1975: 220).

Collins's article was essentially an empirical verification of

the Duhem-Quine thesis.   He showed that what philosophers of science

had considered to be an abstract problem was actually encountered

by scientists in their efforts to establish new knowledge. The

vehicle used to carry the argument was that of 'experimental

replication'. He showed that what counted as 'competent' replications

of an experiment varied amongst different scientists. This can be seen to stem directly from the Duhem-Quine thesis. Inevitably all experiments differ in some respects from each other. For instance, they are often carried out at different times and places by different experimenters. Part of the ceteris paribus clause attached to experimental claims is that such factors which differ between experiments are assumed to be irrelevant. However, as pointed out by Duhem and Quine, in principle the ceteris paribus clause can be challenged. Hence scientists can deny that any experiment is a competent replication of another because they can claim that such-and-such a variable, assumed under the ceteris paribus clause to be irrelevant, is actually very important. As an extreme example take mind-over-matter; clearly all modern physics experiments assume ceteris paribus that psychokinetic effects are unimportant. However, if another assumption is made and such effects are considered to be real, then all experiments which do not control for such effects can be deemed 'incompetent'.

Collins's argument was that, when experimental activity at the frontiers of science is monitored, then scientists can actually be seen to be challenging ceteris paribus clauses. Definitions of competence could be shown to be based on assumptions about how the natural world works. It is Collins's view that these assumptions are embedded in the web of scientific culture. In other words, experimental outcomes do not demonstrate the compulsion of the natural world but rather reflect cultural presuppositions. (Culture here is interpreted in the Wittgensteinian-Winchian sense - see Collins and Pinch ,1982).

Collins's study showed the lack of compulsion of the natural

world in settling experimental claims and it suggested that the
arguments over experiments were settled in terms of 'linguistic,
conceptual and social behaviour'. However, it did not spell out
any of the social processes whereby certain sorts of linguistic,
conceptual and social behaviour became sanctioned.   Collins's
1975 study was an exemplary case of the social deconstruction
of knowledge but he did not take the task of social construction
very far.   That is he did not have much to say about the mechanisms
whereby consensus emerged.

This method for the social deconstruction of knowledge has
now become familiar from a number of other studies (e.g., Collins,1976;
Wynne, 1976; Travis, 1980a,b, 1981; Harvey, 1980, 1981; Pickering,
1980, 1981a; Collins and Pinch, 1982). Although the findings
of these studies often differ in matters of emphasis and detail,
they all tend to deconstruct knowledge by showing the variety of
interpretations of knowledge claims which are available.   The
production of scientific knowledge has, in a number of cases now ,
be consistent with the view that it has
been shown to very little constraint imposed on it by the
natural world.

The approach to the social deconstruction of knowledge followed
in the above studies is most fruitfully located within the arena
of scientific controversy. Examples of the interpretative flexi-
bility of scientific knowledge seem to be relatively easy to come
by during a period of controversy. However, in principle, the
approach can be pursued into less controversial areas, although
interpretative flexibility is much harder to recover in such
cases.   One development in this type of analysis for non-contro-
versial areas has been the use of what might be called the 'hypothetical

argument' (Collins, 1981b; Harvey, 1981). In such cases, scientific

arguments of a hypothetical nature are constructed by the socio-

logist which could, with some plausibility, have been proposed by

the real actors studied. These arguments are designed to show that,

even when consensus has started to develop against a particular

knowledge claim, it is possible to defend the claim on a hypothetical

basis. That is, with the use of such arguments, it is possible to

recover some of the interpretative flexibility of knowledge.

There are other sociological approaches which also deconstruct

scientific knowledge. For instance, one such approach is that

followed in 'anthropological' or 'constructivist' studies whereby

the production of scientific knowledge is followed through in one

setting, such as a particular laboratory (Latour and Woolgar, 1979;

Knorr, 1977). Others have followed more traditional ethnomethodological

concerns and have analysed the processes whereby scientific knowledge

is produced through the vehicle of discovery accounts (Woolgar, 1976,

1980; Garfinkel et al., 1981).[28] Another approach which achieves

the social deconstruction of scientific knowledge is that of 'dis-

course analysis' (Gilbert and Mulkay, 1980; Mulkay, 1981; Mulkay

and Gilbert, 1981).

These approaches, although often following divergent goals and

methods, can be said to share the theme of the deconstruction of the

natural world facticity of scientific knowledge. They do this by

virtue of their focus on interpretative features common to all

human activity such as 'reading', 'accounting' and 'talk'. Their

emphasis on these interpretative procedures leaves no specially

reserved place for the natural world. Scientific facts are socially

constituted because the processes of science are fundamentally

interpretative ones. Whether the processes involve the interpretation
of 'literary inscriptions', 'discovery accounts' or 'available
repertoires of discourse' they are essentially social processes.

The studies referred to immediately above have one methodol-
ogical factor in common. They choose to research individual and
isolated parts of the scientific process, such as the laboratory
(Latour and Woolgar, Knorr), the 'discovery account' (Woolgar,
Garfinkel et al.) or the 'interview transcript' (Gilbert and Mulkay).
This chosen location is often ideal for the acquisition of the
detailed data needed in order to show the richness of science's
interpretative processes. However, such locations are unsuited
to carry through the second task of the relativist programme – the
social construction of knowledge. As will be argued below, such
locations are not suitable for showing the processes of social
construction of knowledge as such processes are not to be found
in any single laboratory, or discovery account. Neither are they
to be found in interview transcripts – at least not in interview
transcripts which are obtained for the purposes of discourse
analysis.[29]

Of course, in arguing that the restricted location of the above
studies makes them unsuitable for the purposes of showing the
processes of social construction of knowledge, I am implying that
the place where such processes are to be found is known. It seems
clear from studies carried out thus far within the empirical programme
of relativism (see below for details) that the appropriate location
for the identification of these processes is the group of scientists
who make experimental or theoretical contributions to the area
of knowledge being investigated. Collins has usefully termed such

groups as Core-Sets (Collins, 1981c). The Core-Set are a group

of scientists who, in a controversy, are 'actively involved in

experimentation or observation, or make contributions to the theory

of the phenomenon, or the experiment such that they have an effect

on the outcome of the controversy'. I would agree with Collins

that it is the Core-Set who decides; however, we should not neglect

the point that the social processes of relevance are also located

in the institutions of science, and, in particular, in the insti-

tutional resources which the Core-Set scientists have at their

command (this will become clear in Chapter 10).

Given this location for the consensus-forming mechanisms, it

would seem that the 'constructivists' 'ethnomethodologists' and

'discourse analysts' have not drawn their net wide enough to give

any definitive account of such mechanisms.[30] Although their

studies, if reinterpreted, can perhaps fill in details and supply

valuable leads, they are in themselves too narrow in their focus

to be used for the second task of the relativist programme.[31]

However, it can be seen that the location chosen for the

studies of the deconstruction of knowledge mentioned earlier (e.g.,

the Collins et al. studies) , which focus on controversies and in which all

the relevant scientists are interviewed, is more promising for the

study of consensus-forming mechanisms. This is because it is

amongst these same scientists that consensus develops. In other

words, the scientists Collins interviewed for his gravity-wave

study are the same scientists who decide whether gravity waves

are or are not facts of the natural world. These studies take us

further than the constructivists, ethnomethologists and discourse

analysts because they at least look in the right place.

Although stage two of the empirical programme has often not been the prime goal of the controversy-type studies,they have made important contributions towards elucidating mechanisms of social construction of knowledge. In this section,I will be reviewing those consensus-forming mechanisms which have been identified in empirical studies of modern science, and, in particular,physics.[32] There is, of course, no shortage of theoretical ideas on the topics. Some of this work will be discussed in Chapter 10, but here I wish to maintain the empirical 'feel' and momentum of the relativist programme as it has been applied to modern physics.

One of the earliest studies of relevance was that made by Wynne (1976) of the reception of Barkla's J Phenomenon. Wynne's main concern was social deconstruction and,in particular,to show the lack of compulsion of attempts to find scientifico-rational grounds for rejecting Barkla's claims. The study,however, is also instructive in that Wynne identified specific social factors which led to Barkla's ultimate rejection. He pointed to Barkla's failure to meet 'localised social interests', namely ' a systematic programme of obviously attractive and promising questions' for research (Wynne, 1976: 36). Wynne also pointed to the importance of ritualised forms of rationality in 'scientifically' rejecting Barkla's claims.

The theme of social interests is developed further by Pickering (1980) in his study of the acceptance of the charm model in high energy physics and the demise of the colour model. Pickering's main explanatory vehicle for consensus formation is the 'interest model'. This is a notion which has been found to be useful in some historical (distant-history type) case studies.[33] Pickering's

argument is that the charm model was more successful than its rival because it was more closely aligned with the pre-existing theoretical interests and expertise of groups of scientists. These interests could be connected with the work of the charm and colour proponents by the exemplary achievements of their different models. The charm protagonists managed to construct exemplars which intersected with the theoretical interests of these groups whilst the colour protagonists could not.

Pickering's study is important for its focus on theoretical developments. However, it does have one weakness to which Pickering himself draws attention. It neglects altogether the activities of experimenters. Clearly in terms of gross materials (i.e.,money) experiments require far more resources than theory, and, if consensus-forming mechanisms are connected with the attempts to procure such resources, then experimental activity will also be an important place to look for the relevant mechanisms.

The criticism of the lack of emphasis on experiment cannot be levelled at Pickering's (1981a) study of the debate over the detection of magnetic monopoles, for this was primarily an experimental controversy. Again it is the mechanisms of closure of the debate which are at the centre of attention. Pickering's conclusion is consonant with that of his earlier study in that ultimately it is theoretical concerns which dictate how the consensus emerges. In particular,prior agreements as to what constituted acceptable experimental practice, which is in itself underpinned by theory, can be seen as the constraint on the closure. However, as Pickering himself again stresses, before we are convinced of his conclusion it needs to be shown in detail how theoretical interests win out and

also the processes whereby rival theories are rejected need further investigation. Perhaps it is the case that rival theories cannot generate a successful experimental practice, in which case experiments would seem to be more important than granted by Pickering.

Another study of modern physics which is of great relevance to the present concerns is that carried out by Harvey (1980, 1981) of the local hidden-variables experiments in quantum mechanics. Harvey showed that, despite the lack of overt controversy over the interpretation of these experiments, the experimental claims could still be deconstructed by considering hypothetical arguments. Harvey (1981) also pointed to the key consensus-forming mechanism being the monopoly of plausibility. As he puts it:

> The suggestion is that the winning side does not possess truth but rather that it has monopolised plausibility .
> (Harvey, 1981: 124).

Plausibility is to be located, not only in the predominant theory (the orthodox version of quantum mechanics, in this case), but also in such factors as 'access to experimental equipment' and 'the support of prestigious scientists'. Also, Harvey showed that the plausibility of one particular theoretical hypothesis increased once it became possible to mount a feasible experimental test of the hypothesis. In other words, Harvey suggests (unlike Pickering) that experimental activity, independent of its theoretical credibility, can in its own right act as a significant motor in the shift of scientific consensus.

There is one other aspect of Harvey's study which is of significance in the context of the present concerns. This is where he draws attention to the point that two sides in a controversy do not start off with equal amounts of plausibility. This means that

it is important to look at consensus-forming mechanisms which

exist _prior_ to the knowledge claim ever being launched. In other

words, it is not enough to study scientific action after the point

where the knowledge claim appears, we must also look at pre-existing

factors. This draws attention again to the importance of historical

studies with their greater flexibility in the time-span chosen for

analysis.

Closely linked to the notion of plausibility developed by

Harvey, and Pickering's notion of interests, is the notion of

'credibility' developed by Latour and Woolgar (1979) and Law (1980).

Instead of monopolising their plausibility scientists are seen as

maximising credibility. Successful knowledge claims are seen to be

ones which produce credible information, that is information which

serves others' interests. As this scheme has not been followed

through in detail for the social construction of a modern piece of

physical knowledge, it receives only brief mention here. A more

elaborate discussion is presented in Chapter 10.

One final study, which I wish briefly to consider, is Collins's

recent (1981b) paper on the gravity-wave episode. The prime

purpose of this paper was to show that the experimental claim for

the detection of large fluxes of gravity waves could by assiduous

sociological work, be deconstructed even at the stage when consensus

had emerged that the claim was false. However, Collins also eluci-

dates one consensus-forming mechanism. This is in connection with

the activities of an experimental group who played an important

part in the demise of the original experimental claim of Weber.

Collins shows that this group's activities were orientated around

the purpose of killing off Weber's claim. Their experiment was

designed with this in mind and this group attached great importance

to publicly exposing 'mistakes' made by Weber. It seems, as Collins(1981b²48)

writes, 'as though he [the leader of this group] did not think that

the simple presentation of results with only a low key comment

would be sufficient to destroy the credibility of Weber's results.'

This finding seems to be consonant with Wynne's study where he

also showed that experimental replications were often ritualised

attempts to discredit the original claim. Collins's stress on

the public humiliation of Weber which this group sought and the

ad hominem nature of their criticism seems to fit in with the

findings of the study made of the rejection of parapsychology

(Collins and Pinch, 1979). There it was shown that publicising

and ad hominem arguments, or what we referred to as 'contingent

forum'-type activity, are key elements in the social construction

or destruction of knowledge.

In summary then, it can be said that a variety of recent studies

of modern science and, in particular, physics have started to

eludicate mechanisms of consensus formation. Such studies seem

to indicate the importance of knowledge claims falling in with

pre-existing interests or investments in experimental

and theoretical techniques. Attention has also been drawn to

the importance of publicising, manipulation of resources and

ritualised forms of scientific rationality. Inevitably as these

case studies have been carried out over the years they have tended

to focus on different aspects of the processes. As we have seen,

such case studies have also raised questions; such as the relative

importance to be attributed to theorising and experimentation in

closure mechanisms.

What is needed now is an attempt to bring some of these factors

together to try and answer these questions within one study. It is

hoped that the present study will go some way toward this. It

thus has to be shown why the solar-neutrino field is a suitable

location for such a study given the above goals.

The Development of Solar-Neutrino Astronomy - A Suitable Case for Research Within the Empirical Programme of Relativism

It will be recalled that ideally to meet the methodological

prescription outlined earlier we require a case in recent physics

where there has been a controversy over a piece of scientific

knowledge which has now been settled and where consensus has emerged.

Unfortunately the social world rarely throws up examples to fit

the neat categories of methodologists. However, the solar-neutrino

case has many of the attributes sought after. In order to introduce

the study I will give a very brief potted history of the field (a

more detailed history of the origins of solar-neutrino astronomy

and the scientific issues touched upon in the work as a whole

are presented in Chapter 2).

Neutrinos are massless, chargeless particles produced as a

by-product of nuclear reactions. Such etherial particles are very

difficult to detect, and billions upon billions pass unnoticed through the Earth

every day. Solar neutrinos are neutrinos produced in the core of

the Sun as a result of hydrogen fusion. The detection of such

neutrinos is of great importance to the field of nuclear astrophysics

because neutrinos, unlike photons (which are produced in the core

of the Sun approximately one million years before they reach the

surface) can be observed almost directly they are produced. Because

neutrinos interact so little with matter they pass straight through

the Sun. They hence provide direct information on what is happening

in the Sun's core. Furthermore,the whole discipline of stellar-

evolution theory and our understanding of the origin of the elements

is founded upon the assumption that nuclear synthesis occurs in

stellar interiors - yet this process has never been directly

observed. The detection of solar neutrinos would provide direct

evidence of nuclear burning in the Sun.

The possibility of detecting solar neutrinos and thus confirming

nuclear fusion as the Sun's motor, first became a realistic enter-

prise in 1958, when it was pointed out by nuclear astrophysicists

that a rare nuclear reaction occurs in the Sun which produces neutrinos

whose energy spectrum is sufficient for them to be detected on Earth.

It became clear, after detailed theoretical work at the Kellogg

Radiation Laboratory of the California Institute of Technology

(Caltech), that a feasible experiment would have to be very large

and costly. Raymond Davis, Jr., of the Chemistry Department of

the Brookhaven National Laboratory, had developed a suitable

experimental technique and, in 1964, he received $600,000 of

funding from the Atomic Energy Authority (AEC) to build such a

detector. The detector was based on a large (100,000-gallon)

tank of perchloroethylene (containing chlorine-37 atoms) with which

incoming neutrinos would interact to produce radioactive argon-37.

This argon could be extracted from the tank and the amount formed

measured from its characteristic radioactive decay. By August 1967

Davis had built a suitable detector in a gold mine in Lead, S. Dakota

(a mile of rock shields the tank from cosmic rays which can also

produce argon-37).

When Davis reported his first result in August 1967 there was

immediate consternation amongst the theorists as Davis seemed to
have reported a result much lower than theoretical predictions
had indicated.   At this stage there was much discussion as to
whether Davis's experiment was working correctly and whether or not
his results were in conflict with theory. By 1972, with ever-
increasing refinements in his experimental technique, Davis reported
results which were even lower. This led to what seemed to be a
crisis in nuclear-astrophysical theory with the appearance of
many non-standard theoretical schemes that were produced in order
to explain what was now widely referred to as the 'solar-neutrino
problem'. However, with the problem becoming more serious, nuclear
astrophysicists began to have more doubts about whether Davis's
experiment was working correctly. These doubts could only be
cleared up by exhaustive testing by Davis of his own experimental
procedures as no other group embarked upon experimental replication
(apart possibly from a Russian group who appear still to be con-
structing the necessary apparatus). It was not until 1978 that Davis
had mostly satisfied his critics and consensus emerged that his
experimental result was most likely a fact of the natural world.
However, by this time the gap between theoretical prediction and
experimental result had again narrowed and arguments were once
more starting to emerge as to whether or not there was a real
contradiction between theory and experiment. Finally, in 1978
new experimental approaches to the detection of solar neutrinos
were developed.These experiments are expected to cost several
millions of dollars and construction should be under way in the
1980's.

Although the title of this thesis refers to the development of

solar-neutrino astronomy, it can be seen that, as an area of astronomy, the field is still in its infancy (especially in comparison to other areas of modern astronomy). Throughout the period of interest measurements have only been made by one experimental group. Comparisons between this field and other areas studied by sociologists working on the development of scientific specialties are thus likely to be misleading (the locus classicus for comparison, if the field had taken off, would have been, of course, the Edge and Mulkay (1976) study of the development of radio astronomy.)[34] In view of only one experimental knowledge claim being the centre of attention, the area falls naturally within the province of the relativistic sociology of science tradition. However, it is hoped that some of the material presented in this thesis will be useful to future workers in the specialty tradition, especially if the field does take off in the 1980's.

The first thing to be said about the solar-neutrino case is that it clearly satisfies the methodological criterion, of being sufficiently recent - all the major events have occurred in the last three decades and most of the significant events in the last twenty years. Secondly, and more importantly, it seems that consensus has emerged that Davis's experimental result is a certified fact of the natural world. Although one or two scientists still have reservations,by and large Davis has convinced his critics. Thus the case seems suitable for the study of the social processes of consensus formation. In addition, it appears that there was a controversy over the theoretical consequences of the result when it first appeared. This controversy was, it seems, settled in 1972 with the consensus view being that there was a contradiction

between theory and experiment. The occurrence of this controversy should enable research to be carried out on some of the processes of knowledge construction especially in relation to the comparison of a theoretical prediction with an experimental result. Furthermore, the reappearance of this controversy in 1978 (when the field work was undertaken) means that the methodological bonus mentioned earlier (note 26) of carrying out a historical study and a contemporaneous study at the same time, is possible for this case.

What is not possible in this study, is the complete delineation of the consensus-forming mechanisms in the theoretical arena as, thus far, there is no consensus as to what the solution to the solar-neutrino problem (if there is such a problem) is. None of the proposed theoretical solutions have been widely accepted. This means that although data can be obtained on the social deconstruction of the standard theory it is not yet possible to make definitive statements about the social construction of theoretical knowledge, since we do not know yet what the accepted theoretical solution (if one is needed) will be. However, the continued use of the standard solar theory and rejection of non-standard theories does make it possible to comment a little on the social processes of construction of the standard theory and the social destruction of the non-standard theories.

This case thus seems to be suitable in terms of general criteria for the social deconstruction and construction of knowledge which were outlined earlier. Furthermore, the case seems to be a particularly fruitful one since it involves both experimental and theoretical knowledge claims.

There are, however, certain peculiarities presented by this

case which in some ways make it harder to research and in other ways

provide unexpected sources of insight. Firstly, on the minus side,

the social deconstruction of Davis's knowledge claim will be

especially difficult to achieve. This is largely because Davis's

experiment, is, thus far, the only one capable of measuring

the expected neutrino fluxes. Since there are no other experi-

mental groups to report possible disconfirmations of his results,

it is not possible to make a study of an experimental controversy

in the Collins mould. There are no warring factions of experimental

groups with different interpretations of the experiment as in

the Weber case. However, as we will see, it will be possible to

deconstruct Davis's knowledge claim to some extent by the consider-

ation of arguments concerning, in particular, tests of his own

experimental procedure.

Some compensation for the difficulty in the deconstruction

of Davis's experimental result has, however, been provided for by un-

expected developments in the arena of solar-neutrino theory.

One of the most fascinating aspects of the theoretical developments

studied has been the change in magnitude of the theoretical pre-

diction over the years. The various predictions in SNU (Solar

Neutrino Units) are shown chronologically in Fig. 1.1. It can be

seen that there was a peak in the prediction around 1964 which

coincided with the funding of Davis's experiment. Also there was

a significant drop in the flux around 1968 when Davis's low results

were first reported. If the view is taken that the theoretical

prediction itself is socially constructed,then it is possible

that wider factors than pure theoretical considerations may have

constrained these trends in the prediction. One obvious hypothesis
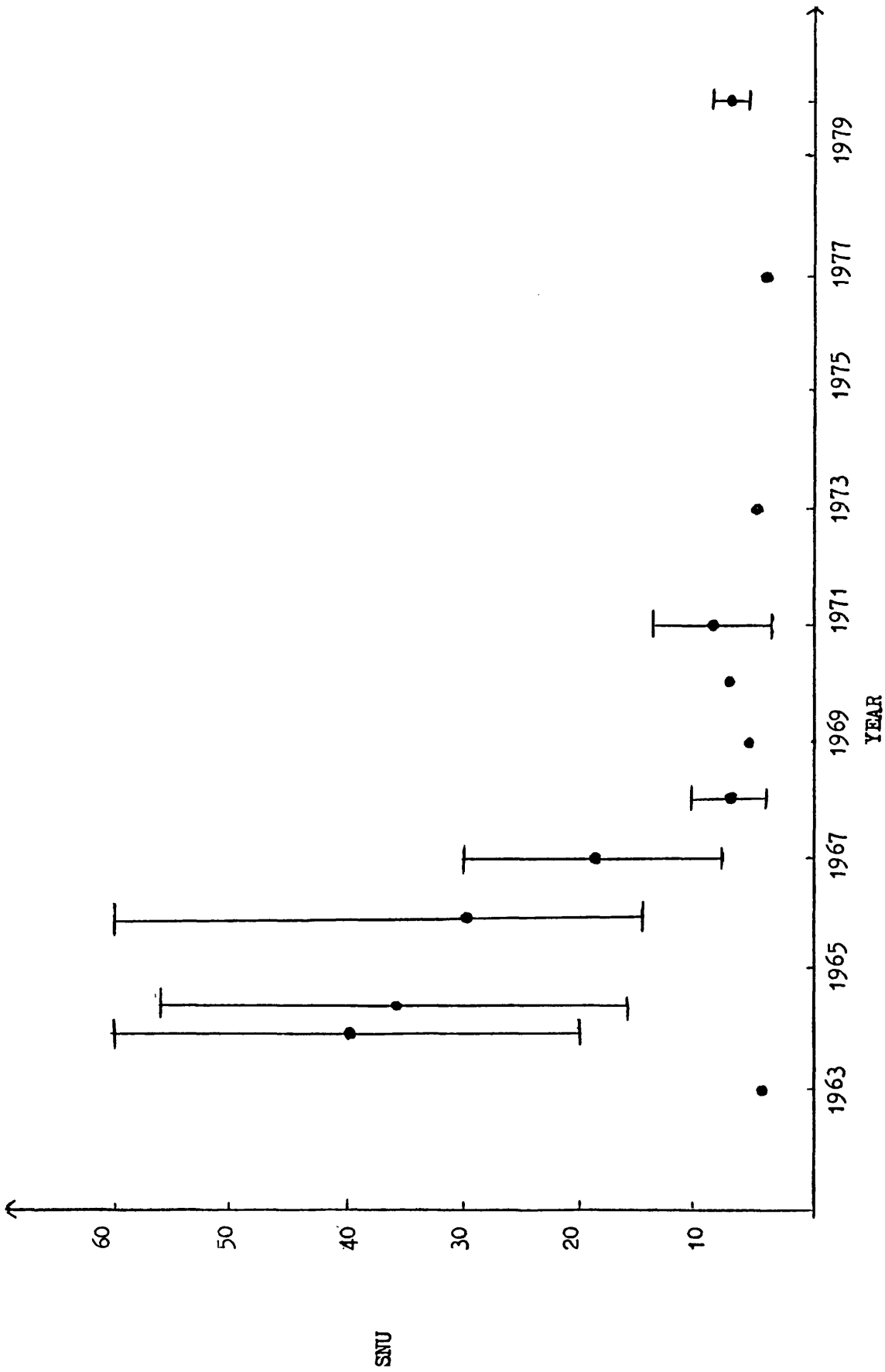
Fig. 1.1 Theoretical Prediction Over Time

(Values taken from Published work of Bahcall)

is that events in the experimental arena and, in particular, the
need to find funding for Davis's experiment in 1964 and the
appearance of his results in summer 1967, are closely connected
with the predictions at those times. As the possibility of such
a connection developed during the research, the scope of the invest-
igation was widened to include data on the funding of Davis's
experiment. Certain key figures in the funding of the experiment
were located and interviewed.

The especially close links between theory and experiment and,
in particular, the links between the Caltech group of theorists
and Davis the experimenter is one of the striking aspects of this
particular case study. The development of solar-neutrino astronomy
seems to have been at the same time the development of a special
relationship between theoreticians and experimenter. These links
are important, not only for the understanding of the social con-
struction of the theoretical prediction, but also for understanding
the reception accorded Davis's result by the theoreticians (in
other words, part of the social construction of the Davis result).
In view of the importance of the links between theoreticians and
experimenter, these links are traced from their emergence in
1958. Thus, the starting point for the historical processes of
greatest interest to this work is 1958.

The material presented in the various chapters is arranged in
a broadly chronological manner. This reflects the historical
orientation of the work. Although, at times, the minutiae of
historical description might seem rather tedious, the tracing
of the historical processes is, it seems, in its nature slow and
painstaking. The processes do not appear suddenly on the canvas
but emerge slowly over a period of about twenty years. The

description of the unwinding of the processes is I hope justified

to some extent by the narrative style which history permits.

Brief Resumé of Chapters

Chapter 2 is a naive historical reconstruction of the back-

cloth from which the events described in later chapters emerged.

The history of neutrino-detection physics and the history of

nuclear astrophysics are outlined - it was from the marriage of

these two scientific fields that solar-neutrino astronomy emerged.

It is hoped that this chapter is also useful in introducing some

of the scientific concepts encountered in later chapters.

The detailed description of experimental activity commences

in Chapter 3. The starting point is 1958 when solar-neutrino

detection, for the first time, became a feasible enterprise.

The account finishes with the funding of Davis's experiment in

1964.

Chapter 4 mirrors the time span of Chapter 3 with attention

being placed on the activities of the theoretical nuclear astro-

physicists. It is shown in detail how these activities and,

in particular,an accurate prediction of the expected flux of

neutrinos led to the funding of Davis's experiment.

Chapters 5 and 6 are both short chapters which pursue the

description of experimental and theoretical activity respectively

from the moment the experiment was funded until the apparatus was

ready to make measurements in the summer of 1967. As well as

details of the construction of the apparatus, the close relationship

between one Caltech theorist - John Bahcall - and Davis is described.

It was Bahcall who made most of the detailed predictions for

Davis's experiment.

Other methods of solar-neutrino detection, with less sensit-
ivity than the Davis experiment, which were undertaken during
this period, are also described (Chapter 5).

Chapter 7 is one of the most important sociological chapters
in the thesis. In Part I, Davis's experimental activities between
1967 and 1978 are described. The publication of his results and
the subsequent experimental refinements and tests are outlined.
Particular attention is focussed on the reception accorded to
his result by other experimenters and by the theoreticians. It is
argued that, by 1978, consensus had emerged that Davis's result
was essentially correct. In Part II, the social deconstruction of
the facticity of Davis's result is attempted. In particular the
arguments made by one astrophysicist, that Davis's results were
an artefact of his chemical procedures, are reviewed. It is
shown that these arguments, although rejected by nearly everyone
by 1978, could, nevertheless, be 'rationally' defended. Some of
the social processes whereby these arguments were defeated are
discussed.

Chapter 8 is also an important chapter in terms of the
overall aims of the study. It too has two parts. In Part I, the
immediate response of the theoreticians to Davis's results is
outlined. The attempts made by Bahcall to accommodate the result
within standard theory are described. It is shown that, in the
aftermath of Davis's results, the theoreticians were in considerable
disarray with some arguing that there was no conflict between theory
and experiment and others arguing that there was a serious discrepancy.
This suggests that 'contradiction' and 'consistency' between theory
and experiment are socially constructed. In Part II, the remaining

theoretical developments which take us up to 1978 are described.

By 1970 all the leading theorists were agreed that there was a

contradiction between theory and experiment. However, by 1976 the

discrepancy had again narrowed and arguments once more appeared

as to whether or not theory and experiment were in conflict (the

analysis of these 1978 arguments is extended in Pinch, 1980a - in

Appendix II).

As a result of Davis's measurement a number of non-standard

theoretical proposals have emerged. These are briefly reviewed in

Chapter 9. Particular attention is focussed on one non-standard

theory and it is argued that, although universally rejected, this

approach can be seen to be as scientifically plausible as the

standard theory. Some of the social processes of rejection of

this non-standard theory are discussed.

Chapter 10 is the concluding chapter and brings together the

main sociological findings of the research and compares them with

other similar work. The social processes of the deconstruction

and construction of scientific knowledge which have been encountered

are delineated. An attempt is made to account for some of these

social processes within an over-arching explanatory scheme.

Appendix I contains a summary of the main sources of data

for the study.

Appendix II contains two papers relevant to the study which

have been presented elsewhere. One paper, published already (Pinch

1981a), is an account of the contemporaneous negotiations encountered

in 1978, over which scientific field was most likely to be the culprit

for Davis's anomalous result. The paper illustrates some of the

interpretative flexibility which can be recaptured even from

seemingly solid scientific fields.   The other paper (Pinch, 1980a)

was presented at a conference.   This paper examines the contem-

poraneous argument over whether or not there is a contradiction

between theory and experiment.   In other words, it continues the

analysis developed in Chapter 8.   In particular, attention is

focussed on the use of statistical criteria in the assessment

of the significance of an experimental result vis-à-vis a

theoretical prediction. This paper thus extends the process of

social deconstruction of knowledge into the arena of statistical

argument.

NOTES FOR CHAPTER ONE

1. A useful review of the origins of the sociology of scientific
    knowledge can be found in Mulkay (1979).

2. For an exposition of many of the relevant arguments and how
    they can be addressed, see Collins and Cox (1976).

3. Whether such rules can be successfully applied is another matter.
    Since Lakatos thinks it possible that a degenerating research
    programme can always make a come-back at a later stage it
    seems difficult to say definitively when it is irrational to
    maintain belief in a research programme (see Lakatos, 1970,
    for more details).

4. With the recent translation of Fleck (1979) it can be seen
    that many of Kuhn's ideas were contained in Fleck's own
    earlier work.

5. See, in particular his introduction to his collection of essays,
    The Essential Tension (Kuhn, 1977).

6. See Kuhn's 'Postscript - 1969', in Kuhn (1971).

7. See Pinch (1981b) for a detailed review of the influence of
    Kuhn on British sociologists of science.

8. It must be noted, however, that Barnes and Bloor and the
    'Edinburgh School' have produced some exemplary historical
    studies. See, for example, Barnes and Shapin (1979).

9. Some of these social processes, as delineated in modern
    physics, will be outlined below. In my own research I
    outline the key role played by funding. However, funding
    machinations can be seen as processes which occur within
    scientific institutions. By and large campaigning for funds
    for particular projects in pure physics does not involve
    much overt politicking with factions outside the scientific
    establishment. Clearly, however, the influence on science of
    the general level of funding available is a stage-three
    process. To meet Collins's goal for stage three would involve
    showing how this general funding level influenced the
    content of particular pieces of natural knowledge - and that
    is a very difficult thing to show.

10. The argument here is not one of principle but more a matter
    of which problems are researchable in the context of a study
    of modern physics. The only study which has produced somewhat
    convincing evidence of the influence of wider social and
    political influences is Forman's (1971) study of the development
    of quantum physics in the Weimar Republic (but see Hendry, 1980,
    for some criticisms). However, this study is located in a very
    special place and time - a revolution in physics and an era of
    historical upheaval.

11. The view being argued here is the opposite of the 'Finalisation thesis'. In that thesis it is claimed that a 'mature' science is more likely to be guided by external factors. For a critique of this thesis see R. Johnston, 'Finalization: A New Start for Science Policy'. Social Science Information, 15, 1976, 331-6.

12. This position is not a new form of 'internalism'. This is because it is granted that social mechanisms and processes within science are important. Internalists would claim that scientific knowledge was free of even these processes. The dissolution of the old external/internal dochotomy within the new sociology of scientific knowledge is discussed by Johnston (1976).

13. The 'fact' might be that such and such a phenomenon does not exist - in which case we might talk about the social destruction of knowledge rather than social construction (see Collins, 1981b).

14. A similar method (although used for a differing purpose) was followed by Mitroff (1974) in his study of moon scientists.

15. The essential historical character of the   empirical work in the new sociology of science has been noted by Martin Rudwick. See his review of the conference 'New Perspectives in the History and Sociology of Scientific Knowledge.' The 4S Newsletter (available from the Sociology Department, Texas A & M University) Volume 5, No. 2, Spring 1980, p. 43. Collins's method of chronological cuts is different from most historical methods in that it is not retrospective. In the context of doing history for a PhD dissertation, however, it is inevitable that some sort of retrospective method is used. The claim is, however, that retrospection is less subject to the normal methodological difficulties when used for events which are relatively recent (i.e., within the last two decades).

16. Collins (1981c) argues that it is difficult to tell when consensus has emerged. Although it is nearly always the case that the originator and perhaps a few supporters for a rejected claim carry on the fight (sometimes until death), a round of interviews with those in the field plus knowledgable outsiders is usually enough to elucidate which way the fight has gone. Perhaps the retrospective bias of the methods advocated here is what gives the analyst the advantage in determining when consensus has emerged.

17. Of course, there is always the possibility that the historical processes of relevance only become visible many years after the consensus has emerged. The assumption here is that this is not the case. The only means of testing this assumption is for a future historian to rework the same case and see whether the wider purview leads to a new and better explanation of the consensus-formation process.

18. Whether this research location is only suitable for physics
is an empirical matter. G.D.L. Travis informs me that in
some areas of modern biology it is hard to say when consensus
has emerged - if it ever does.

19. The very existence of the contemporaneous studies has made the
task of historical deconstruction easier for we know which
types of arguments to look for. Also I am encouraged by the
existence of some excellent historical studies which also
seem to achieve the social deconstruction of knowledge (see,
for example, Shapin, 1979, and,Farley and Geison ,1974).

20. I am thinking here of the work of the American Institute of
Physics, Center for History of Physics. Although they have a
large collection of interviews in their 'Sources for the
History of Modern Astrophysics' these deal largely with
events pre-1960.

21. Of course, the availability of correspondence files depends
on the individual scientists. But as a rule of thumb it is
easier to get such material after consensus has emerged and
the scientists consider the issue to be dead. Thus a study
after the event does have some advantage here over the
contemporaneous study.

22. In the present study some interviews with funding officials
were conducted. It is difficult to see how anything but the
most rudimentary material on their decisions could be
gathered in (say) even twenty years time.

23. It was not uncommon in the present study to be told that I was
very lucky to get copies of correspondence because the
scientists concerned were thinking of throwing such 'dead'
material away.

24. Of course, there are many variables to be considered here.
For instance, very few modern scientists leave note books of
the detail contained in (say) Faraday's notebooks. However,
such notebooks are perhaps not of so much interest in the
delineation of social processes because they are limited very
much to individual thought processes. More of the social
processes are revealed through correspondence. The increased
use today of the telephone (and computer word-processer
information exchanges) means that interviews are vital in order
to obtain any data at all on some scientists.

25. Of course, 'more' of anything does not guarantee better history
unless the correct conceptual problems are addressed!

26. There is one other bonus which this methodology can produce.
It was stressed earlier that one of the core assumptions
of the relativist programme is that knowledge claims are,
in principle, revisable. This leaves open the possibility
that what was consensual at one time may, at another time,
become contentious. If the researcher is lucky (or opportunistic)

26. contd.
enough, he/she may find   contemporaneously with his/her
historical study, that knowledge, which had at one stage
been consensual, is once more becoming controversial and that
interpretative flexibility is again reappearing. In other
words, two studies can in effect be carried out at the same
time - the historical study of the previous consensus forming
mechanisms and social deconstruction of the original claim,
and the contemporaneous study of the ongoing controversy.
In such a case,the task of social deconstruction is made
much easier. Furthermore the sociologist ought ideally to
be able to use his/her historical study of the consensus-
forming mechanisms to predict how and when consensus will
reappear.   As we shall see, such a methodological bonus
was obtained in the current research (this bonus was not
anticipated when the study was launched).

27. There is no necessary order in which these two tasks should
be carried out, as long as they are both carried out somewhere
in the analysis.   In the present work material relevant
to the second task is presented first in order to produce
a 'narrative' account.

28. It is always dangerous labelling anything as ethnomethodological
because of the 'more holy (i.e. ethno) than thou   syndrome'.
Thus I suspect that Garfinkel would not class Woolgar's (1976)
study as ethnomethodological, or that neither would Woolgar
today. By using the label I mean to convey the notion
that, for the ethnomethodologists, activity is constituted
in local settings and hence is an ongoing accomplishment.
Hence the empirical focus of their work is on the minutae of
the 'talk' and other activities in the local setting.

29. The focus in discourse analysis as practiced by Mulkay and
Gilbert seems to be on accounts per se.   These accounts are
not meant to tell us anything about the real processes of
consensus formation. It so happens that, as Mulkay and
Gilbert's sample frame seems to have been constructed with
the aims of carrying out the more traditional research on
a scientific controversy, their data could perhaps be re-
interpreted for the purposes of identifying social processes
of consensus.   It is puzzling why Mulkay and Gilbert have
collected data around the focus of a controversy since, for
the purposes of discourse analysis, it seems that any random
interviews with scientists would have done.

The interview data in the controversy-type study and in the
discourse-analysis-type study have very different methodological
statuses.   In the controversy-type study the sociologist
does interpretative work whereby he imputes that the piece
of interview data (interpreted in a particular way) is
ultimately a reflection of scientific action. In discourse
analysis the interview material is merely a resource from
which a whole host of possible interpretations are to be
drawn.

30. Of course, the authors of the above studies would not be interested in such social mechanisms, anyway. The account of such mechanisms is, at best, yet another 'reading' or an example of sociologists''talk'. See, for instance, Woolgar (1981).

31. To put it at its starkest: Woolgar's (1976) description of the discovery of pulsars would seem, in essence, also to apply to the discovery of N-rays. We cannot learn from such studies why pulsars are facts of the natural world and N-rays are not.

32. The intention is not to be parochial with regard to physics just for the sake of it. It so happens that most of the studies of interest have been of physics and by making comparisons between such studies it is to be hoped that we can really learn something about the development of one area of science.

33. See, for example, MacKenzie (1978), Barnes and MacKenzie (1979) and Shapin (1979). For more discussion of 'interests', see Chapter 10.

34. The present research differs from the research on specialties in that it does not make the firm distinction between the social and cognitive growth of science which seems to be implicit in such studies. See Pinch (1981b) for further details.

CHAPTER TWO

EARLY HISTORY AND BACKGROUND

Solar-neutrino astronomy provides the meeting place for two
specialist fields of science - neutrino-detection physics and
nuclear astrophysics. The first mentioned area, neutrino-detection
physics, is, as its name suggests, largely an experimental field.
Nuclear astrophysics, on the other hand, although not without its
own experimental component, is dominated by theoretical concerns.
In this chapter a brief historical outline of the two areas is
presented. This historical account will form a helpful background
in preparation for the material of later chapters. It will also
serve as a useful guide to the central technical issues in the field.

The reconstruction presented in this chapter is largely a naive
descriptive history, based mainly on scientists' own accounts.
That is to say most scientists working in the area of solar-neutrino
astronomy would probably have few quarrels with this reconstruction.
No claim is made to cover the subject matter exhaustively; neither
is it claimed that the pitfalls of internalist historiography have
been avoided.[1] The two areas are taken in turn.

NEUTRINO-DETECTION PHYSICS

The neutrino was first postulated by Pauli, in 1930, as a
hypothetical particle needed to maintain energy and momentum
conservation in nuclear beta-decay.[2] Such decays are characterised
by one nucleus spontaneously changing into another one which
differs by one unit of electric charge. Either an electron or
positive electron (positron) is emitted in the decay. Pauli
postulated that every emission of an electron or positron was
accompanied by the emission of a massless chargeless particle which

carried off the excess energy and momentum. Fermi, in 1933, named

this particle the neutrino (little neutral one), and developed a

theory of beta-decay to account for its existence.

Symbolically, nuclear beta-decay can be described by the equation

$$[A,Z] \rightarrow [A, Z \pm 1] + \beta + \nu$$

where A is the number of neutrons plus protons in the nucleus, Z is

the number of protons in the nucleus, and β and ν refer, respectively,

to the emitted electron and neutrino.   An example of such a

reaction is the decay of carbon eleven into boron eleven with the

emission of a positive electron and a neutrino:

$$C^{11} \rightarrow B^{11} + e^{+} + \nu$$
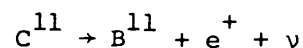
The neutrino is unique amongst nuclear particles in that, not

only is it massless (although claims have been made that it has

a small finite mass) and chargeless, but also it only interacts

via the weak interaction (one, of the four fundamental forces of

Nature). This means that it hardly interacts with matter at all and

hence is very difficult to detect.   It has been estimated that a

neutrino will on the average pass undeviated through 20 million,

million miles of lead before it takes part in a reaction.

For a long time the main evidence for the existence of the

neutrino was circumstantial. The neutrino hypothesis was consistent

with all the available experimental evidence obtained in beta-decay

measurements (such as those made on the recoil momentum of the

nucleus - see below). Its existence was also explained adequately

by Fermi theory.   However, it was still possible that some other

unknown systematic effect accounted for all the experimental

evidence. As Crane puts it in a highly influential review[3] published

in 1948:

> All of the evidence about the neutrino is, as already pointed
> out, indirect in character, since neutrinos have not yet
> been caught after leaving the nucleus (Crane, 1948: 280).

Most of the experimental evidence for the existence of the neutrino
at this time came from 'recoil' experiments. These were experiments
in which the momentum of the nucleus undergoing beta-decay was
measured. The results indicated that the excess momentum must be
taken off in discrete amounts. However, in a way such experiments
begged the question because they took the assumption of energy
conservation, and therefore momentum conservation, for granted.
If such conservation laws no longer held then there would be no
need to postulate an unobservable particle to explain the dis-
crepancy in energy and momentum. As Crane commented:

> ...of all the pieces of evidence the measurement of the recoil
> of the nucleus seems to be the most appealing, at least
> to our pictorial senses. It can, of course, be argued on
> very general grounds that, if energy is not conserved between
> the nucleus and the electron, momentum should not be expected
> to be conserved either; and in consequence of this it has
> often been remarked that the recoil experiments add nothing
> that is really new to our knowledge. (Crane, 1948: 280).

More convincing evidence for the existence of the neutrino
would be provided by its detection independent from the beta-decay
process in which it was produced. But, given that the neutrino
only interacted weakly with matter, such evidence seemed unlikely
to be forthcoming. To most scientists at the time it seemed
improbable that a particle thought to have zero mass and zero charge
would ever be detected.

Inverse Beta-Decay

There is, however, one reaction which, in principle, a neutrino
can undergo. This is inverse beta-decay - a process which is
simply the reverse of the one whereby the neutrino is produced.
According to Fermi theory there is a finite probability that a

neutrino can interact with a nucleus such as boron eleven, for example,
and produce carbon eleven. In this case an electron would also
have to be produced to maintain charge conservation:

$$\nu + B^{11} \rightarrow C^{11} + e^-$$
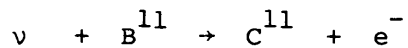
Theorists such as Hans Bethe had considered using inverse beta-
decay to detect neutrinos but their calculations indicated the
probabilities of such reactions occurring to be too low to mount
a feasible detection experiment. As Bethe wrote in 1936:[4]

> It is indeed very unfortunate that the probability of the
> disintegration of nuclei by neutrinos is so unobservably
> small, because this disintegration is the only action of
> free neutrinos which can be predicted with certainty.

Alvarez and Pontecorvo - The Chlorine-Argon Technique

Some experimentalists, however, thought it was worthwhile taking
up the challenge of trying to observe the 'unobservable'. Inde-
pendently, and at about the same time, the Italian-Soviet physicist
Bruno Pontecorvo (1946), working at the Chalk River Laboratory
in Canada, and the American physicist Luis Alvarez (1949), of the
Lawrence Berkeley Radiation Laboratory, outlined a means of detecting
neutrinos by the observation of inverse beta-decay. As this detection
process was to become the basis for Ray Davis's solar-neutrino
detector, it will be discussed here in some detail.

The reaction Pontecorvo and Alvarez proposed to use was the
interaction of neutrinos with the 24.6% abundant isotope of
chlorine, chlorine thirty-seven, to produce the radioactive
isotope of argon, argon thirty-seven:

$$\nu + Cl^{37} \rightarrow Ar^{37} + e^-$$

The advantage of using this reaction is that argon is a rare gas
and hence is chemically inert, which facilitates its separation
from the chlorine. As argon thirty-seven is radioactive, the amount

formed can be estimated by counting the number of decays. The Ar^{37} decay is of a special type in which it captures its own K-shell electron. The emitted electron, known as an Auger electron, has a very short range which enables it to be easily distinguised from background events in a Geiger counter. Calculations made by Pontecorvo and Alvarez indicated that the probability of the Cl^{37} inverse beta-decay occurring was very small. However, they thought an experiment might be feasible if a very large amount of chlorine was irradiated with a large flux of neutrinos.

Alvarez, in particular, was aware of the possibilities inherent in rare-gas chemistry. He had used similar techniques to those which he was now proposing in his war-time work on the collection and purification of a radioactive isotope of krypton and in his later work on the identification of the isotope, nitrogen seventeen.[5] The importance of his war work in leading him to develop the chlorine-argon radiochemical neutrino detection process was stressed to me by Alvarez during interview. As this information has just been declassified, it is worth quoting at some length what Alvarez had to say on the topic:

> The question was posed to me by General Groves who ran Manhattan District ....'You know we don't know whether the Germans have a reactor or not, can you think of some way to find out if they have?' So I thought about it for a few days and concluded that there was a krypton radio-isotope that is emitted from reactors. It comes out of the smokestack, would get mixed with the air and had a short enough life time. So if you flew around over Germany in an airplane that had a concentrator for krypton you could detect this radioactivity. And so the General gave me a carte blanche to get this thing built....You flew over Germany in a light bomber. It brought in the air; the air went through active charcoal that was cooled to a temperature that wouldn't soak up the oxygen and nitrogen, but would get all the krypton which has a much higher boiling point...So when you get home you...warmed that up, and then you got off the krypton, and then you could purify the krypton...We didn't find any over Germany; they didn't have a reactor.

The radiochemical method of detecting neutrinos using chlorine thirty-seven bears close parallel to this. In this case the Ar^{37} (rather than krypton) is purged from a large tank containing the chlorine target (equivalent to the air over Germany) by sweeping with helium gas. The gases are then passed over active charcoal cooled to a temperature where the argon condenses out. Upon heating, the concentrated Ar^{37} comes off and is then purified before being detected by its radioactive decay (it has a half-life of 35 days). By this means, a tiny amount of rare gas can be separated from an enormous volume of liquid. It is interesting that the roots of the neutrino-detection process which was to form the basis of Davis's solar-neutrino detector should lie in the war effort. In this respect, solar-neutrino astronomy is similar to that far better known scientific 'spin-off' from the war - radio astronomy.

The proposed detector was to be based on a large Cl^{37} target. Alvarez first considered using salt water which would provide the necessary Cl^{37} in the form of sodium chloride.[6] In order to obtain a sufficient volume the ocean itself would have to be the target and the source of neutrinos would be the nuclear processes in the Sun. Alvarez went as far as to contact the Dow Chemical Company, who were, at the time, processing considerable amounts of sea water to produce commercial supplies of bromine and magnesium. The idea was to search for Ar^{37} during this processing. $100,000 of facilities for such an experiment were even offered by Dow, but Alvarez did not take this up because calculations indicated that natural radioactivity in the sea would 'swamp' any likely signal from neutrinos.

Eventually Alvarez, like Pontecorvo before him, considered that

a large tank of carbon tetrachloride, CCl_4 - better known as cleaning

fluid - would make the most suitable detector. The cleaning fluid

had the advantage of being fairly cheap and was also rich in

chlorine, and hence Cl^{37}. The exact amount of target material

needed depended on the size of the cross-section for the interaction

of a neutrino with Cl^{37}. There seems to have been some slight

ambiguity as to what the size of this cross-section was and

Pontecorvo used one value and Alvarez another.[7] According to

Pontecorvo a cubic metre of cleaning fluid would be sufficient,

whilst Alvarez's calculations indicated that a tank-car-sized

amount (about 40 metric tons) would be needed.

Although both Pontecorvo and Alvarez considered the neutrinos

produced in the core of the Sun by hydrogen fusion as a possible

source for such an experiment, both rejected the possibility as

it was considered that the flux was likely to be too weak and the

energies of the neutrinos were in general not high enough. The

most promising source was thought to be a nuclear reactor. However,

there was one possible difficulty inherent in the use of a nuclear

reactor as a source. It produced antineutrinos rather than

neutrinos. The chlorine-argon reaction is the inverse of a

neutrino producing reaction and hence strictly needs neutrinos

rather than antineutrinos to trigger it.

The antineutrino, $\bar{\nu}$, is the anti-particle of the neutrino.

Antineutrinos are emitted in a beta-decay in which a negative

electron is also emitted. Neutrinos, on the other hand, are

produced in association with positrons. As both the neutrino

and antineutrino were thought to have the same zero mass, spin

one-half and zero charge, it was hard to see in what physical

way they differed.   Indeed, for most purposes, the formal difference

was ignored and both were frequently referred to as 'neutrinos' (as

was done earlier in this chapter when the concept of beta-decay

was introduced). The formal equivalence of neutrinos and anti-

neutrinos had been postulated by Majorana, as a modification to

Fermi theory.   There was also an important experiment by Fireman

which supported Majorana.[8]   Thus it seemed, at the time, that there

was a good possibility that the chlorine-argon process would be

triggered by the antineutrinos produced by a nuclear reactor

and, further, that such an experiment could be used to confirm

Majorana's theory. That is it could confirm that there was no

physical difference between the neutrino and antineutrino. This

aspect of the proposed experiment was stressed by Alvarez in

particular.

Pontecorvo, having first proposed such a detector during a

lecture at the Chalk River Laboratory, did not attempt a

detailed investigation of the experimental feasibility of

bringing it to fruition (although colleagues of his at Chalk River

may have tried some small-scale experiments using the Chalk River

nuclear pile).[9]   Alvarez's proposal was, on the other hand,

much more detailed and was published as a University of California

Radiation Laboratory Report (it remained classified for many years).

As can be seen from Alvarez's interaction with the Dow Chemical

Company, he was interested in following the proposal through with

a view to building a working detector.

Once Alvarez had decided that a tank of cleaning fluid provided

the most suitable target and that the best source of neutrinos

was a nuclear reactor, he contacted a colleague at the Atomic Energy

Commission with responsibility for the design of a new nuclear

reactor at Savannah River. This reactor was to produce tritium

for the hydrogen bomb. He persuaded his colleague to include in

the design an underground vault suitable for housing such an

experiment. This plan was, however, to be short-lived. Alvarez

became aware of new measurements made on the cosmic-ray background

underground.[10] These showed that the background did not fall off

underground as previously expected. It seemed that cosmic-ray

muons had much greater penetrability than had been previously

estimated. This meant that, at the depth at which Alvarez planned

to perform the experiment, there would be too many cosmic-ray

induced argon-37 transitions for neutrino events to show up (cosmic-

ray muons produce proton secondaries which trigger the detector

via the reaction $Cl^{37} + p \rightarrow Ar^{37} + n$). With this new information,

Alvarez abandoned the project and told his friend at the AEC to

forget the plans for the extra underground chamber at Savannah

River. However, it seems the new information on cosmic-ray

backgrounds came too late to be included in Alvarez's written

proposal for such an experiment. The proposal was never amended,

and, as we shall see, the result was that Ray Davis went ahead and

built such a detector unaware of why Alvarez had himself given up

any such hopes. By the time Davis became involved, Alvarez's

interest had lapsed and he was pursuing his career in particle

physics.

The Reines and Cowan Experiment

The next important step in the field of neutrino detection

was taken by Frederick Reines and Clyde Cowan of the University of

California, Los Alamos Laboratory. They too saw the need to find

evidence of the neutrino's existence at a location different from the source of the beta-decay.[11] The method they devised was simply to scale up the liquid scintillator detectors that were increasingly a part of the nuclear and particle physicist's range of detection techniques.[12] Scintillator fluid usually consisted of a combination of liquid hydrocarbons which would produce a burst of light when irradiated by gamma radiation. In combination with an array of photomultiplier tubes, which detected the light, it provided a very sensitive detector of nuclear processes.

The reaction with which Reines and Cowan planned to observed the neutrino was the inverse beta-decay process produced by the interaction of a neutrino (actually an antineutrino) with a proton:

$$\bar{\nu} + p \rightarrow n + e^+$$

The significance of this reaction for neutrino detection was that both decay products, the positron and the neutron could be observed. The positron is soon annihilated after production and produces a burst of gamma radiation which can be detected by the scintillator. The neutron slows down and then is captured by cadmium dissolved in the detector, again with a resulting burst of detectable gamma radiation. The two bursts of radiation happen one after the other at a fixed interval and the signal produced (known in the jargon as 'the signature') is referred to as a delayed-coincidence event. The occurrence of the two events one after the other with a fixed delay signifies the arrival of a neutrino. Because two events are being detected in sequence it is much less likely that the cosmic-ray background will produce a spurious signal. The proton target was provided by the hydrocarbons of the scintillator. Thus

the detector and target were combined into one and the same piece

of apparatus.

The main novelty of the Reines-Cowan plan to detect neutrinos

was the sheer size of the detector. Although modest by present-

day standards, there were worries at the time that the ninety

photomultiplier tubes needed would exhaust the available supplies.[13]

Overall it was planned to use 75 gallons of scintillator solution.

After first exploring the possibility of using an atomic bomb

as a neutrino source, Reines and Cowan eventually took their

experiment to the Hanford nuclear reactor. They made some initial

measurements in the spring of 1953 but were discouraged by an

unexpectedly large background. This was none other than the cosmic-

ray background which Alvarez had predicted would make the success

of such an experiment very unlikely. However, careful analysis of

their experimental data back at Los Alamos gave evidence of a hint

of a neutrino-induced signal, and Reines and Cowan were able to

publish a paper claiming the first detection of the free neutrino[14]

(actually the antineutrino). They reported a cross-section of

$(12 \pm 6) \times 10^{-44}$ cm^2 which agreed well with beta-decay theory.

In order to verify their observations, Reines and Cowan

improved on the sensitivity of their experimental apparatus. They

did this by the separation of the target material from the

scintillator. This meant that spatial coincidence, as well as

delayed-temporal coincidence, would be the hallmark of the neutrino's

presence. Using a detector consisting of a 'club sandwich' of target

material (water with cadmium dissolved in it) and scintillator,

Reines and Cowan were able to provide definitive evidence of the

neutrino's existence .[15] The reactor they used was at the

Savannah river nuclear pile - the same source of neutrinos which

Alvarez had earlier planned to use in his experiment.   The in-

herent difficulties of neutrino detection are illustrated by this

experiment. Despite the reactor producing a flux of 10^{13} $\bar{\nu}$ $cm^{-2}sec^{-1}$,

Reines and Cowan on average only detected 3 neutrino events/hour

and needed to run their experiment in all for a period of 2085

hours!

Although, as we have seen, Alvarez never carried out his

proposed experiment, at the same time as Reines and Cowan were

confirming their observations at the P-reactor at Savannah River,

a chlorine-argon experiment based on Alvarez's ideas was being

carried out at the R-reactor.   This experiment was operated by

Ray Davis of the Brookhaven National Laboratory.   It is he who

is the pioneer of solar-neutrino astronomy.

Davis - The Early Brookhaven Experiments

Davis's interest in neutrinos had been provoked earlier, in

1949, when he had read the above-mentioned review by Crane.   He

had commenced work on a series of the 'recoil experiments'[16].

Having confirmed the existence of the neutrino by this indirect

method he decided, as Crane, Pontecorvo, Alvarez and Reines had

before him, that the next step was to attempt to detect neutrinos

separate from their source of production. He had read the earlier

proposals of Pontecorvo and Alvarez, and, being trained as a

chemist,[17] he found the radiochemical-detection technique very

appealing. Also, as he worked at the Brookhaven National Laboratory,

with its emphasis on nuclear and particle physics (the first nuclear

reactor for research purposes and the first high-energy accelerator

built (the Cosmotron) were both coming into operation at BNL at

about this time), Davis, with the encouragement of his department

chairman, Dick Dodson, was looking for a way of making his

chemical expertise relevant to nuclear problems. The detection

of the neutrino by radiochemical methods seemed to offer an

appropriate challenge.

With this project in mind Davis and a colleague at BNL,

Seymour Katcoff, in 1954, investigated the feasibility of several

inverse beta-decay processes upon which a radiochemical neutrino

detector could be based. They found that the most favourable reaction,

both in terms of the chemistry and the cross-section, was indeed

the chlorine-argon reaction suggested earlier by Pontecorvo and

Alvarez. Thus, five years after Alvarez had completed his

proposal, Davis started planning how to construct a detector based

on this reaction (he did this without consulting Alvarez with whom

he had no formal contact until a decade later).

Davis decided that the only feasible source of neutrinos for

the experiment were those produced by a nuclear reactor. And he

did not have to go far to find such a reactor since there was one

at Brookhaven. However, such a reactor would produce antineutrinos

rather than neutrinos so unless the two particles were identical

Davis would not expect to see anything. The first working experi-

ment consisted of a fifty-five-gallon tank of carbon tetrachloride.

This small-scale experiment indicated the feasibility of the

project and was also used to set an upper limit on the neutrino

cross-section (a value of $< 2 \times 10^{-42}$ cm^2 as opposed to a theoretical

value of 10^{-45} cm^2 - calculated with the assumption of neutrino-

antineutrino equivalence). It was already clear that the large

cosmic-ray background which Alvarez had forecast would determine

the ultimate sensitivity of the experiment. Davis attempted to measure the likely effect of this background by exposing his tank at various altitudes.

Davis next built a 1,000-gallon tank, which he again tested at the Brookhaven reactor. However, he was not able to set a much better upper limit on the neutrino cross-section as he continued to encounter a large cosmic-ray background. Clearly, scaling-up the size of the experiment alone would not solve the problem, because a larger tank, although capable of detecting more neutrino events, would also detect more cosmic-ray induced events.

It did not seem that the experiment in its present form and with this source of neutrinos would be sensitive enough. However, there was another type of measurement for which the apparatus could be used. This was to set an upper limit on the flux of neutrinos coming from the Sun. The possibility of using solar neutrinos as a source, had, as we noted above, been considered by Pontecorvo and Alvarez, who rejected the possibility because it seemed the flux would be too small. Also, Crane (1948) in his well-known review paper, had considered solar neutrinos as a means of setting an upper limit on the neutrino cross-section. If a significant number of solar neutrinos were captured by the material of the Earth then the planet would heat up beyond its present temperature. By assuming the magnitude of the neutrino flux from the Sun, and knowing the present temperature of the Earth, Crane was able to set an upper limit on the neutrino cross-section. What Davis in effect did was to turn this argument on its head. By assuming a value for the cross-section (derived from theory), he used his experiment to set an upper limit on the

neutrino flux from the Sun. He did this by measuring the neutrino

flux when his tank was buried under 19 feet of earth - a sufficient

depth to shield out a large component of the cosmic-ray flux.

With the assumption that the Sun was producing neutrinos by the

CN-cycle (one of the cycles of hydrogen buring in the Sun - see

below for details), he set an upper limit of $< 1 \times 10^{14}$ neutrinos

$cm^{-2} sec^{-1}$ (40,000 SNU in today's units - 1 SNU (Solar Neutrino

Unit) is equivalent to 10^{-36} captures sec^{-1} target $atom^{-1}$). This

was not a terribly useful result as the theoretical flux of solar

neutrinos was expected to be many orders of magnitude less than

this (6×10^{10} neutrinos $cm^{-2} sec^{-1}$ or 24 SNU). But nevertheless

this measurement represented the first result of solar-neutrino

detection astronomy.

Davis published these early results in 1955, in _Physical Review._[18]

Perhaps the difficulties he faced in his attempts to detect neutrinos

can be seen in the wry comment which his paper evoked from one of

the reviewers, who wrote:[19]

> ...Any experiment such as this, which does not have the
> requisite sensitivity, really has no bearing on the question
> of the existence of neutrinos. To illustrate my point,
> one would not write a scientific paper describing an
> experiment in which an experimenter stood on a mountain
> and reached for the moon, and concluded that the moon was
> more than eight feet from the top of the mountain!

As if the difficulties Davis was encountering with the cosmic-

ray background were not enough, there was always the possibility

that, in the end, he would not see anything because his detector

might not be sensitive to the antineutrinos produced by nuclear

reactors. This possibility had become more likely in 1952, with

the collapse of the earlier experimental evidence which supported

Marjorana's theory of the physical similarity of the neutrino and

antineutrino.[20]   In addition, Reines and Cowan had by this stage

already reported their tentative results. It thus seemed unlikely

that Davis would be the first to detect free neutrinos, if he

detected them at all.

The Savannah River Experiment

In order to achieve better sensitivity Davis needed to find a

stronger source of neutrinos and a location which would provide

more cosmic-ray shielding. Both these assets were to be found at

the Savannah River nuclear pile with its powerful reactors and

massive concrete shielding. Also this was where Reines and Cowan

were in the process of setting up their detector. As there were

two suitable reactors at Savannah River, it made sense for Davis

to continue his project by joining Reines and Cowan.

There was an additional reason for Davis to move to the same

location as Reines and Cowan. If Davis's detector turned out not

to be sensitive to reactor neutrinos (antineutrinos) and Reines

continued to get a positive result then at least Davis could use

the Reines result to provide a definitive interpretation of his

own experiment. A negative result with Davis's detector could

mean one of two things: either Fermi theory was incorrect and

there were no neutrinos or antineutrinos to detect, or else the

antineutrino and the neutrino were different. To choose between

these possibilities required a detection process that was known

to be sensitive to antineutrinos - and Reines and Cowan had such

a process. If Reines and Cowan got a positive result, then Fermi

theory was correct and Davis's negative result would mean that the

neutrino and antineutrino were indeed different. Thus, even if

Davis detected nothing, his experiment would serve some purpose.

Thus it came about that, in 1955, the two main neutrino-detection experimenters(Reines and Davis)were both to be found at the Savannah River nuclear pile, both attempting to provide the first definitive evidence for the existence of the neutrino.

Davis's apparatus was essentially the same as he had used at Brookhaven - the 1,000 gallons of target material being contained in two 500-gallon tanks of carbon tetrachloride. It soon became clear that he was not detecting a significant flux of neutrinos. At the same time,Reines and Cowan obtained their confirmatory positive result. However, since Davis's new upper limit for the neutrino cross-section (0.9×10^{-45} cm^2) was lower than that reported by Reines and Cowan it did seem that Davis had provided definitive evidence that the neutrino and antineutrino were physically different.[21] Thus, Davis had a small consolation for his efforts.

Although it now seemed unlikely that the neutrino and antineutrino were identical, events in nuclear physics in 1957 placed a fresh emphasis upon finding out how exactly they differed. This new significance for Davis's experiment arose from the discovery of the non-conservation of parity. In order to explain the breakdown of parity, Lee and Yang had introduced their two-component theory of the neutrino[22]. According to this, the neutrino had an opposite helical spin from the antineutrino. Not all theorists were happy with the two-component theory and some favoured a four-component theory in which parity was still conserved. (In this theory each neutrino has a mixture of the two types of spin).[23] Indeed there was some (short-lived) experimental evidence in favour of the four-component theory.[24] One consequence of the four-component

theory was that there might be a measurable antineutrino cross-section for the inverse beta-decay Davis used. The exact value of this cross-section would depend on how the two spins were mixed. Thus Davis's detector might serve to test the four-component theory if he could search for an even smaller upper limit on the cross-section than he had set already.[25] Pauli, who had developed a four-component theory himself, wrote to Davis and suggested that this new experiment might settle the issue.

An increase in size to 3,000 gallons could conveniently be carried out at Savannah River and in the spring of 1958 Davis, with the assistance of a chemical engineer, Don Harmer, commenced new measurements. He was able to show that the antineutrino cross-section must be less than 0.25×10^{-45} cm^2, a factor of twenty below the cross-section calculated with the assumption that the neutrino and antineutrino were identical.[26] It thus seemed very unlikely that the four-component theory was correct.

This was another small success for Davis, but again he had not found a positive result. As before, he had managed to show that a theory of doubtful plausibility was incorrect. However, by this stage an event had occurred in another field, which enabled Davis to abandon his largely fruitless reactor-neutrino detection programme.

The event that was to prove so significant for Davis and heralded the dawn of solar-neutrino astonomy occurred at a meeting of the American Physical Society in early 1958. Two nuclear physicists reported new measurements of a nuclear-physics cross-section. Their measurements indicated a result that was of the order of a thousand times greater than expected.[27] In order to see the relevance of this

to Davis's experiments we will have to trace briefly developments in nuclear astrophysics - the other branch of science from which solar-neutrino astronomy emerged.

NUCLEAR ASTROPHYSICS

The interdisciplinary field of nuclear astrophysics,which combined the concerns of nuclear physics and astronomy, was born out of the attempt to find an energy source in the Sun sufficient to maintain its luminosity for a period at least equivalent to the age of the Earth. At the turn of the century it had been shown by Helmholtz and Kelvin that release of gravitational potential energy would provide some tens of millions of years of solar luminosity, but this fell well below the age of many of the Earth's rocks as determined by geologists. It was then suggested that the Sun derived its energy from the disintegration of heavy elements such as uranium and thorium, but this theory, in turn,seemed unlikely when it was found that the Sun consisted mainly of hydrogen. It was Eddington and Jeans who, in the early 1920's, were the first to suggest what is now accepted as the solution to the problem. The source of solar energy, according to them, must lie in the conversion of mass into energy, possibly through the transformation of hydrogen into heavier elements.

It was known from Aston's mass spectrographic work that the fusion of four hydrogen atoms to make one helium atom could release a considerable amount of energy. However, there was some doubt as to whether temperatures in the Sun were hot enough to produce a sufficiently fast reaction rate. The situation was clarified by Atkinson and Houterman, in 1928, when they showed that there

was a small, but finite, probability of charged particles pene-
trating Coulomb potential barriers (the force wall surrounding a
nucleus due to its charge). This meant that the nuclear reactions
could proceed at stellar temperatures. The specific nuclear
reactions were not identified until the late 1930's, by which time
nuclear physics had become an established discipline in its own
right.

The use of nuclear physics to explain astrophysical phenomena,
such as the process of energy production in the Sun, forms the
basis of nuclear astrophysics. The aim is to provide an under-
standing of astronomy, not with the use of telescopes, but by the
study of nuclear reactions. As nuclear processes are thought to
be the energy sources of stars, and the elements and their isotopes
are created in stellar-nuclear reactions, nuclear astrophysics has
a profound place in physics, chemistry and astronomy.

The Role Played by Kellogg

One of the centres for nuclear physics which has been parti-
cularly influential in the development of nuclear astrophysics
has been the Kellogg Radiation Laboratory of the California
Institute of Technology (Caltech). This laboratory was founded
by Robert Millikan with the support of W.K. Kellogg in 1931.[28]
The nearby Mount Wilson telescope, which produced many of the key
astronomical observations of the period, made this a particularly
suitable location for the study of nuclear astrophysics. Many
investigations of nuclear reactions relevant to stars and, in
particular, the determination of nuclear cross-sections for such
reactions, have been carried out at Kellogg.

The start of Kellogg's involvement in nuclear astrophysics

was the discovery of radiative capture by Lauritsen and Crane in

1934. They found that, after a carbon target had been bombarded

with protons, there was a ten-minute period of radioactivity.

This was the first time a radioactive material had been made arti-

ficially by bombarding a substance with particles. The radiative

capture reaction thought to occur was:

$$C^{12} + p \rightarrow N^{13} + \gamma$$
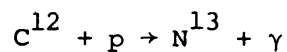
Subsequent work at Kellogg and elsewhere showed that radiative

capture of protons occurred for many other target nuclei.[29] The

significance of this discovery for nuclear astrophysics was that

it led Bethe (1939) to postulate one of the series of reactions

whereby nuclear fusion in the Sun and other stars could occur.

He suggested that hydrogen was converted into helium by a cycle

of catalytic reactions (the CN-cycle) involving isotopes of

carbon and nitrogen which underwent radiative capture. The

first reaction of the cycle was the radiative capture of a proton

by carbon twelve. This was followed by similar captures by

carbon thirteen and nitrogen fourteen. Subsequently, it has been

shown that certain oxygen isotopes can also play a (small) role

in the cycle. The complete cycle of reactions of what has become

known as the CNO-cycle is shown in Fig. 2.1.

By means of the CNO-cycle hydrogen (protons) is converted

into helium (alpha particles) with the various isotopes of

carbon, nitrogen, and oxygen acting as catalysts and not themselves

being consumed in the cycle. The net result can be represented

symbolically by the simple formula:

$$4H^1 \rightarrow He^4 + 2e^+ + 2\nu$$
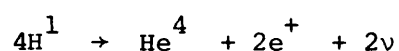
In addition to this cycle of reactions, Bethe and Critchfield (1938)

Fig. 2.1. The CNO Cycle

$$C^{12} + p \rightarrow N^{13} + \gamma$$

$$N^{13} \rightarrow C^{13} + e^+ + \nu \qquad (E_{\nu,max} = 1.20 \text{ MeV})$$

$$C^{13} + p \rightarrow N^{14} + \gamma$$

$$N^{14} + p \rightarrow O^{15} + \gamma$$

$$O^{15} \rightarrow N^{15} + e^+ + \nu \qquad (E_{\nu,max} = 1.74 \text{ MeV})$$

$$N^{15} + p \rightarrow C^{12} + He^4$$

Fig. 2.2 The Proton-Proton Chain

$$p + p \rightarrow H^2 + e^+ + \nu \qquad (E_{\nu,max} = 0.420 \text{ MeV})$$

$$p + e^- + p \rightarrow H^2 + \nu \qquad (E_{\nu,max} = 1.442 \text{ MeV})$$

$$H^2 + p \rightarrow He^3 + \gamma$$

$$He^3 + He^3 \rightarrow He^4 + 2H^1$$

$$He^3 + He^4 \rightarrow Be^7 + \gamma$$

$$Be^7 + e^- \rightarrow Li^7 + \nu \qquad (E_{\nu,max} = 0.861 \text{ MeV})$$

$$Li^7 + p \rightarrow 2He^4$$

$$Be^7 + p \rightarrow B^8 + \gamma$$

$$B^8 \rightarrow Be^{8*} + e^+ + \nu \qquad (E_{\nu,max} = 14.06 \text{ MeV})$$

$$Be^{8*} \rightarrow 2He^4$$
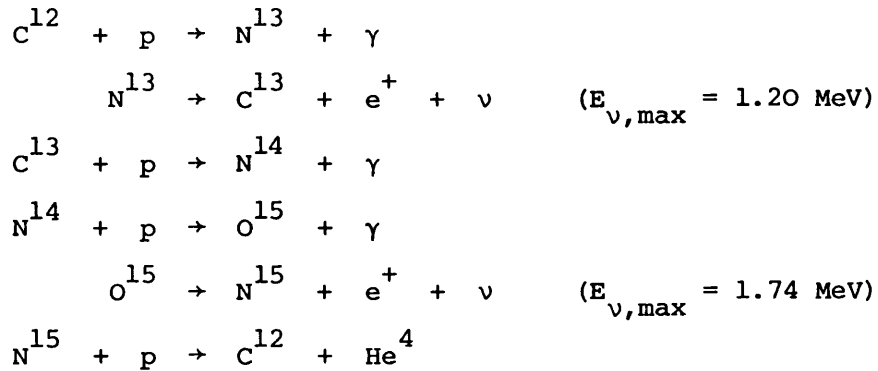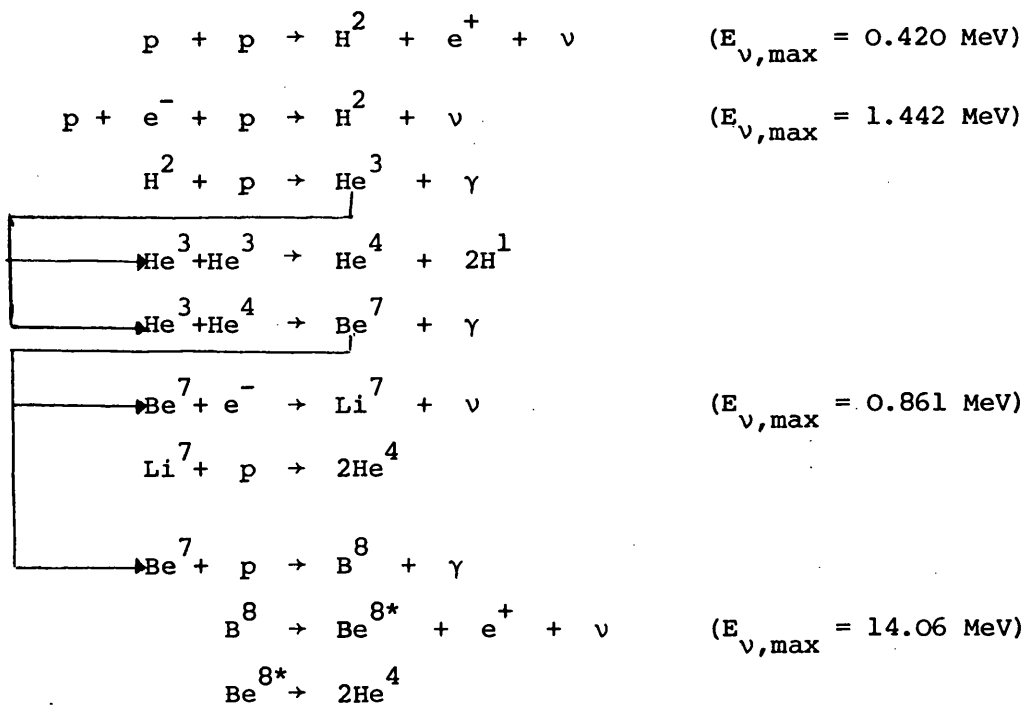
Here $E_{\nu,\,max}$ is the maximum neutrino energy.

suggested another series of reactions by which hydrogen could be converted into helium. This series was named the proton-proton (pp)-chain. The full proton-proton chain is shown in Fig. 2.2. The basic reaction is the combination of two protons to form deuterium. Deuterium,in turn,combines with hydrogen to form helium three. The helium three is consumed in one of two ways. Either two helium three atoms combine together to form helium four directly, or helium three combines with helium four to form beryllium seven. Beryllium seven can be consumed by either proton or electron capture. If beryllium seven electron-captures it forms lithium seven which, in turn, combines with a proton to form helium four. If beryllium seven proton-captures it forms boron eight. This decays into beryllium eight which is unstable and forms helium four.

As in the CNO-cycle, the net result of the proton-proton chain is the conversion of hydrogen to helium four. Which of the competing branches of the proton-proton chain predominates depends on the details of the cross-sections and reaction rates for the various reactions.

Of these two processes, it was thought at the time they were proposed, that the CNO-cycle was dominant in the Sun and that the pp-chain was more important in cooler stars. It has subsequently emerged (in the late 1950's) that it is more likely that the pp-chain is the predominant means of energy generation in the Sun.

The important thing was that,once the basic means of energy generation had been spelt out by Bethe and his associates,the details could be filled in by further calculations and by labora-tory studies of the reactions. Much of the subsequent detail has

been provided by work carried out at Kellogg.

The stimulus to the work at Kellogg was not only provided by the Sun but also by the need to develop a general theory of the evolution of stars and the origin of the elements. The difficulty here was to explain how stars ever got beyond the stage of burning helium in order that heavier elements might be produced. The breakthrough came at Kellogg with the work of Salpeter, and later Hoyle, who both provided reasons for why the reaction $3\alpha \to C^{12}$ was most likely to be the source of helium burning. Experimental work by Lauritsen and Fowler at Kellogg, amongst others, confirmed this and, after two astronomers, Geoffrey and Margaret Burbidge, joined the team at Pasadena, a comprehensive theory of nucleo-synthesis in stars and the production of all the elements and their isotopes was produced. The classic paper of the Burbidges, Fowler and Hoyle, in which this theory was presented, was published in Reviews of Modern Physics in 1957.[30] This could be said to be the high point of nuclear astrophysics at the Kellogg Radiation Laboratory.

Testing Nuclear Astrophysics

At the same time as nuclear physicists were producing more and better information concerning the basic energy-production mechanisms in stars, astrophysicists and astronomers were continually improving their theories of stellar structure and stellar evolution. The possibility of producing detailed models of stars had been shown by Russel, Jeans and Eddington.[31] This work was pursued through the 1920's and 1930's by Strömgren and Chandrasekhar. Following on from Bethe's path breaking work on the nuclear reactions in stars in the late 1930's, tremendous advances were made in the field by scientists such as Chandrasekhar, Schönberg,

Sandage, Schwarzschild and Hoyle.[32] Many of the more recent

advances in the subject were aided by the development of computers

which facilitated the building of models of stars.

The development of stellar-evolution theory was not only made

possible by the information which the nuclear physicists produced

but also by the increasingly refined astronomical observations that

were made. Developments, such as the Hertzsprung-Russel diagrams,

in which luminosity and the colour of stars' radiation could be

compared, enabled generalisations concerning stellar evolution

to be drawn. The various stages of the history of stars such as

'main sequence', 'red giant', and 'white dwarf', could be traced

on these diagrams. Stellar-evolution theory, as well as providing

an explanation of the history of stars, could also be used to

account for particular classes of anomalous stars such as the

Cephides Variables (stars which have a periodic luminosity).

The undoubted successes which stellar-evolution theory had

achieved by the late 1950's were, of course, founded on the idea

that the primary energy source of stars was nuclear reactions.

Despite the match between stellar-evolution theory and a large

body of astronomical data,it could always be maintained that there

was no direct evidence to confirm nuclear reactions as the source

of energy. For instance, for the star for which there was the most

detailed information - the Sun - most of the information was based

upon observations of photons from its surface. However, these

photons are generated one million years beforehand in the core

of the Sun.   It takes this period of time for the photons to pass

through the rest of the material by thermal diffusion. Better

evidence for nuclear fusion in the Sun would be provided by the

immediate observation of the products of the reactions in the core.

One such product is the neutrino. When Bethe outlined the nuclear

reactions he thought could occur in the Sun he mentioned in passing

that neutrinos were produced. But he did not pay much attention to

them. Neither did anyone else at the time, since, as was mentioned

earlier, it was considered that they would never be observed because

of their lack of interaction.

The disinterest in astronomical neutrinos was acutely felt

by the neutrino experimenters at the time. As Davis told me:

> It's an interesting thing, and maybe my view is distorted
> a little bit, but if I was Hans Bethe writing the CN-cycle
> paper, one of the foremost thoughts in your mind is how are
> you going to test all this. You would think that somewhere
> in his article he would say that this can be detected by the
> neutrino radiation...It shows me that those people were not
> thinking about the neutrinos at all. And I think it's
> psychology, you drop the neutrino because it's the hopeless
> one to detect and therefore it doesn't come into the
> picture at all.

An anecdote told to me by Martin Schwarzschild, one of the leading

exponents of stellar-evolution theory, supports this view of the

neutrino:

> I remember very well on one occasion where a very bright
> graduate student, after me talking in great detail about
> the structure of more evolved stars, just asked the question
> 'Why don't you take a photograph of that star in neutrinos,
> and then you can see those shells you are talking about? '
> The class broke out in laughter.

The lack of interaction of neutrinos, which encouraged most

people to neglect them, is, however, important when considering

possible tests of nuclear fusion in the Sun. Because they interact

so little, neutrinos pass straight through the outer layers of the

Sun. If neutrinos could be detected they should be observable on

Earth only eight minutes after they are produced in the centre of

the Sun. Thus, the particle which nearly everyone chose to ignore,

could, nevertheless, be used to test stellar-evolution theory -

provided it could be detected.

The nuclear astrophysicists working at Caltech, and in

particular William Fowler, were aware of the possible strategic

importance played by the neutrino in this respect. However, it did

not seem that there were neutrinos of sufficient energy to have a

realistic chance of being detected. Most of the neutrinos produced

by the pp-chain, which by the 1950's, was thought to predominate

in the Sun, were of rather low energy. It did not seem feasible

that such neutrinos could be detected in any numbers by experi-

ments such as those being performed by Davis. Also other astronomical

sources of neutrinos, such as stellar collapses, did not seem to

provide a definite enough flux of neutrinos to warrant a detection

experiment. The nuclear astrophysicists were, nevertheless,

keeping a close eye on developments in detection technique. At

conferences in the 1950's, well known nuclear astrophysicists,

such as Fowler and A.G.W. Cameron of Chalk River, used to discuss

these matters with the neutrino experimenters, Reines, Cowan and,

of course, Ray Davis.

Thus it came about that when two nuclear physicists, H.D.

Holmgren and R.L. Johnston of the Naval Research Laboratory,

reported an unexpected result at an American Physical Society

meeting in early 1958, Fowler and Cameron immediately saw the

consequences for neutrino detection. As Davis told me:

> That was the real turning point of everything...Willy 's
> eyes lit up and so did Al's. Well Good God! If that
> cross-section is what they say it is then it would play
> a role on the Sun.

The cross-section measurement presented by Holmgren and Johnston was for the

reaction: $He^3 + He^4 \rightarrow Be^7 + \gamma$
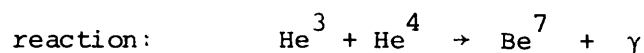
As can be seen from Fig. 2.2, this is one of the reactions of the

pp-chain.   The cross-section for this reaction had never been

measured before, but theoretical estimates had indicated it had a

rather low value.   Consequently it was believed that the pp-chain

was mainly terminated via the competing reaction:
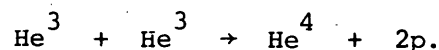
$$He^3 + He^3 \rightarrow He^4 + 2p.$$

The new measurements gave a result approximately one thousand

times greater than the old value.   If correct, this would mean that

the branch of the pp-chain through $He^3 + He^4$ would, as Fowler

and Cameron immediately saw, play a much greater part than had

previously been believed.   Furthermore, it would mean that the source

of neutrinos from the decay of boron eight (see Fig. 2.2) would

be much more important.   This is significant because the boron-

eight neutrinos are of very high energy (14 MeV maximum).   These

neutrinos have enough energy to trigger the inverse beta-decay of

Cl^{37}, and hence might be expected to produce a measurable flux

in a detection experiment such as that operated by Davis.

Although the cross-section for the reaction in the pp-chain

immediately preceding the boron-eight decay - proton capture by

beryllium seven - had never been measured, calculations indicated

that it should have a sizeable value. Ralph Kavanagh, of the Kellogg

Radiation Laboratory, immediately set about attempting to measure

this cross-section.

Fowler and Cameron both published papers[33] in which they

indicated the new possibilities for neutrino detection and they

simultaneously wrote to Davis drawing his attention to developments.[34]

If Davis could measure the flux of neutrinos expected from the

decay of boron eight then he would be able to test directly one

of the central tenets of nuclear astrophysics and stellar-evolution

theory - that the source of energy in stars was nuclear.

With this test in mind the marriage between neutrino-detection

physics and nuclear astrophysics was begun. It was this marriage

which shaped the subsequent development of solar-neutrino astronomy.

NOTES FOR CHAPTER TWO

1. Thus the impression will frequently be given in this chapter that developments proceeded according to their own immutable scientific logic and paralleled the unravelling of the secrets of the Natural World.

2. A useful survey of the literature on the history of the neutrino can be found in L.M. Lederman, 'Resource Letter Neu-1 History of the Neutrino', American Journal of Physics, 38, 1970, 129-36.

3. The influence of Crane's review can be judged by the fact that Alvarez (1949:3) and Davis (interview material) both draw attention to it.

4. This is a quote from the Bethe 'bible', (p.198). H.A. Bethe and R.F. Bacher, 'Nuclear Physics, Part 1, "Stationary States of Nuclei", Reviews of Modern Physics, 8, 1936, 82-229.

5. L. Alvarez, 'N^{17}, a Delayed Neutron Emitter', Physical Review, 75, 1949, 1127-32.

6. Interview material, and L. Alvarez, 'Post UCI Conference Reminiscences', in Reines and Trimble (1972: appendix).

7. Pontecorvo estimated a value of 10^{-42} cm^2 for the cross-section, whilst Alvarez used a value of 10^{-45} cm^2. This difference seems to have resulted from Pontecorvo using an order-of-magnitude-type calculation as opposed to Alvarez's exact numerical method. See Alvarez (1949: 5-6).

8. E.L. Fireman, 'A Measurement of the Half-Life of Double Beta-Decay from Sn^{124*}', Physical Review, 75, 1949, 323-4.

9. See Alvarez (1949: 6); also, interview material. According to Alvarez, Pontecorvo's proposal was written up by someone attending the lecture. This report was, of course, like all work associated with nuclear reactors at the time, classified. According to Ray Davis (interview material), Pontecorvo was keen to do the experiment but lacked the necessary chemical expertise.

10. These measurements were made by E.P. George and J. Evans. See for instance, J. Evans and E.P. George, 'Observation of Nuclear Disintegrations Below Ground', Nature, 164, 1949, 20-22.

11. Reines (1979); F. Reines and C.L. Cowan, Jr., 'A Proposed Experiment to Detect the Free Neutrino', Physical Review, 90, 1953, 492 ; and F. Reines, 'Neutrinos Old and New', Science, 141, 1963, 778-83.

12. C.L. Cowan, Jr., F. Reines, F.B. Harrison, E.C. Anderson and F.N. Hayes, 'Large Liquid Scintillation Detectors', Physical Review, 90, 1953, 493-4.

13. See Reines (1979).

14. F. Reines and C.L. Cowan, Jr. 'Detection of the Free Neutrino', Physical Review, 92, 1953, 830-1.

15. C.L. Cowan, Jr., F. Reines, F.B. Harrison, H.W. Kruse, A.D. McGuire, 'Detection of the Free Neutrino: a Confirmation', Science, 124, 1956, 103-4.

16. See, for instance, R. Davis, Jr., 'Nuclear Recoil Following Neutrino Emission from Be^7', Brookhaven National Laboratory, Quarterly Progress Report, April 11 - June 30, 1950, 83-7.

17. Davis's background was a PhD in electro-chemistry from Yale in 1940. From 1941-5 he was in the army. He took up a post as chemist at BNL in Spring 1948.

18. R. Davis, Jr., 'Attempt to Detect the Antineutrinos from a Nuclear Reactor by the Cl^{37} ($\bar{\nu}$, e^-) A^{37} Reaction', Physical Review, 97, 1955, 766-9.

19. Quoted in Bahcall and Davis (1980: 4).

20. M.I. Kalkstein and W.F. Libby, 'An Investigation of the Double Beta-Decay of $_{50}Sn^{124+}$', Physical Review, 86, 1952, 368-9.

21. R. Davis, Jr., 'An Attempt to Detect the Neutrinos from a Nuclear Reactor by the Cl^{37} ($\bar{\nu}$, e^-)A^{37} reaction', Bulletin of the American Physical Society, 1, 1956, 219.

22. T.D. Lee and C.N. Yang 'Parity Nonconservation and a Two-component Theory of the Neutrino', Physical Review, 105, 1957, 1671-5.

23. See, for example, M.A. Preston, 'A Proposed Beta-Decay Interaction', Canadian Journal of Physics, 35, 1957, 1017-20; and M. G. Mayer and V.L. Telegdi, '"Twin" Neutrinos: A Modified 2-Component Theory', Physical Review, 107, 1957, 1445-7.

24. E. Ambler, R.W. Hayword, D.S. Hoppes, and R.P. Hudson, 'Further Experiments on Beta-Decay of Polarized Nuclei', Physical Review, 106, 1957, 1361-3; and H. Postma, W.J. Huiskamp, A.R. Miedema, M.J. Steenland, H.A. Tolhoek, and C.J. Gorter, 'Asymmetry of the Positron Emission from Polarized ^{88}Co and ^{52}Mn Nuclei', Physica, 24, 1958, 157-68.

25. See R. Davis, Jr., 'An Attempt to Observe the Capture of Reactor Neutrinos in Chlorine-37', in R.C. Extermann, (ed.), Radioisotopes in Scientific Research, Vol. 1, New York: Pergamon, 1958.

26. R. Davis, Jr., and D.S. Harmer, 'Attempt to Observe the Cl^{37} ($\bar{\nu}$, e^-) Ar^{37} Reaction Induced by Reactor Antineutrinos', Bulletin of the American Physical Society, 3, 1958, 26.

27. Letter, J. Bahcall to R. Davis, January 3, 1963.

28. A brief history of the Kellogg Radiation Laboratory can be found in Engineering and Science, 'Special Issue in Memory of Charles Lauritsen', 32, June 1969.

29. For details see, W.A. Fowler, 'Nuclear Astrophysics - Today and Yesterday', Engineering and Science, 32, 1969, 8-13.

30. E.M. Burbidge, G.R. Burbidge, W.A. Fowler and F. Hoyle, 'Synthesis of the Elements in Stars', Reviews of Modern Physics, 29, 1957, 547-650.

31. For a much more informative historical account of developments in this period see Karl Hufbauer, 'The Stellar-Energy Problem, 1919-1939', Paper presented to the H.S.S. Meeting, Madison, Wisconsin, October 1978.

32. The standard modern accounts of stellar-evolution theory are to be found in M. Schwarzschild, Structure and Evolution of the Stars, Princeton: Princeton University Press, 1958; and D.D. Clayton, Principles of Stellar Evolution and Nucleo-synthesis, New York: McGraw Hill, 1968.

33. See Fowler (1958); and Cameron (1958).

34. Letter, W. Fowler to R. Davis, January 7, 1958; and letter A. Cameron to R. Davis, January 22, 1958.

CHAPTER THREE

EXPERIMENTAL DEVELOPMENTS IN SOLAR-NEUTRINO ASTRONOMY   1958-1964

In this chapter, experimental developments are described from the time in early 1958 when solar-neutrino detection became a feasible proposition until the point in November 1964 when the first experiment was funded.   As we shall see below, the successful funding of such an experiment was the high point of much of the scientific activity over the period.

The account presented (as throughout the thesis) is based upon correspondence files;   interviews with the main participants; and upon the scientific literature. Apart from the task of accurate historical description, this chapter is important for establishing the basis for certain sociological themes which are discussed in detail in Chapter 10. Rather than place these sociological concepts to the fore and order the account around them explicitly, I have related events chronologically, and drawn attention to the developments of sociological interest as and when they arise. This is because, as emphasised in Chapter 1, the social processes of interest - those which shape the social construction of scientific knowledge - are essentially temporal processes and their unfolding can only be noticed by following through an extended period of scientific development.

One of the central themes of this thesis is the exploration of the interaction of theory and experiment in the production of scientific knowledge. As will become apparent, once the prospect of measuring the neutrino flux from the Sun became more favourable in 1958, subsequent experimental and theoretical developments were closely linked. For reasons of presentation it is more convenient

to deal with experimental and theoretical developments separately.

Thus a chapter on experimental developments will be followed by

a chapter on the parallel theoretical developments which occurred

over the same period. However, given the close ties between theory

and experiment, some overlap between chapters is inevitable.

For example, theoretical events will be mentioned in this chapter

which will get more exhaustive discussion in Chapter 4.

Similarly experimental events treated in great detail here will

receive brief mention again in Chapter 4.

1958-60, Davis Starts Plans for Solar-Neutrino Detection

It will be recalled from the preceding chapter that in early

1958, when Davis received letters from Fowler and Cameron informing

him that he might be able to detect solar neutrinos, he was still

making reactor-neutrino measurements at Savannah River. Before

hearing from Fowler and Cameron, Davis had been unaware of the new

developments in nuclear astrophysics. As he told me:

> It first really got to me. You see I didn't know Holmgren
> and Johnston. I didn't follow nuclear physics that well.
> I didn't know that much about how that fitted into the
> scheme of things.

Davis was excited about the developments in nuclear astrophysics

because they meant he could extend his research programme in a

new direction - furthermore a direction in which he was likely to

meet with more success since the Sun produced neutrinos rather

than antineutrinos. As we saw in Chapter 2, his reactor-neutrino

experiments had met with only limited success, largely because

his technique was not suitable for the detection of antineutrinos.

Although he had not yet detected anything (apart from background

counts), he was nevertheless by now an acknowledged expert in this

particular technique. As Fowler commented in his letter to Davis:[1]

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

Davis, of course, had earlier been interested in the Sun as a neutrino source. As we saw, in 1955 he had used his 1,000-gallon apparatus to set an upper limit on the solar-neutrino flux. However, because theory at the time indicated that he would not detect anything, such an experiment had not been expected to produce any useful information.[2] But, from Davis's point of view it was an interesting experimental project in its own right to try and make such a measurement, even though theoretically he might not expect to see anything. As John Bahcall, a close collaborator of Davis's, told me:

> Now Davis very much wanted to do this [a solar-neutrino experiment]. He just needed a demonstration that it was feasible, a good theoretical underpinning. He had wanted for some time just to put a detector out there and look, but he couldn't get anyone to support that because he couldn't make a case that he would see anything that was interesting.

With the events of 1958 it now looked possible that nuclear astro-physics would provide Davis with the theoretical rationale for which he had been looking.

It is important to note that Davis has always had an ambivalent attitude towards nuclear-astrophysical theory. Although it was obviously important to him because it gave him a justification for proceeding with a solar-neutrino experiment (and, as we shall see, enabled him to get funding for such an experiment), at the same time he was rather sceptical about what the theory had to say. As he told me:

> I guess this is an experimentalist's attitude. What do you use the theory for? You use the theory to tell you that it makes sense to do the experiment, whether you take the theory seriously or not...For example, earlier, before Holmgren and Johnston...and all of that, you'd say if I do the experiment

> I would get nothing...There is no sense in doing the experi-
> ment, so you don't do it. And when the theory tells you the
> Sun is different to what we thought, there are some neutrinos
> coming out, the boron-eight neutrinos; they've got a high
> cross-section and calculating these things you get kinda
> numbers that go all in the loft [i.e., a big signal predicted].
> 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, of course, had, by this stage, good reason to be somewhat

sceptical towards theory. In his work at nuclear reactors he had

seen different theories come and go.

There is it seems an element of what might be described as

'instrumentalism' in the attitude of Davis towards the theory. For

him the theory was a means of arguing for his own experimental

programme. This theme will be taken up again in Chapter 10 but

instrumentalist attitudes on the parts of both experimenter and theor-

eticians will be a recurring theme throughout the developments

described.

Davis, upon receiving Fowler's and Cameron's letters, immediately

started to explore the new prospects for solar-neutrino detection.

Calculations indicated that he could expect a signal in his 1000-

gallon tank. He soon wrote back to Fowler and Cameron[3] informing

them of his calculations. He felt that, although it was possible

that part of the background in his experiment at Savannah River

was produced by solar neutrinos, in order to make a more definite

measurement he would have to move his apparatus to a mine, where

the extra cosmic-ray shielding provided by the roof would sub-

stantially increase the sensitivity of the detector. He estimated

that for a 1,000-gallon experiment approximately 500 feet of rock

would be needed. As he told Fowler:[4]

> If these calculations look reasonable to you we will
> seriously consider moving the 1000 gallon experiment to a
> mine after the Savannah River background experiments are completed.

Given the uncertainties in the, as yet, unmeasured beryllium

seven-proton capture cross-section (known as S_{17}), upon which the

flux of boron-eight neutrinos was crucially dependent, Davis felt

that a much more definitive test could be made using a 10,000-

gallon tank detector. Experimentally, he felt such an increase

in size would be perfectly feasible.

Fowler's reaction to Davis's suggestion of an experiment in

a mine was very positive. He wrote back:[5]

> The calculations given in your letter look very reasonable
> indeed and also very exciting. I do hope you will proceed
> to measure the solar neutrino flux by repeating your
> experiments in a mine.

Fowler went on to emphasise the crucial importance of the unknown

beryllium seven-proton capture cross-section, S_{17}. As mentioned

in Chapter 2, Ralph Kavanagh, of the Kellogg Radiation Laboratory,

was already attempting to make this vital measurement. If part of

the background in Davis's detector at Savannah River was indeed

from solar neutrinos then he could be detecting evidence for the

Be^7-p reaction in the Sun before anyone else; an achievement which,

as Fowler put it, 'would be a great feather in your cap'.[6]

Cameron's reaction was equally enthusiastic. New calculations

of S_{17} indicated that solar neutrinos from B^8 decay should be

detectable. His advice to Davis was:[7]

> ...you should put a 10,000 gallon tank of CCl_4 in a mine to
> detect these...

Clearly, any such larger experiment would require considerably

more resources. Davis's funding came from the chemistry-department

budget of the Brookhaven National Laboratory. Each year the

departmental chairman had responsibility for assessing the forth-

coming budget of his department and he would then apply to the Atomic

Energy Commission (AEC), who sponsored Brookhaven, for the requisite

money. Davis's work at this stage required funds in the order of

tens of thousands of dollars and could fairly easily be accommodated

within the overall chemistry department budget. However, any

effort to increase the size of the experiment substantially, would

require funding more in the magnitude of hundreds of thousands of

dollars - an amount which would require a special application to

the AEC to be made.

The impact of funding considerations on the development of

solar-neutrino astronomy will be one of the main themes stressed

in this chapter and the next. Even at this early stage the need to

find finance for a large-scale experiment was emerging. For

instance, Davis, in his reply to Fowler, wrote:[8]

> I have talked to Dick Dodson about the possibilities that
> you propose and he is very much interested in extending the
> experiment to look for solar neutrinos.

Dodson was, at the time, chairman of the Brookhaven chemistry

department, and it was he whom Davis would have to approach initially

in order to obtain funding. Davis concluded his letter to Fowler

as follows:[9]

Dick would like for me to send on his best regards.

Dodson and Fowler were friends and former colleagues at Caltech.[10]

The personal connection between them, as emphasised in Davis's

greeting to Fowler, was acknowledged in Fowler's subsequent reply:[11]

...please give my very best regards to Dick Dodson.

The close personal ties between Fowler and Dodson were, as we shall

see below, to prove vital for the eventual funding of the solar-

neutrino project.

In April 1958, Davis contacted the New Jersey Zinc Company with

a view to moving his 1,000-gallon experiment into their Franklin,

N.J. mine.   The prospects for solar-neutrino detection at this

time were set out by Davis in a letter to Willard F. Libby, the

Nobel laureate who developed radio-carbon dating, and who was a

high-ranking official in the AEC.   Libby's support would be needed

before a larger experiment could be undertaken.[12]

> Our present plans are to move the 1,000 gallon experiment
> to a mine deep enough to remove all μ-meson background
> effects.   Actually a 1,000 gallon experiment is only
> marginal, however, we can do this inexpensively. It would
> be far better to perform an experiment with 10,000 or more
> gallons which would be a crucial test of Fowler and
> Cameron's calculations.   A measurement of the neutrinos from
> the Sun is the only direct experimental way of testing the
> overall conclusions reached by astrophysicists on nuclear
> reactions in stars.

The importance of nuclear astrophysics to Davis's plans

can be seen from this letter.   He was able to argue that his

experiment would be a 'crucial test' of nuclear-astrophysical

theory.

1958-1960, Davis Moves to a Mine But the Prospects Do Not Look Good

Davis got permission from Dodson to move his thousand-gallon

detector into a mine and, by July 1960, he had installed his

apparatus in a 2,300-feet-deep limestone mine in Barberton, Ohio

(operated by the Columbia Southern Chemical Company). The basic

experimental apparatus and procedure for this solar-neutrino

detector were the same as had been used in the earlier reactor

experiments.   The only difference was that the less volatile

material, perchloroethylene, replaced carbon tetrachloride as

the target material.   The apparatus consisted of two five-hundred-

gallon tanks of perchloroethylene (see photograph in Fi g. 3.1).

The tanks were equipped with agitators and a helium-purging

system. After the exposure of the tanks for several months, by

which time Ar^{37} activity should have built up to a measurable

Fig. 3.1. The Barberton Experiment
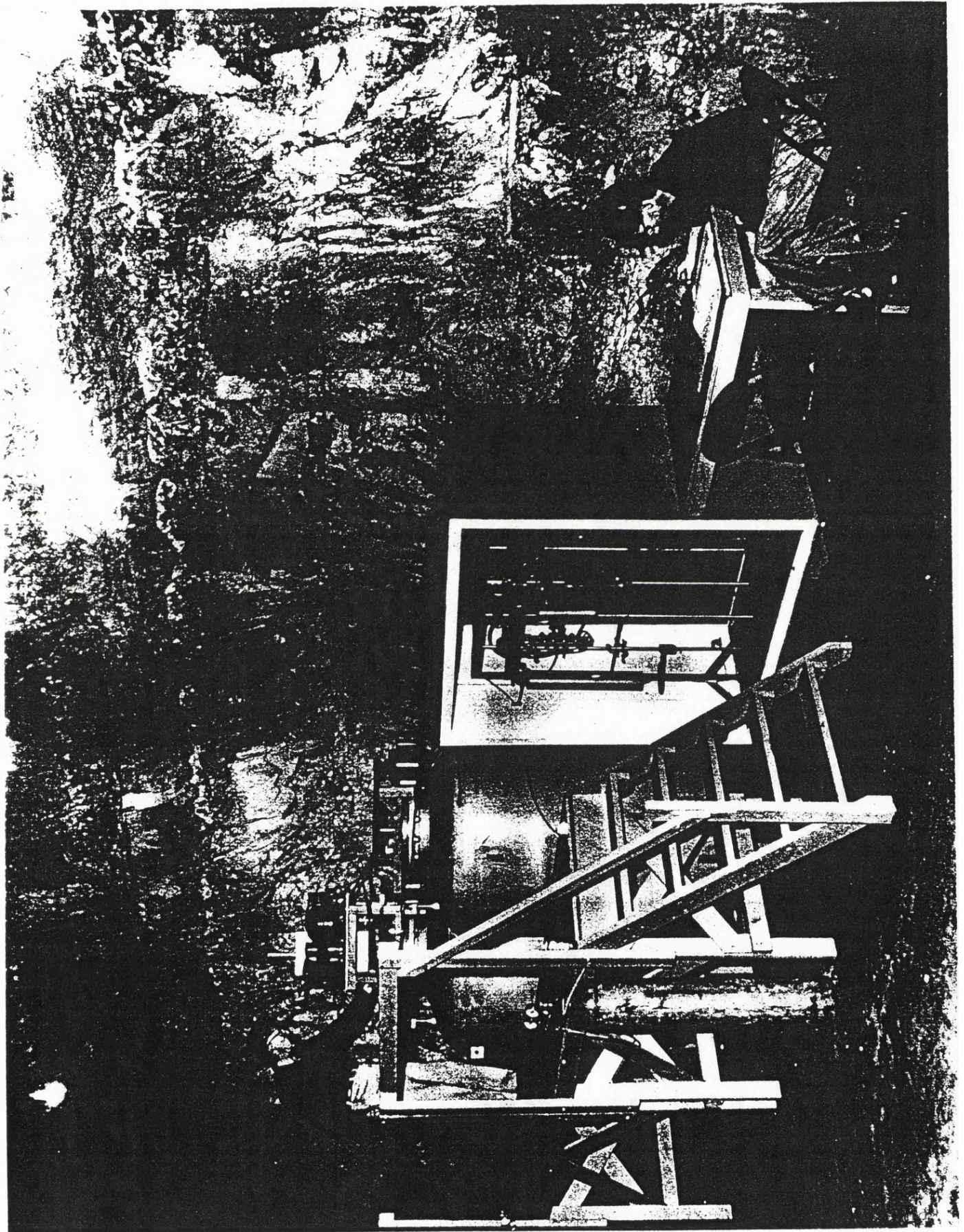
amount, any Ar^{37} formed was removed by passing a stream of helium gas through the tanks. The helium gas stream, after leaving the liquid, was passed through condensation traps to remove perchloroethylene vapours and then through charcoal cooled to liquid nitrogen temperatures. At this low temperature, argon is adsorbed on the charcoal and the helium passes through. The sample of argon was removed by heating the charcoal and purified. The sample was then placed in a small low-level counter surrounded by heavy shielding and anticoincidence detectors, and the characteristic radiation from the Ar^{37} decay observed.

In addition to this basic experimental procedure, Davis ran a check on the argon-recovery efficiency. This was carried out by the introduction of a small amount of Ar^{36} carrier gas into the tanks before each exposure. By performing a mass analysis on the recovered argon, he was able to show the recovery efficiency was of the order of 90 - 95%. Thus, any Ar^{37} formed in the tank should be extracted along with the carrier argon. Although Davis was looking for only a few Ar^{37} atoms amongst the 1,000 gallons of liquid, in principle he should have been able to extract and identify such small numbers of atoms.

The first experimental run indicated a result which was consistent with 10 neutrino captures a day. In subsequent runs, the counting efficiency was improved (that is the counter's background was reduced) and Davis was eventually able to set an upper limit of ∿0.5 neutrino captures per day. If this signal came from solar neutrinos, it would correspond to approximately 300 SNU. However, the signal was estimated by Davis to be produced almost entirely by the cosmic-ray background in the tank. Thus the most he could say was that the solar-neutrino flux was no larger

than 300 SNU.[13]

As was mentioned above, even with the signal estimated in early 1958, Davis had felt a one-thousand-gallon experiment was only marginal and that a ten-thousand-gallon experiment would be needed to stand any realistic chance of detecting solar neutrinos. His estimates then had been based on Fowler's and Cameron' calculations of the still unmeasured value of S_{17}. However, by the end of 1958, Kavanagh had reported his provisional results of the measurement of S_{17} and he had found the cross-section to be much lower than expected.[14] This meant that the boron-eight flux would not be as large as it had seemed in early 1958. Indeed it looked as if the branch of the pp-chain through B^8 was not, after all, very significant. Fowler wrote in an important review, published in 1960:

> ...this mode of completion [via B^8] of the pp-chain can be neglected in the Sun...(Fowler, 1960: 211)

There was a flurry of theoretical interest at this time in a way of completing the pp-chain which Bethe had earlier considered and rejected. This was a series of reactions involving the formation of lithium four which itself decayed by producing a very high energy neutrino (for details see next chapter). Most theorists thought such a possibility was unlikely. Davis was, however, more interested in this possibility: as he told me:

> [A Soviet physicist] pointed out it may be possible, you don't know - that kind of argument. And Willy [Fowler] said 'Oh Hell, lithium four can't be stable'. He talked about the nuclear structure...So you had these two viewpoints. For me it was interesting because at least someone posed something you could test. So if lithium four formed I would see something.

Davis's instrumental attitude towards theory of using it to justify

his experiments, even if the theory itself seemed improbable, is

again reflected in his attitude to the lithium-four possibility.

If lithium four existed, Davis would expect to see a sizeable

signal (approximately 20 neutrino captures a day). As Davis's results

were lower than this limit, it seemed that he had indeed ruled out

the lithium-four possibility. Again, however, as in his reactor

experiments, he had merely used a negative result to rule out a

possibility that was not very likely in the first place. He still

had not succeeded in detecting any neutrinos. The future for

solar-neutrino detection did not look very promising at this stage.

Reines, in concluding a review article published in 1960, summed

up the prospects as he saw them:

> ...the probability of a negative result even with detectors
> of thousands or possibly hundreds of thousands of gallons
> of CCl_4 tends to dissuade experimentalists from making
> the attempt. (Reines, 196C: 25).

1960-1963 Davis Continues his Programme and Bahcall Commences his Involvement

Davis, however, having persevered for a decade with his technique,

was not easily dissuaded. Indeed, in terms of experimental goals,

his move to the Barberton mine had been successful. It had enabled

him to refine his technique and show that it worked in a mine.

And he had never really expected to detect solar neutrinos with

this size of detector, anyway. As he wrote at the time:[15]

> ...the experiment does not represent a serious attempt to
> detect solar neutrinos. To observe the flux of solar
> neutrinos now calculated by the astrophysicists would require
> an experiment using 50 to 100 times the volume of perchloro-
> ethylene we are now using, and this would be a very large
> undertaking.

If Davis wanted to continue with his programme he would just

have to build an even larger detector. In 1961 he was starting to

explore this possibility. Again an excerpt from his correspondence reveals his intentions:[16]

> It is possible to scale up the detector by a factor of 100 and I am now considering whether the large investment in effort and expense would be worthwhile.

Whether it would be worthwhile proceeding with such a 100,000-gallon experiment would depend crucially on the prediction of the expected solar-neutrino flux. Such information could only come from the nuclear astrophysicists.

Thus far, the two nuclear astrophysicists to show interest in Davis's experiment had been Fowler and Cameron. Although Cameron continued to follow Davis's work and calculated some neutrino fluxes, he did not play the direct active role which Fowler came to undertake. Cameron's interest has been very much a by-product of his more general astrophysical interest in stellar-evolution theory. Fowler's interest on the other hand, as has been emphasised, emerged from his background in nuclear physics. Through his work with the Burbidges and Hoyle on stellar-nuclear synthesis, Fowler had already made his reputation by the early '60's. The direct test of nuclear synthesis made possible by the detection of solar neutrinos would be, as one solar-neutrino scientist told me, 'the icing on the cake'.

Fowler did make some calculations of the expected neutrino fluxes and got a stellar-model specialist at Caltech, R. Sears, to make some computations (see Chapter 4 for details) but it was soon clear that whether or not neutrinos were detectable on Earth depended on exactly what the solar model said. And working out the problem in the sort of detail that was needed was going to be a major theoretical undertaking. It was this problem that a

young nuclear theorist, John Bahcall, tackled.   If solar neutrinos
were to be Fowler's 'icing on the cake', they proved to be
Bahcall's 'bread and butter'.

Bahcall was working at the Indiana University , Bloomington,
on nuclear-decay rates in stellar interiors. Fowler drew Davis's
attention to Bahcall's work because he felt he might be able to
make accurate calculations of reactions in the pp-chain (and, in
particular, the rate of electron capture by beryllium seven) relevant
to solar-neutrino detection. Davis read one of Bahcall's articles.
However, he was not immediately enthused;   as he told me:

> I read this article [by Bahcall] but it was a typical
> theorist's article, all the formulation was there but
> he didn't do anything of interest, he didn't calculate
> anything. So I coudn't do much with his article. So I
> wrote to John...about his article and I described this
> business about Be^7, was it formed in the Sun? So you would
> like to know accurately what the Be^7-capture was in the Sun
> and how it related to solar neutrinos.

From Davis's point of view, theoretical articles were mainly of
interest if they contained calculations of what he, as an experi-
menter, might expect to detect.

Bahcall, upon receiving Davis's letter (in February 1962), agreed[17]
to make the necessary calculations. He, also, considered the
proposed experiment to be a crucial test of nuclear astrophysical
theory and he wrote back to Davis describing the proposed experiment
as 'very important'.[18] He wrote several further letters to Davis in
the Spring of 1962 reporting on his progress, and, by late May,
he had the result.[19]   Although this particular calculation did
not show any great promise for the experiment (see Chapter 4 for
details), Bahcall was now enthusiastic about the project and was
willing to make more calculations. He was able to do this by
virtue of a post-doc. position at Caltech, working with no less

a figure than Fowler.

In the late summer of 1962 intensive theoretical work on the prediction of the solar-neutrino flux was commenced at Kellogg. By October 1962, a detailed calculation had been carried out by Bahcall with the assistance of Fowler and two Kellogg solar-model specialists, R.Sears and Icko Iben. The results did not look very hopeful for Davis's experiment. Using the latest values for the He^3-He^4 cross-section, which had just been remeasured at Kellogg by Parker and Kavanagh[20] (a value about half that found by Holmgren and Johnston), the Kellogg group predicted a boron-eight flux of only 3 SNU (Bahcall, Fowler, Iben and Sears, 1963). As Fowler informed Davis:[21]

> This is $\sim$1/1000th of the value you mentioned... as the detection
> limit for the 1,000 gallon experiment. On the face of it,
> this value looks very difficult to reach.

A copy of Fowler's letter to Davis was also sent to Fred Reines, who continued to have an interest in all types of neutrino detection, including solar neutrinos. His reaction was still the pessimistic one which he had expressed in his 1960 review. He wrote back to Fowler:[22]

> The solar-neutrino problem stops me cold...

Davis's reaction was more positive. He thought that, although solar-neutrino detection would be even more difficult, a larger experiment was not impossible. He wrote:[23]

> This looks grim, detecting a few a day in a 100,000 gallons,
> but not impossible. I would propose the following approach:
> (1) Use a 100,000 gallon tank at a cost of around $250,000...
> (2) Build a low level counter with a zero background, so
>   that all pulses are the event sought for. This is
>   possible in principle by purifying all components (chemist's
>   work), and carrying out the counting underground to
>   eliminate γ's produced in the shield by cosmic rays.

> These improvements should give an improvement in detection
> sensitivity by a factor of 1000 that is required. It still
> appears that the inverse beta process in chlorine (cleaning
> fluid method) offers the best ultimate sensitivity for low
> energy neutrinos... This letter is just to let you know I
> haven't lost interest in the experiment and to say it still
> looks feasible.

The difficulty of any such experiment was reiterated by Bahcall

in a letter sent to Davis just four days after Davis's letter to

Fowler:[24]

> These results suggest the experiment is extremely difficult.
> Do you think it is possible? We are all very much interested
> in your comments.

After a further calculation by Bahcall[25] in which he corrected

some errors made in the previous computations (see Chapter 4 for

details) things looked slightly more optimistic. Davis wrote at

the time:[26]

> It would be possible to observe a rate as low as this [1.6
> captures/day] with 10^5 gallons of perchloroethylene, and a
> counter with an essentially zero background. 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 attitude of the Kellogg group to Davis's continued optimism

was expressed by Bahcall in an important letter, dated January

3, 1963:[27]

> We were all pleased to learn...that you have started
> exploratory discussions concerning the possibility of
> performing the solar neutrino experiment. It seems to us to
> be a fundamental experiment since it would afford the only
> direct evidence of specific nuclear reactions occurring in
> the interior of a star, namely the Sun. Please keep us
> informed of your progress and let us know if we can be of any
> help.

It can be seen that the Kellogg group, too, were hopeful that

such an experiment could be performed.

1963, The Kellogg Group and Davis Co-operate to Try and Get Funding for a Large Experiment

Thus, at the start of 1963, we find that Davis and the Kellogg Radiation group (particularly Bahcall and Fowler) were moving towards active cooperation in getting a large solar-neutrino experiment under way. Davis had outlined the feasibility of the project based on the Caltech calculations and had initiated exploratory discussions to obtain funding. His enthusiasm for the project, in view of his efforts over the years, was clear. Similarly the Kellogg group were enthused by the project in view of the test that it provided of nuclear synthesis in stars. Their request to Davis for him to keep them informed of his progress and their offer of assistance shows the degree of their commitment to the project.

Of course, any progress to be made would depend to a large part on whether or not Davis could get funding for a new experiment. Davis had already made it abundantly clear that the experiment would be expensive - of the order of $200,000. Although the general funding climate for physics experiments was much more favourable than today, the post-war boom of Brookhaven's sponsors, the AEC (based on the success of nuclear weapons), was coming to an end. The space effort of the 1960's meant that NASA was becoming the prestige agency.[28] Also it must be borne in mind that a solar-neutrino detection experiment was an unattractive venture to funding agencies, as it would produce only one type of measurement. It would not provide the facilities for large groups of experimenters and many different types of experiment such as could be produced by investment in an accelerator.

The procedure for getting support for an experiment of this size

was first for the experimenter, Davis, to approach his departmental

chairman in the normal way and convince him of the worth of the

experiment.[29]   As we saw above, Davis had already made such an

approach as far back as 1958.   If the chairman gave his support

it was then up to him (and the experimenter) to convince the

AEC that they should put the extra funding in the departmental

budget.   As part of the process of convincing the AEC it would be

necessary also to have the support of the director of BNL. If the

AEC refused to fund the experiment then it might be possible for

an application to be made to some other agency, such as the NSF

or NASA.

As the experiment Davis was proposing to carry out was in the

area of nuclear astrophysics and Davis was himself a chemist, out-

side assistance would be particularly useful in the bid to obtain

funding.   Bahcall's letter to Davis, offering the help of perhaps

the most prestigious group of nuclear astrophysicists in the

country was thus an offer of some substance. Of course, Bahcall

as a young research fellow would have a limited influence, but

Fowler, as an eminent Professor, was in a position to be of much

more help.   Bahcall's relationship with Fowler at the time was

described to me by another scientist who had spent time at Kellogg:

> I was certainly around in the early days when John Bahcall
> was pushing, you know, making propaganda for the solar-
> neutrino experiment. And Willy Fowler was then the sorta
> big white father of, you know the sorta father confessor,
> so to speak, of both John Bahcall and me. And he was good
> at pushing what his young men were interested in and so on.

Bahcall, as Fowler's prodigy, naturally showed Fowler copies

of his correspondence with Davis. Indeed most of the correspondence

between Davis and Bahcall, and Fowler was circulated amongst other

members of the Kellogg group such as the stellar-model specialists,

Sears and Iben, and the nuclear experimentalists Parker and

Kavanagh. I was given access to the correspondence files of

Davis, Bahcall and Fowler. At the bottom of the copy of Bahcall's

(January 3) letter to Davis (mentioned above) filed by Fowler,

there is the following handwritten note:

>   Write to Dick Dodson
>   Glenn Seaborg
>   Lee Haworth.

This note, written by Fowler, indicates how he planned to help

Bahcall, and Davis, with getting funding for the solar-neutrino

experiment. The persons referred to in the note are all of crucial

importance in winning support for the experiment.

Dick Dodson has already been mentioned. He was Fowler's former

colleague and friend who was now chairman of Davis's department at

Brookhaven. Glenn T. Seaborg was a member and Chairman of the AEC

from 1961 to 1971, and Leland J. Haworth was also a member of the

AEC from 1961 to 1963. We can surmise that Fowler was thinking

of writing to these scientists at this moment because of their

probable influence in the battle to get funding.   Whether Fowler

did actually write to Seaborg and Haworth is not known. What is

known is that one day later he wrote to Dodson. As this letter

is of some interest, I will quote extensively from it. It begins:[30]

>   Dear Dick,
>   Greetings after a long time abroad. [Fowler had spent some
>   time in Cambridge, England .] How time flies! I recall my visit
>   to Brookhaven several years ago and your splendid hospitality
>   with much nostalgia.

This opening passage re-establishes the personal links between

Fowler and Dodson. This is no cold polite letter from an eminent

Professor in one field to an unknown departmental chairman in another

field at another institution. It is a letter between two friends

and former colleagues.   The letter continues:

> As you may have heard we have been in correspondence once
> again with Ray Davis about the possibility of solar-neutrino
> detection. The present situation is that we have completed
> our measurements of the cross sections for He^3 (α,γ) Be^7
> and Be^7 $(p,\gamma)B^8$, have carried out rate integrations over
> realistic solar models and come up with rates $\sim$1 capture/
> 10^5 gallons-day...This is smaller than we had hoped for
> some years ago but knowing Ray and his ability, we feel it
> might be detectable. Moreover, a <u>larger</u> value would tell us
> that the <u>central</u> solar temperature is higher than we think.
> In nuclear astrophysics, we really need a check on nuclear
> reaction rates and solar models so all of us here are hoping
> the experiment will be done.   I hope you will agree and, if
> so, Charlie Lauritsen and I will be willing to help you with
> Seaborg, Haworth, Goldhaber <u>et</u> <u>al</u>. if it is necessary.
>
> I would welcome your reaction to this (especially a favourable
> one!)

Charles Lauritsen, as mentioned in the previous chapter, was a founder

member of the Kellogg Laboratory and a very distinguished and

influential Caltech scientist in his own right.   Although, as far

as can be established, he played no direct part in funding the solar-

neutrino experiment, the backing of such a powerful ally no doubt

helped Fowler in his efforts.[31]   Of the other people mentioned in

the letter, the only one whom we have not yet encountered is

Maurice Goldhaber.   As well as being a very eminent nuclear and

particle physicist, he was the Director of the Brookhaven National

Laboratory.   It can be seen that in his letter, Fowler not only

endorsed Davis's experiment for Dodson's benefit but also offered

to help with influencing these other key individuals.

The importance of this letter to Dodson can be seen from his

reply. Again the introduction reciprocates the friendly style of

Fowler's letter:[32]

> Dear Willy,
>   It was delightful to hear from you again.   Best greetings.
> to you and our mutual friends in Pasadena (especially including
> Charlie).

He went on

> I was very pleased to hear your considerations on the solar
> neutrino flux and your remarks about a detection-measurement
> experiment by Ray Davis. I'm in favour of doing it - on
> the assumption, of course, that it looks like a reasonable
> scientific gamble. Your encouragement is a very strong
> consideration.

It can be seen that Dodson, anyway, by this stage seems to have

been convinced. In the final part of his letter Fowler's and

Lauritsen's offer of help in persuading others was acknowledged

and Dodson wrote that it was 'highly probable' that this would

be taken up at a later stage. Indeed he had already set things

in motion by holding informal discussions with BNL-Director,

Goldhaber, and AEC-official, Seaborg.[33]

By July 1963 the attempt to get funding seemed to be moving

smoothly. Davis, in a letter to Bahcall, was able to write:[34]

> We are planning a 100,000 gallon experiment, and I am
> reasonably certain funds will be available to set it up
> next year...

1963, Davis Searches for a Suitable Mine

Whilst Fowler and Dodson were attempting to ensure the funding

for the project Davis continued with his technical investigations

of the feasibility of a large experiment. One of the major difficulties to be overcome was finding a suitable location for the

experiment. As Davis mentioned to Bahcall in his letter of July

1963:[35]

> We are now looking into deep mines in the U.S. and Canada
> but the list of possibilities is rather small, and the space
> is usually quite limited in deep mines.

The reason Davis needed a deeper mine than for the thousand-gallon

experiment was because of the greater cosmic-ray background which

he could expect to detect with a larger experiment. His calculations

indicated that he would need a mine 5,000 feet deep.   Also, the

rock should ideally have a low natural radioactivity (again to

prevent spurious background counts) and should be sufficiently

hard to support the large experimental chamber needed.

In addition to these technical problems there was the problem

of forming a satisfactory working relationship with the mining

company. Mine operators,who might be described as typically 'hard

headed', may not necessarily see any advantage to them in having a

large tank of cleaning fluid located in their mine!

In order to search for a suitable mine for the experiment,

Davis and an administrative assistant of Dodson's, Blair Munhofen,

approached the Bureau of Mines.   They were given three possible

locations, two of which they visited.   The mine which appeared to

be best suited for the experiment was the Homestake Gold Mine in

Lead, South Dakota.   This is the deepest mine in the U.S. and

there   would be no problem in meeting the cosmic-ray shielding

requirements.   Also the rock is very hard, even at great depths

(4,850 feet below the surface) and there would thus be no structural

problems in cutting out a chamber sufficiently large to house a

100,000-gallon tank.   However, there was one major obstacle to this

location. The Homestake Mining Company appeared to be somewhat

cool in their attitude towards the project and gave what was felt

to be an unrealistic estimate for the cost of excavating a suitable

chamber and providing services - $300,000-$400,000 -   which was

more than the total estimated cost of the tank and cleaning fluid.

The second choice at the time was a copper mine in Butte, Montana.

Here the mining company (the Anaconda Copper Company) were much

more favourably disposed towards the project.   The chairman of

the company liked the idea of an important scientific experiment being performed in Montana - not a part of the country noted for its science. However, there were technical problems with this location. The rock was much less stable than at Lead, and a long cylindrical cavity would have to be cut and lined with concrete, at the 4,200 feet level. This would give space for a close-fitting tank but give very little additional room. Such a long thin cylindrical tank did not appeal much to Davis. By November 1963, a suitable location for the experiment had yet to be found.

The basic design Davis planned for the experiment was similar to that which he had used previously - except on a much larger scale. An early design for the arrangement of the tank and associated equipment at Homestake is shown in Figure 3.2. Eventually the spherical design of the tank was changed to a cylindrical one as it was felt that it would be advantageous if the surrounding area could be flooded with water. A large diameter cylindrical tank would require a smaller volume than a spherical tank. This 'water shield' would cut down the background produced by fast neutrons. These could arise either from the decay of alpha particles in the rock or from spontaneous fission. The chain of reactions whereby fast neutrons could produce Ar^{37} is as follows:

$$Cl^{35} + n \rightarrow S^{35} + p$$
$$Cl^{37} + p \rightarrow Ar^{37} + n$$
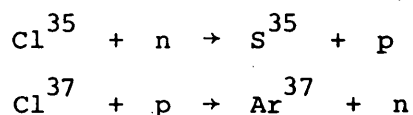
It is clear that the construction of a large tank underground would be a major engineering feat in its own right. As well as finding a suitable mine, Davis had to prepare estimates of the cost and feasibility of building such a tank. These estimates would be needed for any detailed funding application.

Fig. 3.2   An Early Design for the 100,000-gallon Experiment

PROPOSED EQUIP ARRANGEMENT
FOR
BROOKHAVEN SOLAR
NEUTRINO OBSERVATORY
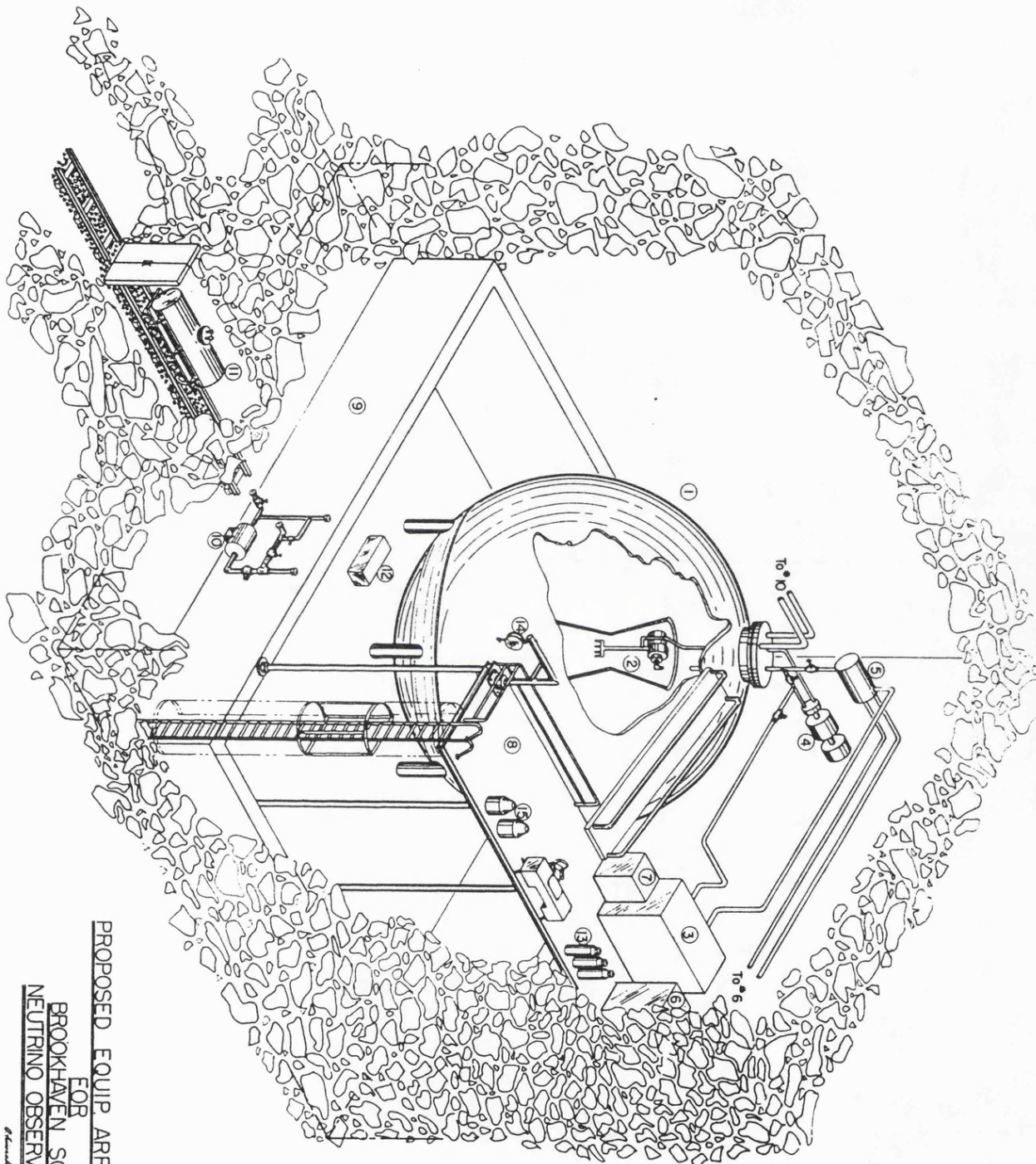
1. VESSEL
2. JET PUMP
3. GAS ANALYZING SYS.
4. HELIUM CIRCULATOR
5. CONDENSER
6. REFRIGERATION SYS.
7. CONTROL CONSOLE
8. OPERATIONS PLATFORM
9. SPILL RETAINING WALL
10. CHARGE & DRAIN SYS.
11. LIQUID TRANSPORT CAR
12. HALOGEN LEAK DETECTOR
13. HELIUM SUPPLY BOTTLES
14. HOIST
15. DEWARS

1963, Bahcall's Discovery of Analogue State Makes the Experiment Even More Feasible

Whilst Davis was engaged with these experimental problems there was a development on the theoretical front which made the prospects for funding the experiment much more favourable. Bahcall had started to prepare detailed calculations of all the likely uncertainties in the theoretical prediction. As part of this effort he recalculated the neutrino-capture cross-section for chlorine thirty-seven. He presented his result at a seminar at the Niels Bohr Institute in Copenhagen,in the late summer of 1963. During the seminar Nobel Laureate,Ben Mottelson ,asked Bahcall whether he had considered transitions from the ground state in Cl^{37} to excited states of Ar^{37}, and in particular one state known as the analogue state. These would greatly enhance the interaction probability of boron-eight neutrinos. The importance of these types of state were only just beginning to emerge in nuclear physics and Bahcall had not looked at this possibility. He described what happened next:

> I got very excited about it and I learnt a lot of nuclear
> physics...how to calculate these things...I worked extremely
> hard...I went back to Caltech and calculated things very
> accurately...I gave a seminar at Caltech where I seemed to
> get off OK. The idea was OK and in fact 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 discovery of the analogue state was important because it boosted the predicted flux of neutrinos which Davis could expect to detect. As Davis told me:

> That was crucial in arguing for the experiment. That meant
> that if you did build the experiment then you would expect
> in those days a neutrino-capture rate of between five and ten
> a day.[In other words an 'easily' measurable rate .]

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, Maurice Goldhaber. Ray and I talked to Maurice about the experiment. Ray's opinion was well we had to sell Maurice or else we wouldn't get the experiment. Ray's opinion was that this nuclear-physics trick [the analogue state] would be something that Maurice would be turned on about, because he was himself a very bright nuclear theorist amongst other things...He would think that that's a cute idea and get turned on about it. So we decided to sell him the experiment based mainly on this trick. Then more or less that worked.

As was stressed above, Goldhaber's backing was essential in order to persuade the AEC to fund the experiment. It seems that Goldhaber shared the scepticism which many 'hard' (nuclear and particle) physicists displayed towards astrophysics. As Davis told me:

> A person like Maurice Goldhaber...most physicists of that type talk about hard physics, really detailed calculations, things you know about and so on. Astrophysics is a kinda looser subject. One of the obstacles was to convince Goldhaber that the whole thing made sense...And Goldhaber was never against it, but I think he had that attitude. 'I don't think I really trust these guys with their calculations'... From Goldhaber's viewpoint, you spend the money for the experiment and [if] you don't see anything - so what? It doesn't really tell you anything.

Goldhaber's approval, it seems, was won by the influence of Bahcall who was able to stress the breakthrough in the relatively 'hard' nuclear-physics part of the calculation. Again the influence of the Kellogg group in helping to bring Davis's project to fruition can be seen.

Fowler, too, soon added his voice in the attempt to persuade Goldhaber. In a letter to Goldhaber dated November 26, 1963, he wrote:[36]

> It is my feeling that the Davis experiment is now more than ever a promising one and I think it is worth a try at submitting a proposal to NASA as well as to the AEC or other agencies. If there is any way I can help I am at your service.

Fowler's suggestion of an application to NASA stemmed from a seminar

given by Bahcall which had been attended by several NASA officials. It

seems that they expressed interest in the project.

Goldhaber, in any case, was by this stage persuaded. In his reply

to Fowler he wrote:[37]

> As soon as Ray has reached a decision on the particular mine he
> wants to use, the details of the detector and the cost of the setup,
> we would like first to approach the AEC for support. I may call
> on your help at that time.

Again, Fowler's offer of help is acknowledged - an offer which may be

taken up later.

1963 - Publicising the Proposed Experiment

Apart from convincing key individuals of the worth of the experiment

it was, of course, important to have a favourable climate of opinion for

the project. An opportunity to inform other astrophysicists of develop-

ments arose in November 1963 when a meeting on stellar evolution was

held at the Institute for Space Studies, New York.[38] This meeting was

attended by Cameron (the co-organiser), Fowler, Sears, Iben, Bahcall and

Davis amongst others. Bahcall and Davis gave a presentation at the

meeting.[39] One participant told me of the efforts of Bahcall at this

meeting in urging his colleagues of the need to give their support to

a solar-neutrino experiment.

The wider community of physicists would also need convincing. Fowler,

in particular, saw the special need for this as part of the drive to get

funds. Davis recalled what happened at the meeting referred to above:

> Willy Fowler was jumping on us and he said 'You've got to
> do this. You've got to write something that everyone can
> look at.' He said 'You'll never get the money to build it
> unless you publish something on this.' So I started writing
> something with John. And I remember during this meeting he
> and I sat in an office and we were outlining it.

Davis (1964) and Bahcall (1964a) eventually wrote separate

articles which were published 'back to back' in Physical Review

Letters in March 1964. The format of this, their first major

publication stemming from their collaboration[39a], was to be that of

all their subsequent major publications.   Davis told me why they

had decided to use this format rather than producing a jointly-

authored article:

> See,the topics were very different in that John's going to
> tell you something about solar-model calculations, cross-
> sections, and what kind of effect you expect to observe...
> I said the thing for me to do was... to write about the
> experiment in the Barberton mine. I had a result and I could
> set a limit and then say what the possibilities were for
> building a bigger one. So that's why I wanted to write it
> separately and John too.   See,I can't say anything about
> solar models, and the cross-sections, that's completely
> separate from my experience and my background. To do the
> experiment, how to do the experiment, I know all about that.

As we will see later, this publication format is of some

significance because it symbolises the nature of the partnership

that was developing between the experimental and theoretical

groups. It was a partnership which had common ends - the establishment

of a   solar-neutrino experiment - but which allowed for the

separate competences and responsibilities of the two parties.

Although the papers were written separately,both partners

kept in close touch over their respective drafts, and early drafts

of both papers were circulated around the Kellogg group. The

connection between Davis and Bahcall, and Fowler in the writing

of these papers can be seen in the following comments, taken from

a letter of Bahcall's to Davis:[40]

> Everyone here was pleased with your draft.   I am enclosing one
> copy with a few minor questions on it...
> As far as Willy and I are concerned, the enclosed draft of my
> part is final unless you want to make some changes...We can
> send out preprints from Kellogg to Willy's astronomy list
> (they probably don't read Phys. Rev. Letters). ·

This last point again shows the importance of reaching as many

scientists as possible.   The network of scientific communication centred

on the Kellogg 'Orange Aid' and 'Lemon Aid'   Preprint series enabled

many hundreds of scientists throughout the U.S. and worldwide to

get speedy access to Bahcall's and Davis's papers.[41]   This was

another small step in the development of solar-neutrino astronomy

which was helped by the resources at the command of the Caltech.

group.

That the Bahcall-Davis articles could be important for winning

direct support, or even funding, for the experiment is evident

from the reaction of at least one scientist to them.   Davis had

sent copies to Alvarez, who, as mentioned in Chapter 2, was the

physicist who had played such an important role in the original

design of the chlorine experiment.   This was the first formal

contact between these two pioneers of radiochemical neutrino

detection. Davis had only asked Alvarez for his comments but he

received, in return, a warm letter of support. Alvarez wrote:[42]

> I'm very impressed by your work to date and by your proposal
> in general.   It makes me feel fifteen years younger.
> I'd be happy to give my support in a more formal manner,
> as for example by being asked for an opinion by an NSF
> officer who might be thinking of funding the experiment.

With the funding application going ahead to the AEC, Fowler's

earlier offer of help with NASA, and now Alvarez's possible inter-

cession with the NSF, the solar-neutrino project looked to be in

a good position with respect to all the major US funding agencies.

Alvarez's response to the appearance of the Bahcall-Davis

papers was, of course, atypical in view of his own earlier conn-

ection with the project. Most people, upon reading the papers,

could not be expected to react by pledging their support and

offering help with funding agencies (few would even be in a position

to offer such assistance). The major influence of the articles was in

bringing about an atmosphere in which the eventual funding of the experiment would seem appropriate.[43]

In addition to this publication in the mainstream scientific literature, another publication, in which Bahcall's and Davis 's plans were outlined, may have been important. This was an article written by Herbert Reeves of the University of Montreal, Canada, for the semi-popular astronomy journal <u>Sky and Telescope</u>.[44] Reeves had kept in close contact with Davis about his proposed article throughout 1963 and had even proposed that Davis should be a co-author - a suggestion which Davis turned down.[45] The article was eventually published in May 1964. Its expected influence can be seen from Davis's comment to Reeves that:[46]

> The article is well written, and should help to draw support for the experiment.

One final means of drawing attention to the project was through the popular media and the press. In early 1964 an article appeared in <u>Time</u> magazine describing the experiment and it seems that the <u>New York Times</u> also gave coverage.[47] The <u>Time</u> article is reproduced in Fig. 3.3.

It was hinted to me by one respondent that the press interest had been cultivated deliberately. Ray Davis could not remember any particular effort being made in this direction but he did remark somewhat elliptically:

> Of course, I don't know what Willy Fowler is doing in the background!

Although he could not recall the press coverage in this particular case, the general importance of publicising experimental proposals was stressed by Bahcall. He told me:

> I don't remember it being crucial but all of those things helped.
> Certainly they help, popular interest and scientific recognition
> both help a great deal in getting new experiments.

Fig. 3.3

ASTROPHYSICS

Learning from Neutrinos

At Long Island's Brookhaven National Laboratory, Physicist Raymond Davis Jr. is designing one of the most extraordinary instruments known to modern science. When completed, it will be a swimming pool full of cleaning fluid, and will be installed in a deep mine to X-ray the sun.

Scientists have long been fascinated by the sun's center, where all the energy originates that supports life on earth. But the only practical way to observe this arcane spot is to study the neutrinos that are a by-product of its fierce thermonuclear reactions. The ghostly particles pay hardly any attention to matter. All except one in a billion of them pass through the sun's dense material and escape into space.

Rare Reaction. Dr. Davis estimates that about 54 billion solar neutrinos hit each square centimeter (.155 sq. in.) of the earth's surface every second. They have no effect that is normally detectable, but if they happen to collide with atoms of chlorine 37, a small fraction of the collisions results in the manufacture of radioactive argon 37. When it occurs, this rare reaction gives Dr. Davis a chance to count solar neutrinos.

Backed by funds from the Atomic Energy Commission, Dr. Davis plans to set his 100,000-gal. tank in a mine at least 5,000 ft. deep to protect it from cosmic rays. Only neutrinos will reach the tank's supply of perchlorethylene, a cleaning fluid containing about one quarter of chlorine 37. Dr. Davis estimates that out of the countless trillions of solar neutrinos that will be passing through the tank, between four and eleven per day will react with chlorine 37 atoms.

Firmer Figure. To detect these few hits, a stream of helium will bubble through the tank, sweeping any argon 37 and carrying it to a charcoal filter. Then a special instrument will count the argon atoms by means of their radioactivity. Their number will be in direct proportion to the total number of neutrinos emitted by the sun.

First thing the neutrinos will measure is the temperature of the core. Astrophysicists now estimate it at 29 million degrees F., but the neutrino observatory will give a firmer figure because the nuclear reaction that produces solar neutrinos is favored by high temperature. If Dr. Davis counts more neutrinos than current formulas predict, astrophysicists will know that the temperature of the core is higher than they have guessed.

Goldhaber, who, as the director of BNL, had had many dealings with the press was, it seems, able to advise Bahcall, who was still a novice at this kind of thing. For instance, in a postscript to a letter he sent Goldhaber, Bahcall wrote:[48]

> I took your advice with respect to the Times reporter. It however took much time. 2 hours original interview plus 1 hour correcting by phone most of her mistakes. Hope that made up for the original Snafu.

The nature of the 'original snafu' is not evident but, if nothing else, the above comment shows that Bahcall was experiencing the peculiar difficulties inherent in using this medium to draw attention to the experiment.

It is doubtful whether press articles had much influence upon scientists compared with the effect of the articles published in Physical Review Letters. However, they did have some 'spin off' for Davis. It seems that the commercial companies with which he was negotiating at the time over the construction of his apparatus were suitably impressed. As Davis wrote:[49]

> It is interesting that these tank people take us more seriously after the article in Time.

Davis Makes his Final Estimates of the Costs and Scientific Aims of the Experiment

By the start of 1964 Davis had commenced negotiations over another possible location for his experiment. This was the Sunshine Silver Mine at Kellogg, Idaho (the coincidence in the names did not go unnoticed). The company chairman happened to be interested in astronomy and thus was keen to have the experiment. Although this mine was not as suitable as the Homestake Mine (the rock at the 5,400-feet level where the experiment would be sited, was less stable, the backgrounds from natural radioactivity higher, and the support

facilities, such as hoists and access, less convenient) a realistic

estimate ($121,000) had been obtained for the excavation of the

cavity.   A mine in Switzerland and one in India also became

available at this time but in the circumstances Davis decided that

the Sunshine Mine would be the most convenient location for the

experiment. Davis had also started negotiations with three companies

over the building of a 100,000-gallon tank. It was these companies

that were impressed by the <u>Time</u> article referred to above.   It

seemed that a suitable tank could be constructed for about

$120,000. By March 1964 he had obtained estimates for the full cost

of the project. The total came to $601,000 (see fig. 3.4).

By this stage the scientific objectives of the experiment could

be clearly stated. The theoretical prediction at the time was such

that he should expect to observe a signal of 40 SNU or 10 counts

a day.   Davis claimed that he would be able to measure this to

within 10%. Also,if the prediction was too high,he expected

that he would be able to look a factor of ten lower. That is he

might be able to see as little as 4 SNU . The optimism with which

Davis viewed the experimental possibilities can be seen in the

hope, expressed at the time, that he would be able to detect a 7%

difference in the signal between measurements made at the perigee

and apogee of the Earth's orbit. This would confirm that the source

of neutrinos was indeed the Sun.   Two years of observations would

be needed to reveal this effect.   If the measurements were success-

ful he hoped to go on to make long-term measurements with a view to

looking for correlations of the neutrino luminosity with the sun-

spot cycle.[50]

Budget Estimate for Solar Neutrino Observatory
(Prepared at BNL by K.C.Hoffman, March 17, 1964.
Transcribed by R.W.Dodson, July 16, 1964.)

1.	Excavation of underground chamber including transfer of perchlor into vessel	$130,000
2.	Vessel, installed with supports	110,000
3.	Transportation of vessel parts, equipment, and erection personnel into mine. Including power and supplies for erection	10,000
4.	Equipment, installed in vessel (mixing jets, pumps, internals, instrumentation and electrical	30,000
5.	Analytical equipment including helium ciculator and refrigerated trap	21,000
6.	Engineering, inspection and travel	50,000
7.	Equipment hook-up by BNL personnel	10,000
	Subtotal	$361,000
8.	Contingency (20% of $361,000)	72,000
9.	Perchloroethylene (100,000 gallons)	168,000
	TOTAL	$601,000

Fig. 3.4   Estimated Cost of the BNL Experiment
(Taken from BNL file 'Solar Neutrinos')

The Final Push for Funding

With the main experimental details, scientific objects and costs of the project settled, Davis at last set about preparing his funding application. As the application was initially to be made to the AEC, who routinely funded most of the work carried out at Brookhaven, no formal research proposal was needed. At the time, funding decisions for individual projects which required money in excess of the annual departmental budget, were largely informal and were taken in consultation with the scientist seeking the funds and the chairman of his department. To this end, Davis, accompanied by Dodson, went to the AEC headquarters in Washington and gave a one-hour presentation of their plans. They also brought along various documents in support of the experiment, such as the detailed cost estimate, the plan of the experiment, the Physical Review Letters papers and the Reeves article.

The person in the AEC who had responsibility for making the funding decision was A.R. Van Dyken who was director of the Chemistry Branch of the Division of Physical Research. Although his decision had to go higher within the AEC for ratification, this was usually merely a formality. Van Dyken had a very close relationship with Dodson. The relationship had been cemented over the years as Dodson and Van Dyken had together built up the BNL Chemistry department. Because Dodson administered a yearly budget which was allocated by Van Dyken they had had many previous contacts. Dodson and Van Dyken would often speak to each other on the telephone to discuss financial matters. Thus, Van Dyken was able to keep Dodson informed by telephone as to how Davis's project was fairing. The first signs were not encouraging. It seemed that the AEC was rather short of money and it did not look as if the necessary

$325,000 would be added to the Brookhaven Chemistry Department budget for the 1965 financial year.

In response to this pessimistic news, Dodson decided to re-establish contact with his old friend Fowler, who was spending the summer of 1964 at the Institute of Astronomy in Cambridge. As the subsequent interchange of correspondence between Dodson and Fowler dramatically changed the fortunes of the project, lengthy excerpts will be quoted below.

Dodson's letter to Fowler of July 27 reads as follows:[51]

> Dear Willy:
>
> Sorry to bother you while you are abroad, but the time has come to take you up on your offer of a year and a half ago to help us get Ray Davis's solar neutrino experiment going. I hope you are still so inclined...
>
> This is a tight budget year, and the indications are strong that the requested funds will not be forthcoming. However, the matter is still under consideration. During the considerations of the next few months it would be helpful to have an authoritative statement from outside our group on the importance of doing the experiment. Would you be willing to provide one for our use in presenting the case?
>
> I suppose one can reduce, somewhat crudely, the question we need the answer to: why spend a substantial sum trying to measure something which is calculated with great confidence by nuclear astrophysicists- and who cares about confirming the central temperature of the sun anyway? You can imagine variations which might occur to hard pressed disbursers of funds. We have, of course, given answers ourselves; and now we need an expression of the point of view of an expert nuclear astrophysicist.
>
> I hope you can back us up. I'm not suggesting that you review the technical details of Ray's experiment of this time, because I think it would be well if you are in a position in the future to be an objective referee for these aspects...

This letter is largely self-explanatory. It seems that Dodson felt that the presentation to the AEC of a letter of support from a nuclear astrophysicist independent from the Brookhaven group would improve the funding prospects. As Fowler had already offered such assistance, it is natural that Dodson should turn to him for support. The significance of the earlier interchanges between Dodson

and Fowler can now be seen.

Of course, Fowler's independence from the Brookhaven group
in the context of this experiment is somewhat illusory. As we have
seen, Fowler had as much interest as the Brookhaven group in getting
the experiment launched. Arguably he had more interest since it
was he who had first written to Davis pointing out the B^8 solar-neutrino
possibility and it was he who had initiated direct contact with
Dodson. But,to a grant awarding body,such as the AEC, his inde-
pendent institutional position would be sufficient for him to
be granted the status of an 'independent peer'. Thus, if need
be, he could be used as an independent 'objective referee'whose
support for the experiment could be counted upon.

In view of Fowler's previous offers of assistance and his
own undoubted enthusiasm for the experiment, it is not surprising
that he replied to Dodson with a glowing letter in praise of the
experiment. In this,he reiterated the arguments for the experiment
concerning it being a crucial test of nuclear-astrophysical theory,
which we have encountered already. These arguments will be looked
at more closely in the following chapter on the theoretical
prediction. The tone of his letter can be seen from the following
extracts:[52]

> Dear Dr. Dodson
> The Brookhaven solar-neutrino experiment has my enthusiastic
> support. It is good to learn that funds are being sought from
> the AEC...I do hope that these funds will be made available.
> It is my firm conviction that the experiment proposed by Ray
> Davis is technically feasible and that the results will have
> far reaching significance.
> The observation of solar neutrinos and the detection of the
> flux at the earth is crucial to further progress in nuclear
> astrophysics and to related efforts in thermonuclear research
> and the space sciences...
> It will be clear that I place the Brookhaven solar-neutrino
> experiment in the forefront of the significant research efforts
> of the present time. The results of the experiment are eagerly
> awaited by those involved in astrophysical, thermonuclear and
> space research.

Accompanying this formal letter was a hand-written note:

> Dear Dick
>   I hope the accompanying 'formal' letter will be useful.
> If you need more (or less) please let me know.

Dodson immediately sent a copy of Fowler's formal letter to Van

Dyken at the AEC. At the same time he wrote back to Fowler thanking

him for his 'fine letter'.[53] Within a very short time (approximately

two weeks) Dodson heard by telephone from Van Dyken that the extra

money for the neutrino experiment had been placed in his budget

allocation for the forthcoming year. Soon afterwards Davis broke

the good news to Bahcall:[54]

> There has been some very encouraging news lately on funds for
> our experiment...
> Willy wrote an excellent letter to Dick Dodson supporting the
> experiment, and this helped get us over the hump.

The importance of Fowler's letter can be seen from this remark.

Dodson also drew my attention to the role of Fowler's letter. As he

informed me:[55]

> The reason I believe Fowler's July 31, 1964 letter to me was
> of crucial importance is that before we had it the funding
> prospect was doubtful and after I had used it to strengthen
> our request for funds to the AEC the prospect improved
> dramatically.

Van Dyken, who made the funding decision, stressed to me

the importance of his personal interaction with Dodson. No formal

peer-review process was initiated (it was not standard AEC practice

at that time). There are no reviews of the experiment or letters

of support, other than Fowler's to be found in those AEC files

to which I was given access.[56] Maurice Goldhaber told me that

it was possible that he also wrote a formal letter of support,

but this letter, if written, has not been traced. In any case,

Davis would not have been able to proceed at all without the support

of Goldhaber. It would seem that Fowler's letter sufficed for

the purposes of peer review as it came from a noted authority not

directly connected with the Brookhaven group.[57]

Finally, in connection with the funding of the experiment, it

is worth noting that the money was eventually transferred to the

'operating costs' part of the BNL chemistry budget, rather than the

'capital costs' part where such a major investment in equipment

would normally have gone.   The precedent for such a move had been

set by the funding of certain thermonuclear experiments which, for

security reasons, could not appear in the budget as separate items

under 'capital costs'. In general, the operating costs part of the

 budget was more flexible and money could be allocated to it

more easily. This not only made the funding of the neutrino

experiment easier but also had a beneficial effect on the chemistry

department as a whole. This was because it meant, in effect, that

the budget would remain at least at that level of 'operating costs'

for subsequent years, even after the neutrino experiment needed

less money! The benefit which the neutrino experiment brought

to the chemistry department as a whole was stressed to me by

Dodson. It enabled him to counter the grouses of other chemists

in the department who felt that the neutrino experiment was taking

the 'lion's share' of the budget.

There was to be one more cause for celebration at Brookhaven

in 1964.   Bahcall had, over the previous nine months, been trying

to get nuclear experimentalists interested in making calcium

thirty-seven.   The decay of this isotope by emission of a

positron was expected to be very similar to aspects of the chlorine

thirty-seven neutrino capture reaction and it could be used to

test indirectly Bahcall's calculations concerning the analogue state.

Bahcall had even attempted to get Maurice Goldhaber and his wife

Gertrude Schaff-Goldhaber, a notable nuclear experimentalist, to

look for this isotope. Calcium thirty-seven was finally produced

in November 1964 by a group at Brookhaven.[58] The experiment was

carried out by Arthur Poskanzer, who had beaten the Goldhabers

to the discovery. Bahcall's calculations of the analogue state

were largely confirmed and Bahcall sent a bottle of champagne,

on behalf of the Kellogg group, to Brookhaven in order to celebrate.

This was to be consumed when the solar-neutrino experiment went

underground[59] but, in the event, it was opened in Goldhaber's office

on the occasion of a visit by Fowler on November 17.[60]

As Fowler, Davis and Goldhaber toasted the solar-neutrino

experiment, they had good reason to celebrate. The partnership

between the nuclear astrophysicists of Caltech and the Brookhaven

group had enabled them to secure the necessary funding for the

experiment. This, of course, was only the first stage in bringing

the joint venture to fruition. The detector had yet to be built.

Only then would it be possible to find out just how well the nuclear

astrophysicists understood the Sun, and whether Davis would at

last manage to detect some neutrinos.

NOTES TO CHAPTER THREE

1. Letter, W. Fowler to R. Davis, January 20, 1958.

2. See Chapter 2 for details of this epidode.

3. Letter, R. Davis to W. Fowler, January 15, 1958; and letter, R. Davis to A. Cameron, February 4, 1958.

4. Op. cit. note 3.

5. Letter, W. Fowler to R. Davis, January 20, 1958.

6. Ibid.

7. Letter, A. Cameron to R. Davis, February 13, 1958.

8. Op. cit. note 3.

9. Ibid.

10. Dodson had been a research fellow at the Kellogg Radiation Laboratory in 1940.

11. Op. cit. note 5.

12. Letter, R. Davis to W.F. Libby, April 11, 1958.

13. The results are given in Davis (1964).

14. R.W. Kavanagh, 'Be^7(p,γ)B^8 and Be^7(d,p)Be^8 Cross-Section Measurements', Bulletin of the American Physical Society, 4, 1958, 444; and R.W. Kavanagh, 'Proton Capture on Be^7', Nuclear Physics, 15, 1960, 411-420.

15. Letter, R. Davis to E. Kinkead, December 7, 1960.

16. Letter, R. Davis to R. Jastrow, October 23, 1961.

17. Letter, R. Davis to J. Bahcall, February 19, 1962.

18. Letter, J. Bahcall to R. Davis, March 5, 1962.

19. J. Bahcall, 'Electron Capture and Nuclear Matrix Elements of Be^7', Physical Review, 128, 1962, 1297-1301.

20. P.D. Parker and R.W. Kavanagh, 'He^3(α,γ)Be^7 Reaction', Physical Review , 131, 1963, 1578-82.

21. Letter, W. Fowler to R. Davis, October 17, 1962.

22. Letter, F. Reines to W. Fowler, November 29, 1962.

23. Letter, R. Davis to W. Fowler, November 16, 1962.

24. Letter, J. Bahcall to R. Davis, November 20, 1962.

25. Letter, J. Bahcall to R. Davis, November 29, 1962.

26. Letter, R. Davis to J. Bahcall, December 20, 1962.

27. Letter, J. Bahcall to R. Davis, January 3, 1963.

28. Interview material with G. Ragosa and A.R. Van Dyken.

29. Interview material with R. Dodson.

30. Letter, W. Fowler to R. Dodson, January 4, 1963.

31. Bahcall and Davis (1980: 12-13) claim that Lauritsen was important in mobilising support for the experiment. However, no-one was able to recall just what form this support took. Dodson, who was a friend of Lauritsen's, did not know either (letter, R. Dodson to T. Pinch, May 30, 1979).

32. Letter, R. Dodson to W. Fowler, January 9, 1963.

33. Ibid.

34. Letter, R. Davis to J. Bahcall, July 15, 1963.

35. Ibid.; see also, letter, R. Davis to J. Bahcall, January 21, 1964.

36. Letter, W. Fowler to M. Goldhaber, November 26, 1963.

37. Letter, M. Goldhaber to W. Fowler, December 3, 1963.

38. The Proceedings of this meeting were published much later. R.F. Stein and A.G.W. Cameron (eds.), Stellar Evolution, New York:Plenum Press, 1966.

39. J.N. Bahcall and R. Davis, Jr., 'On the Problem of Detecting Solar Neutrinos', in Stein and Cameron, op. cit., note 38, 241-3.

39a. The joint presentation of Bahcall and Davis at the conference was actually published as a joint paper (op. cit., note 39). However, this was only a brief schematic paper unlike the more important publication in Physical Review Letters.

40. Letter, J. Bahcall to R. Davis, December 6, 1963.

41. 'Orange Aid' preprints are sent to astrophysicists and 'Lemon Aid' preprints to nuclear physicists. When I visited Kellogg in November 1978 the names and addresses of 700 scientists were on these preprint lists.

42. Letter, L. Alvarez to R. Davis, undated. Alvarez scribbled his reply on Davis's own letter to Alvarez of December 3, 1963.

43. The suggestion being made here is not that these scientific publications served the <u>sole</u> purposes of winning funding and support. Clearly they also served to communicate scientific information. The point is that scientific activity is multi-facetted. However, in the past there has not been sufficient emphasis placed on the fund-raising side of scientific publication.

44. H. Reeves, 'The Detection of Solar Neutrinos', <u>Sky and Telescope</u>, 27, 1964, 276-8.

45. Davis wrote that 'it would be much better for you to be the sole author of the article' (Letter, R. Davis to H. Reeves, November 6, 1963.) It is possible that Davis declined to be co-author because the article would seem more authoritative coming from outside Brookhaven. This article was used by Davis and Dodson in their bid to get funds from the AEC (see below).

46. Op. cit. note 45.

47. 'Astrophysics: Learning from Neutrinos', <u>Time</u> , January 3, 1964. I have not been able to trace the article in the <u>New York Times</u>. However, its existence can be inferred from Bahcall's comment to Goldhaber (below and note 48).

48. Letter, J. Bahcall to G. Scharff-Goldhaber and M. Goldhaber, January 23, 1964.

49. Letter, R. Davis to J. Bahcall, January 21, 1964.

50. The above information comes from notes and internal memoranda contained in the Brookhaven National Laboratory, Chemistry Department file on solar neutrinos.

51. Letter, R. Dodson to W. Fowler, July 27, 1964.

52. Letter, W. Fowler to R. Dodson, July 31, 1964.

53. Letter, R. Dodson to W. Fowler, August 19, 1964.

54. Letter, R. Davis to J. Bahcall, September 14, 1964.

55. Letter, R. Dodson to T. Pinch, May 30, 1979.

56. The AEC has undergone two major re-organisations, when it became ERDA for a short while and then the DOE. It is possible that letters have been lost but it is perhaps significant that Fowler's letter still remained on the DOE files to which I was given access.

57. Although no formal peer-review process was used in this case, it can be seen that Fowler's letter gave the appearance of peer review. It is possible that the informal contacts which have been so important in this case have a role to play even when

57. contd..
   formal peer review is operated, such as by the NSF. Cole,
   Rubin and Cole (1978) conclude formal peer review is working
   satisfactorily; however, not all US science (or even a large
   part of it) is funded by formal peer review (e.g., National
   Laboratories seem to use more informal criteria) and, even if
   a formal system is used, informal processes may serve to
   undermine the system. These informal processes will probably
   only show up in in-depth studies of funding decisions such
   as the present research.

58. P.L. Reeder, A.M. Poskanzer and R.A. Esterlund, 'New Delayed-
   Proton Emitters: Ti^{41}, Ca^{37}, and Ar^{33}', Physical Review
   Letters, 13, 1964, 767-9.

59. See letter, M. Goldhaber to J. Bahcall, November 5, 1964.

60. See letter, A. Poskanzer to J. Bahcall, November 18, 1964.

CHAPTER FOUR

THEORETICAL DEVELOPMENTS IN SOLAR-NEUTRINO ASTRONOMY, 1958-1964

The theoretical developments which underpinned the experimental

project between 1958 and 1964 forms the subject matter of this chapter.

As we saw in Chapter 2, it was developments in nuclear-astrophysical

theory and the intercession of the nuclear astrophysicists Fowler

and Cameron which first made solar-neutrino detection a feasible

proposition in 1958. Also, as we saw in Chapter 3, it was the con-

tinuing interest and involvement of theoreticians, and in particular

the nuclear astrophysicists at Caltech, which eventually led to

the successful funding of the Davis experiment. To reiterate a

point that will be made throughout this thesis: the history of

solar-neutrino astronomy is the history of the interaction between

experimental and theoretical endeavours.

In this chapter the various changes in the predicted neutrino

flux are examined more closely. Particular attention is paid to

the theoretical prediction of 1964, which was so important for

the funding of the experiment. In terms of the graph of theoretical

predictions over time shown in Fig. 1.1, the focus is on the period

leading up to the 1964 prediction of 40 SNU.

Much of this chapter, like the preceding one, is descriptive,

and again, much of the sociological relevance will only become

apparent later (in Chapter 10 particularly).The material presented

here is somewhat sparser than that given earlier on experimental

developments. There are two reasons for this.

Firstly, there was not much fundamental theoretical activity

going on over the period of interest. The main theoretical developments

in nuclear astrophysics (as outlined in Chapter 2) were, by the

start of this epoch, already largely complete. By 1958 the basic

principles of stellar structure had been laid down and the nuclear-

energy generation mechanisms of stars were considered to be well

understood, especially for a main-sequence star like the Sun. The

central theoretical concern of the time was the development of

a detailed solar model using the most recent input data in order to

produce an exact numerical prediction of the expected neutrino fluxes.[1]

Although, as Fig. 1.1 clearly shows, the precise value of this

prediction did change over the years, this was caused not so much

by developments in the underlying physical theory, but rather by

more mundane changes in the values of the input parameters (such

as the nuclear cross-sections) upon which the detailed calculation

was based. Much of this chapter, therefore, is concerned with how

values of various key pieces of input data were derived.

A second reason for the dry appearance of parts of this chapter

has to do with the difference between the description of theoretical

and experimental activity from the point of view of historical

reconstruction. The production of a theoretical prediction does

not usually leave the rich trail of artefacts which accompanies

the construction of a large and expensive experiment. As we have

seen, the need to raise funding and find a suitable location for

the experiment led to many letters being exchanged amongst the.

interested parties. On the other hand, in order to produce a

theoretical prediction all that is required is, at a minimum,

a pencil and the back of an envelope.[2] And, since most scientists

dispatch their envelopes to the litter bin, there is not much left

for the historian to recover!

It so happens that the generation of the solar-neutrino predictions did involve some more visible activity than this. For instance, several different types of theoretical competence were called upon and this led to interactions and correspondence between different sorts of theoretician. In addition, the collaborative nature of the venture required the theoreticians to keep the experimenter informed of their progress. The correspondence amongst theoreticians and between theoreticians and experimenter provides much interesting material in this case. Interviews with theoreticians in which they recalled their work over the period also provide a further informative source of data. Nevertheless, despite all the advantages which this particular theoretical episode presents, theorising is, on the whole, a less visible activity than experimentation. Often, all that remains for the historian or sociologist to work with is the final theoretical result, as reported in the scientific literature. The hazards of basing an account purely on what is to be found in the scientific literature are too well-known to need reiteration here.[3]

S_{34} and S_{17}

The first attempt to produce a concise prediction of the flux of neutrinos expected in the chlorine-argon experiment was made in 1958. The event which prompted this calculation was, as has been stressed earlier, the discovery by Holmgren and Johnston that the He^3-He^4 cross-section was much larger than expected. Before that discovery there had been little point in calculating the number of neutrinos which could be detected in a Davis-type experiment, since there were thought to be no neutrinos produced of sufficient energy to trigger the detector.

As can be seen from Figure 2.2, where the full pp-chain is outlined, if the He^3+He^4 reaction takes place then the pp-chain can be completed by the branch which goes through the decay of boron eight. The neutrinos emitted by the decay of boron eight - the so-called 'boron-eight neutrinos' -have sufficient energy to be detected by a chlorine-argon experiment.[4] The number of these neutrinos produced will depend on the comparative rate of He^3 consumption by the $He^3 + He^4$ reaction vis-à-vis the He^3+He^3 reaction (this comparative rate is known as the branching ratio). In addition, the rate of the Be^7+p reaction will also affect the number of boron-eight neutrinos produced. The larger the cross-section for this reaction, the faster it will proceed and the greater will be the number of neutrinos produced.

Before Holmgren's and Johnston's measurement, the He^3+He^4 cross-section for solar energies (known as S_{34}) had been calculated to have a value of 0.6 eV-barns. This was too small to produce any significant amounts of beryllium seven and hence any boron-eight neutrinos. Holmgren's and Johnston's measured value was much larger (1.2 keV-barns) and held out much more promise for the production of beryllium seven. In the light of this experimental result, theoreticians re-examined the He^3+He^4 reaction.[5] They found that, by describing the reaction mechanism in a different way, they could obtain a result of the same order of magnitude as that obtained, experimentally, by Holmgren and Johnston.

As was pointed out in Chapter 3, once it became clear that there might be a flux of neutrinos to detect, Davis, encouraged by the letters he received from Fowler and Cameron, made an estimate of the number of events he should expect from boron-eight

neutrinos. He estimated the boron-eight neutrino flux at the

Earth's surface to be $\phi_B 8 = 4.3 \times 10^{10}$ neutrinos $cm^{-2}sec^{-1}$ (equiva-

lent to 3,900 SNU , or a capture rate of 7.7 events per day in a

one-thousand-gallon tank).[6]

Whether this number of neutrinos would be emitted in full

depended crucially on the, as yet, unmeasured value of the Be^7-p

cross-section (S_{17}). Although there were theoretical estimates

of S_{17} available these were rather uncertain. The importance

of S_{17} was pointed out by Fowler[7] in his letter to Davis in response

to Davis's initial calculation and elaborated on in his (1958)

article in the Astrophysical Journal. Fowler found that, for the

most probable internal temperature of the Sun and with $S_{17} > 10$ keV-

barns (a value suggested by some calculations), $\phi_B 8 = 2 \times 10^{10}$

neutrinos $cm^{-2}sec^{-1}$. If, on the other hand, $S_{17} < 10$ keV-barns,

then he estimated the flux to be negligible. He noted that the

'probability that S_{17} exceeds 10 keV-barns is rather low' (Fowler,

1958: 556). Thus a very large flux of neutrinos seemed unlikely.

Cameron (1958) also calculated the likely flux of boron-

eight neutrinos and obtained $\phi_B 8 = 4 \times 10^9$ neutrinos $cm^{-2}sec^{-1}$.

The discrepancy between this value and the slightly larger value

obtained by Fowler seems to have stemmed from Cameron's preferred

choice of $S_{17} = 1.5$ keV-barns(which he estimated from his own

calculations). As we shall see throughout this chapter, and other

chapters on the theoretical prediction (6 and 8), different pre-

dictions of the neutrino fluxes can be obtained by different choices

of nuclear parameters . The discrepancy between Fowler's and

Cameron's estimate of the boron-eight flux does not seem to have

been significant enough to have warranted attention at the time.

In any case, the exact numerical value of the prediction was of little consequence at this stage given the large uncertainties in the values of S_{17}.

Although, as we saw in Chapter 3, Davis went ahead and moved his 1,000-gallon detector into a mine, any immediate hope of detecting boron-eight neutrinos vanished in late 1958 when Kavanagh reported his initial measurements of the Be^7-p cross-section.[8] He found S_{17} to be only 0.02 keV-barns.[9] With this value the boron-eight flux only amounted to $\sim 10^7$ neutrinos $cm^{-2}sec^{-1}$ - an amount considerably lower than both Fowler's and Cameron's previous estimates.

The Li^4 Possibility

The only other theoretical hope for neutrino detection at the time seemed to reside in the Li^4 possibility (discussed briefly in Chapter 3). Li^4 could be formed by the reaction chain:

$$He^3 + p \rightarrow Li^4 + \gamma$$
$$Li^4 \rightarrow He^4 + e^+ + \nu$$
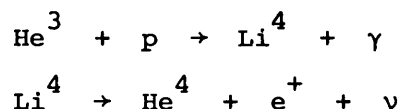
The neutrinos emitted would be very energetic and should be detected by Davis's experiment.[10] However, from the theoretical point of view the formation of Li^4 seemed unlikely since it was thought to be particle unstable. As Fowler commented in his 1960 review:

> Cogent theoretical arguments based on the empirical
> systematics of light nuclei can be advanced against the
> stability of Li^4 to such decay (Fowler, 1960: 212).[11]

However, it seems that theoretical arguments alone could not settle the issue, for Fowler went on to remark:

> In spite of these <u>theoretical</u> arguments against the existence
> of Li^4...it is clear that more conclusive <u>experimental</u> evidence
> on the matter is urgently needed. Theoretical arguments based
> on specific models cannot be trusted too far. (Fowler, 1960:214,
> his emphasis).

One potential source of experimental evidence was solar-neutrino

experiments. As Fowler remarked in a letter to Reines[12], written

at about the same time as the above comment appeared:

> All of this astrophysical uncertainty [over the formation
> of Li^4] can be swept away even by a negative [solar-
> neutrino] experiment.

If neutrinos were not detected in a solar-neutrino experiment it would

suggest that Li^4 was indeed particle unstable.

The importance of experiments in settling such issues reflects

the relative lack of precision of nuclear theory. Although the

general properties and mechanisms of nuclear reactions were well

understood and predictions for simple nuclear systems were reasonably

accurate, predictions for reactions involving complex systems (i.e.

many-body systems) could not be trusted (as indicated by the

revisions noted above in the theoretical calculations of S_{34} and S_{17}).

As it turned out, the new experimental evidence on the stability

of Li^4 which Fowler hoped for came, not from solar-neutrino experi-

ments, but from more conventional nuclear-physics experiments.

Fowler added a note in proof to his 1960 review pointing out that

Bashkin, Kavanagh and Parker[13] (working at the Kellogg Laboratory)

had unsuccessfully searched for the formation of Li^4 by the reaction

$He^3 + p \rightarrow Li^4 + \gamma$.

Although Davis's measurement with his 1,000-gallon tank was

thus not needed to set a limit on the particle stability of

lithium four, it later became clear that his results could be used

to rule out the formation of lithium four by another process - a

low energy resonance in the $He^3 + p$ reaction (a resonance is a sudden

peak in the cross-section). This possibility was noted by Parker,

Bahcall and Fowler; they wrote:

...the only experimental limit that can be placed at present
on the astrophysical importance of the Li^4 termination
derives from the solar neutrino measurements of Davis.
(Parker, Bahcall and Fowler, 1964: 613).

The failure of Davis to detect any lithium-four neutrinos in his

1000-gallon experiment (as mentioned in Chapter 3), thus indicating

the absence of lithium four in the Sun, can be seen to be one of

the first significant contributions made to nuclear astrophysics

by solar-neutrino astronomy.

The 1960 Solar Model - Sears Becomes Involved

At the same colloquium at which Fowler presented his afore-

mentioned review in 1960, a leading stellar modeller, R. Sears,

presented his latest solar model.[14]   Before going on to discuss

the significance of this particular model a few wor ds must be said

about solar models in general(a more detailed discussion will be given

in Chapter 8).

Solar models provide a means of accurately determining the

temperature-density profile of the interior of the Sun. As the

temperature and density at different points in the solar interior

govern the rates at which the nuclear reactions proceed, solar

models are important in the calculation of neutrino fluxes.

The solar model consists of certain fundamental equations (of a

partial-differential type) which describe the basic physical

processes of the Sun. Given an initial chemical composition for

the primaeval Sun, a series of models can be constructed which

represents the evolution of the Sun. The solar model is the one

that matches certain observed properties of the present Sun,

after the correct period of evolution (4.7 billion years). It is

this model which gives a picture of the temperature and density

throughout the current solar interior. The model requires many

input parameters, such as nuclear-reaction cross-sections and
opacity tables (these give the distribution of elements throughout
the Sun). The production of such a model is a complex mathematical
operation and the problem can only be solved with the aid of a large
computer programme.

Sears's model used all the latest values for the input parameters
and was the first to include the new value of S_{34}.[15] As such ,
his model was considered to be the most accurate thus far constructed.

With this model newly available, Davis made a fresh calculation
of the boron-eight neutrino flux. He estimated that he should observe
a signal of approximately 50 SNU [16] - a signal which, as we saw
in Chapter 3, he hoped might be detected with a much larger (100,000-
gallon) experiment. The calculation was at this stage, however,
still highly provisional, bearing in mind the variety of uncertainties
in the array of inputs fed into the solar model. If a large (and
costly) experiment was to be performed, a much more detailed and
elaborate calculation would have to be undertaken. Davis, up until
this point, had been able to make his own highly approximate
calculations from the information which theorists such as Fowler,
Cameron and Sears had provided. However, a more detailed calculation
would have to be carried out by a specialist theorist. With this
aim in mind, Davis, with the encouragement of Fowler, wrote to
the one specialist who might be capable of carrying out such a
calculation in all its gory detail[17]. Thus Davis got in contact
with John Bahcall, who, as we have mentioned already, was to play
a pivotal role in subsequent developments.

The 1962 Caltech Calculation

Bahcall was expert in the calculation of beta-decay rates in stellar interiors. His calculations were more refined than those previously attempted. In particular, he took into account the special physical conditions peculiar to stellar interiors and the effect of these on nuclear processes. As a result, Bahcall had been able to suggest some modifications in the Burbidges-Fowler-Hoyle schema for the synthesis of the elements.[18]

The particular calculation which Davis hoped Bahcall would be able to work on concerned the reaction

$$Be^7 + e^- \rightarrow Li^7 + \nu$$
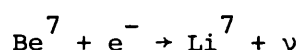
This reaction competes with the consumption of beryllium seven by proton capture (see Figure 2.2). The faster that the electron-capture reaction proceeds, the fewer will be the number of beryllium-seven atoms available for proton capture. And, since the proton capture eventually produces the boron-eight neutrinos, the fewer will be their number too. Thus knowing the rate of electron capture accurately is important in the estimation of the magnitude of the boron-eight neutrino flux.

Bahcall had not calculated this reaction rate when he received Davis's letter in February 1962. In view of Bahcall's role in subsequent development, it is worth documenting here why he decided to work on the solar-neutrino computations at all. Bahcall described to me his reaction upon receiving Davis's letter:

> It was certainly the beginning of my interest and I would
> say in a certain sense was the first thing which led to the
> sequence of events that got the experiment funded. Because
> what actually happened was I had not calculated that rate
> [Be^7 capture], that rate involved me in learning some nuclear
> physics of a slightly different kind than I had done until
> then.

His decision to go ahead and do this calculation was taken in the following way:

> For me I think the turning point, the reason I decided to
> work on it, is the result of a conversation with a
> theoretical physicist at the University of Indiana...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...As a result of that conversation I became
> convinced that it was...really a unique way of testing an
> otherwise very fundamental theory.

It can be seen that Bahcall, like the other nuclear astro-physicists, was attracted by the prospect of a crucial test of the theory of nuclear synthesis in stars. In the sense that Bahcall was not interested in the detection of solar neutrinos per se, but in the theoretical consequences of their detection, then his interest in the experiment can be said to be an 'instrumental' one. The role of instrumental interests in science in general will be discussed in Chapter 10.

Once he became convinced of the worth of the project Bahcall started to calculate the rate at which beryllium seven captured electrons. By May 1962 he had completed the calculation. His results were more precise than previous work because he took into account the attractive electron-nucleus interaction (an electro-magnetic interaction which perturbed the density distribution of electrons in the vicinity of the nucleus). Bahcall found the rate of electron capture by beryllium seven to be slightly less than previous estimates.[19] Although this calculation alone did not throw up any surprises, it was important because it demonstrated Bahcall's commitment to the solar-neutrino project, a commitment which he was able to pursue by virtue of his fellowship at Caltech working with Fowler's group. It was at Caltech, in the summer of

1962, that other fine computational details that were likely to be important for the expected numbers of boron-eight neutrinos were investigated.

In order to make a sufficiently detailed calculation, several types of expertise were needed. It first of all required someone with a theoretical nuclear physics background who could calculate all the relevant nuclear-reaction rates (both in the Sun and in the experiment) - this was Bahcall's particular expertise. In addition, knowledge of the latest experimental values of the nuclear-physics cross-sections was needed. This was provided by Fowler and those nuclear physicists working with him in the Kellogg Laboratory. Lastly, it was necessary to have a detailed solar model which used all the latest input parameters and from which the neutrino fluxes could be calculated. This model was provided by two astrophysicists who were working at Kellogg at the time, R. Sears (whose work we encountered above) and Icko Iben. Sears and Iben were two of the leading specialists in the construction of computer models of stars.

Bahcall knew of the work of Sears and Iben before he joined the Caltech group; as he told me:

> I had in mind organising those guys to help me calculate
> the neutrino flux accurately; that was the project I
> had in mind.

The stellar-model specialists, and in particular Iben, were, however, not as keen on the project as Bahcall. They were more interested in the late stages of stellar evolution and could see little point in making a very detailed calculation for the Sun, which was con-sidered to be well understood anyway. The situation was described to me by one respondent, familiar with the Kellogg group, as follows:

> You see most astrophysicists are interested in stellar
> models in general, and what you would like to do is to
> develop the theory of the understanding of the evolution

> of stars, so they are interested in the general case. So
> what John [Bahcall] said was 'I want you to calculate
> the Sun, the best you can, because you know all about the
> Sun.'   The replies were 'Why do that? The Sun is just
> one star, you know it's a typical star but why do you have
> to know that much about the Sun?   There is no need to
> calculate that well.'

Eventually it seems Bahcall persuaded Sears to help him. However,

Sears needed to use Iben's computer programme, which was the best

for the job. What happened next was described to me by Bahcall

as follows:

> Icko was very unwilling to do that, he thought it was a
> 'boon docker' [low-status problem]. He wasn't really
> interested in that. He wanted to find out how stars
> evolve, that was his thing...He didn't want to bother
> to use his code for this purpose. So it required some
> intervention from Willy Fowler...He agreed to use his
> administrative control to get Icko to let Dick Sears
> use his programme, and I think that what happened was
> he taught Dick how to use his programme in a few hours and
> Dick punched the cards.

Bahcall told me his role was:

> I sort of told them what we wanted out, what we wanted to
> calculate, and we calculated it with some improved reaction
> rates.

The most important of the new values for the reaction rates used

was that for the He^3 + He^4 reaction, the cross-section of which had

just been remeasured by Parker and Kavanagh at Kellogg.[20]   Their

result, S_{34} = 0.5 keV-barns, was smaller than the previous value

obtained by Holmgren and Johnston (1.2 keV-barns).

The result of the Kellogg calculations of the boron-eight

flux ($\phi_B 8$ = 3.6 x 10^7 neutrinos $cm^{-2} sec^{-1}$) was communicated to

Davis by Fowler.[21]   This flux still seemed to be too small to

warrant an experiment. One thing the computation did reveal,

however, was the acute temperature dependence of the B^8 flux.

Fowler pointed out that, if the temperature of the Sun (T_{eff}) was

actually higher by ten or twenty percent of tne standard value, then

the B^8 neutrino flux would be increased by factors of seven and forty

respectively. The possibility of such an error in the temperature

seemed unlikely but a new rationale for the Davis experiment was

now apparent - it would constitute a very sensitive thermometer.

Although it might not detect any neutrinos, it could be used to

set an upper limit on the solar temperature. As Fowler wrote to

Davis:[22]

> We feel that the probability for such an error in T_{eff} is
> very small. On the other hand, the positive detection of
> high-energy neutrinos from the Sun would certainly shake up
> the astronomers and nuclear physicists and this might be a
> good thing! A negative result would also be valuable in
> that it would provide an upper limit for the central temperature
> of the sun. All valid solar models must have a central
> temperature below this limit.

Davis, whose main aim, as we have seen, was to detect something,

was naturally disappointed when he heard of the results of the

latest calculation. As he wrote to Fowler:[23]

> It appears that the harder one looks at the B^8 neutrino
> flux the lower it gets.

Beryllium-Seven Neutrinos

Davis, however, also drew the attention of the Kellogg group

to a point which they had neglected - a point which made the

prospects for his proposed experiment slightly more encouraging.

He noted that the neutrinos produced by the electron capture of

beryllium seven (see Figure 2.2) had sufficient energy (just)

to trigger his detector.[24] Indeed, Davis estimated that he would

see more neutrinos from this reaction than from the decay of

boron eight. He calculated that with a predicted flux, $\phi_{Be}7 =$

3.2×10^{10} neutrinos $cm^{-2} sec^{-1}$, he should expect 2.2 captures/day

in a 100,000-gallon tank whilst only 0.05 captures/day would result

from the boron-eight flux. Davis felt that these calculations meant

the 100,000-gallon experiment 'still looks feasible'.[25]

The Kellogg Group soon wrote back to Davis acknowledging the

importance of the beryllium-seven flux. As Bahcall commented:[26]

> this is a very nice point, one which we had overlooked
> completely.

However, Bahcall was not as optimistic over the contribution made

by the beryllium-seven flux to the overall number of events to be

detected. This was because he estimated the cross-section for

beryllium-seven neutrino capture by chlorine thirty-seven to be a

factor of twenty less than the value upon which Davis had based

his calculation. (It must be remembered that the number of events

detected depends on the capture cross-section as well as the flux).

It seemed to Bahcall that there would be little hope of detecting

beryllium-seven neutrinos.

Bahcall's pessimism over the importance of the beryllium-seven

flux was, however, to be shortlived. Within a week he had dis-

covered an error in his estimation of the capture cross-section

(this was because he had used an incorrect value for one of the

parameters).[27] His revised estimate was in close agreement with

the original value quoted by Davis! Thus it seemed beryllium-seven

neutrinos could be detected after all.

Bahcall immediately wrote back to Davis pointing out his mistake

He concluded his letter by drawing Davis's attention to yet

another error (this time made by Davis) in the calculations. He wrote:[28]

> I am now leary of suggesting another result in contradiction
> to yours, but I obtain 0.5 captures per day per 100,000
> gallons for B^8 neutrinos rather than the 0.05 captures per
> day which you give...

Davis, upon reworking the calculation, agreed that Bahcall's pre-

diction of the number of events due to the boron-eight flux was in

fact correct.[29] He had made a simple numerical error in his earlier

calculation. However, he was still not certain that he was in agreement with all Bahcall's predictions. In particular he was puzzled as to why Bahcall had used a beryllium-seven flux of $\phi_{Be}7 = 2 \times 10^{10}$ neutrinos $cm^{-2}sec^{-1}$ in his letter setting out the results, but preferred a value of $\phi_{Be}7 = 1 \times 10^{10}$ neutrinos $cm^{-2}sec^{-1}$ in a preprint he had co-authored with Fowler, Iben and Sears (Bahcall, Fowler, Iben and Sears, 1963). Davis felt the higher value (i.e. the one given in the letter) seemed 'reasonable'. Davis concluded his letter by tabulating the various results that had been obtained (see Fig. 4.1). It was this prediction which convinced Davis that a 100,000-gallon experiment would be feasible.

Figure 4.1   Theoretical Predictions 1963

Source	ϕ $cm^{-2}sec^{-1}$	σ cm^2	Captures/10^5 gallons	SNUs
Be^7	2.0×10^{10}	2.7×10^{-46}	1.1	5.4
B^8	3.6×10^7	6.5×10^{-44}	0.5	2.34
		Total	1.6	7.7

The anomaly over the differing values for the beryllium-seven flux was cleared up by Bahcall in his next letter to Davis.[30] He indicated that his earlier value of $\phi_{Be}7 = 2.0 \times 10^{10}$ neutrinos $cm^{-2}sec^{-1}$ was actually a guess made before the detailed work carried out by Iben and Sears and that the preferred result was the smaller value given in the paper. With this flux the total signal became 5 SNU. Bahcall, at this point, felt able to express agreement with the rest of Davis's figures.

It was this letter by Bahcall to Davis which spurred Fowler

into writing in support of getting the experiment funded (see Chapter

3, p.92) .Thus this particular calculation played an important

part in the development of the project.

Discussion of the Caltech Calculation and Davis's Response

The above description of the minutiae of the negotiations between

Bahcall and Davis as different results were exchanged has not been

included solely for the sake of historical exegesis.   Something of

the messy character of making a theoretical prediction is reflected

in this episode.   This messiness is not normally visible by the

time such predictions appear in the literature. The production of a

theoretical result in science seems to be no more   straightforward an

activity than the production of an experimental result.[31]

Most of the discussion between Bahcall and Davis over the

result concerned the standardised numerical part of the calculation,

and most disagreements could quickly be put down to 'mistakes' or

'errors', which were readily acknowledged as such by both parties.

It is as if two scientists had been discussing an experimental result

and one of them realised that the other had forgotten to switch on

part of the apparatus. Such a mistake would be quickly acknowledged.[32]

However, it is not clear that the disagreement between Bahcall and

Davis over the preferred value of the Be^7 flux could be settled quite

as easily.   In this case, Davis seems to have been content to accede

to Bahcall's views – and after all Bahcall was the theoretician![33]

However, as we shall see later, there are aspects of the theoretical

calculation where theoreticians can disagree amongst : themselves,

and where the production of a con sensus is much more problematic.

Interestingly enough, the possibility that the above prediction

of the neutrino fluxes might contain a further 'error', unrecognised

at the time and of no consequence today, was pointed out to me in

passing by one of the participants in the above calculation. This

scientist told me that when the error was discovered, the pre-

diction was later jokingly referred to as FIBS (Fowler, Iben

Bahcall and Sears). He went on to say:

> I think we made a mistake in calculating the age of the model,
> and thought it was four and a half billion years old, and it
> was only three billion. It turned out two or three years
> later...that was 'FIBS' because we had told a lie about the
> age of the Sun.

Whether this error would have had a significant effect on the neutrino

flux is an open question.[34] However, the above comment does serve

to remind us that theory is no more a guarantee of immutable

scientific truth than experiment. It seems likely that most theore-

tical results, like experimental results, are uncontentious and thus

there is often little emphasis placed upon checking them.[35]

The Precision of the Prediction

Once Bahcall and his colleagues at Caltech had estimated the

magnitude of the neutrino fluxes' they set about assessing the

error in the predictions due to possible uncertainties in the input

parameters upon which the solar model was based.[36] Four areas of

uncertainty were investigated. These were: (1), composition un-

certainties (uncertainties in the amount of hydrogen, helium and

heavy metals which make up the Sun), (2), solar age uncertainties,

(3), opacity variations (the opacities describe the distribution

of elements throughout the Sun and are crucial in the determination

of the rate of flow of energy out of the Sun), and (4), nuclear-

physics uncertainties. The detailed work on the problem was carried

out by Sears (1964, 1966). He computed several different models

(referred to in more detail below) in order to see how the neutrino

fluxes varied with the choice of different input parameters. His

conclusion was:

> All models predict nearly the same neutrino fluxes, within a
> factor of two, so we can have some confidence in the neutrino
> fluxes predicted from these models (Sears, 1966: 248).

Bahcall was even more confident of the certainty with which the

prediction was known. He informed [37] Davis:

> You will be encouraged to know that we can't find uncertainties
> in the model or the parameters (1) - (4) [those referred to
> above] that lower the predicted fluxes by more than 40 percent.
> Moreover, the biggest uncertainty (relative to the ν fluxes)
> is probably the S_o for He^3-He^3 [i.e. S_{33}] and this raises
> somewhat the predicted fluxes.

This comment by Bahcall reflects the particular concern over

uncertainties which might reduce the neutrino fluxes. As the

experiment could easily detect a larger flux,it was of more importance

to explore the lower limits of the predicted value. Thus Bahcall

was encouraged that the largestuncertainty, which was in the value

of S_{33}, would probably lead to an enhanced flux.

The Case of S_{33}

The uncertainty in the He^3-He^3 cross-section had first been

noticed by the Caltech nuclear physicist, Peter Parker, when he was

carrying out a detailed review of all the nuclear-physics data

relevant to the pp-chain (this review was eventually published

as Parker, Bahcall and Fowler, 1964). As this cross-section plays

an important part in events described later (in this chapter and

in Chapter 6) the nuclear-physics issues it raises will be gone into

at some depth.

The key point about most of the nuclear cross-sections that

are used in the solar-neutrino calculations is that they are measured

in the laboratory at much higher energies than pertain in the Sun.

This is because the reactions proceed too slowly to be studied in the laboratory by the use of energies comparable to those found in the Sun. The cross-sections are studied at much higher energies (several hundreds of keV) in the laboratory and then the results are extrapolated down to low energies (a few keV). The main energy dependence of the cross-section at low energies comes from the Coulomb field (a repulsive electromagnetic field which exists between like-charged particles). The usual procedure is to factor out an exponential factor (corresponding to the Coulomb field) from the cross-section data in order that a straight line extrapolation can be made. The remaining cross-section (known as the S-factor) is assumed to be constant or else to vary linearly with energy. Thus, the extrapolation to low energies is carried out simply by a straight-line extension of the S-factor plot against energy.

In practice the S-factor may display some non-linear energy dependence. In such cases it is often not clear which is the best extrapolation to take and nuclear-theoretical considerations and 'parsimony' often combine to produce an 'educated guess'. The arbitrary element in the extrapolation procedure is well exemplified by the case of the $He^3 + He^3$ reaction data.

In Fowler's early reviews of the stellar nuclear-physics data (e.g. 1954, 1960), the value of S_{33} was given as 1100 keV-barns. This value was derived from such cross-section measurements made by a group at Oak Ridge in 1954.[38] The plot of S-factor versus energy obtained from the Oak Ridge data is shown in Fig. 4.2. It can be seen that the cross-section seems to vary linearly with energy before tailing up at about 1100 keV-barns. S_{33} is obtained

Fig. 4.2   Cross-Section Factor S_{33}

Data of Good, Kunz and Moak.

Figure reproduced from Kavanagh (1972).

CENTRE-OF-MASS ENERGY (KeV)
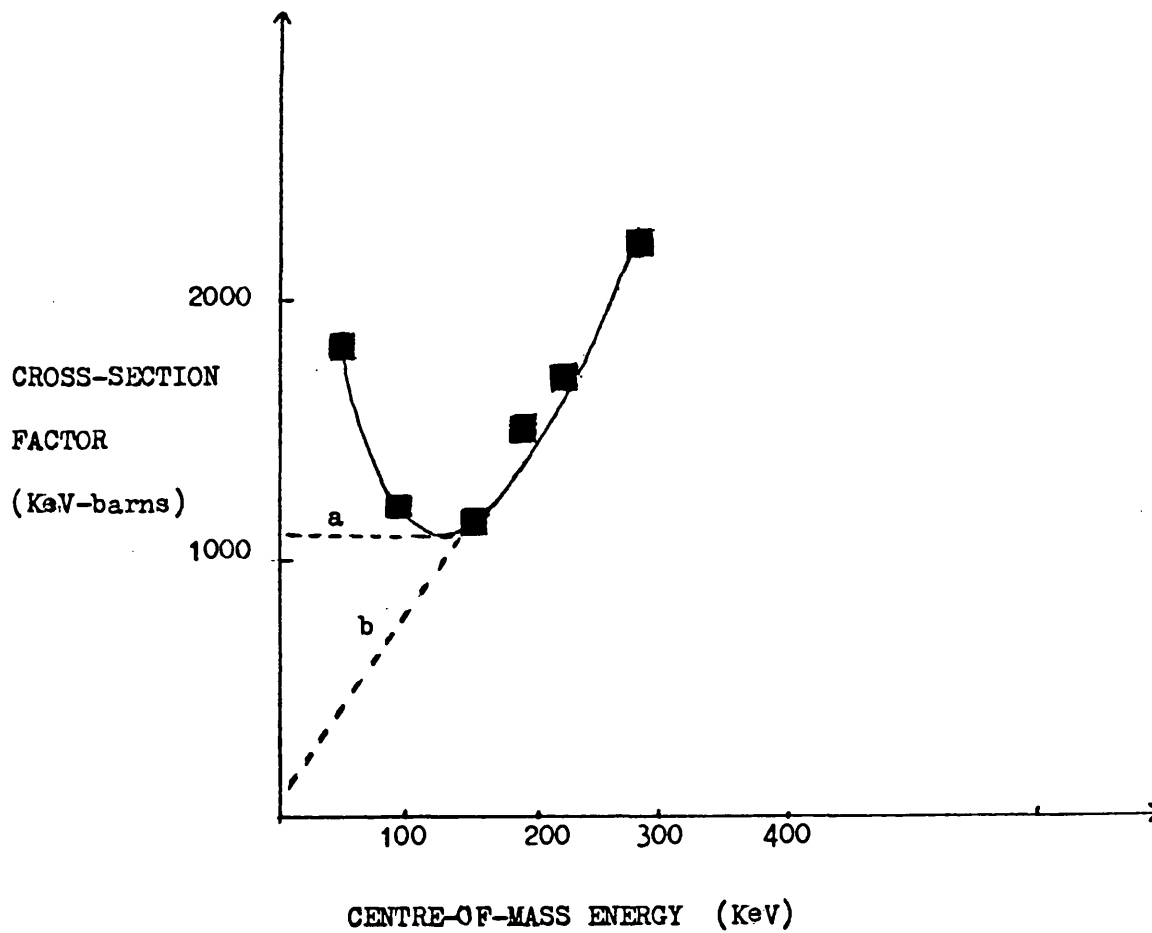
from this plot by extrapolating down to zero energy (i.e., the point

where the S-factor axis is intersected - known as the zero-energy-

intercept). It seems that Fowler's value for S_{33}, 1100 keV-barns,

was obtained by drawing a horizontal line across to the S-factor

axis from the lowest point in the S-factor curve (dotted line 'a'

in Figure 4.2). When Parker re-examined the data in 1963 it became

clear that another extrapolation was possible. This is the con-

tinuation of the downward trend in the data until it intercepts

the S-factor axis at about 200 keV-barns (dotted line 'b' in

Figure 4.2). With the possibility of this alternative extrapolation,

Parker, Bahcall and Fowler wrote in their review article that:

> The correct value...may be as much as a factor of 5 or even
> 10 different from the value quoted above [1100 keV-barns].
> (Parker, Bahcall and Fowler, 1964: 615).

Although they noted that the available data was also 'not inconsistent'

with an intercept of 1100 keV-barns, they felt that this value 'may

have been determined by the limitations of the Oak Ridge experiment'

(Ibid: 615).   In other words, it was felt that the upward

trend of the last two points (those at lowest energy) could be

caused by experimental errors (such as beam straggling) which

were known to be more severe at lower energies.   In any case,

Parker et al.urged that further experimental measurements be

attempted.   With this in mind, Kellogg nuclear physicists Bacher

and Tombrello eventually commenced work on this reaction in an

attempt to get better low-energy cross-section measurements (Bacher's

and Tombrello's results will be discussed in Chapter 6.)

The significance of all this for the neutrino-flux predictions

can be seen from Figure 2.2. A lower value for the cross-section

for the $He^3 + He^3$ reaction would mean that the competing $He^3 + He^4$ reaction would take place more often and hence would lead to more beryllium-seven and boron-eight neutrinos being produced. Thus Bahcall, as we saw above, was not greatly worried by the uncertainty in the extrapolation, as the uncertainty suggested an increased signal.

As Peter Parker, who shared Bahcall's view that the cross-section would probably be lower, told me:

> Whenever you do scientific measurements I think to some extent they are coloured by our own prejudices and what you'd like kinda to see happening and so on. I think to some extent in those days looking at solar neutrinos was something we were really looking forward to and wouldn't it be nice if the He^3-He^3 cross-section was lower, then we will have a much more predominant neutrino flux. And I think to some extent that one probably expected that was what was going to happen. And to some extent it's also true that if you look at the cross-section factor it makes a downward trend and then it turns up. So one would suspect that the turning up is probably suspect, because they're going to lower energies and therefore your measurements are really getting pretty erroneous at that point. And maybe really the trend is downward rather than upward and therefore one would expect this [S_{33}] to go down.

To check the exact effect of a lower value of S_{33} on the neutrino flux, Sears (1964), at the urging of Bahcall, computed a solar model with S_{33} = 200 keV-barns. He discovered that, with this value for the cross-section, larger fluxes were indeed predicted ($\phi_B 8$ = 4.5 x 10^7 neutrinos $cm^{-2} sec^{-1}$ and $\phi_{Be} 7$ = 1.7 x 10^{10} neutrinos $cm^{-2} sec^{-1}$).

Other Work on the Theoretical Prediction - Pochoda and Reeves

In addition to the efforts of the Caltech group, two other physicists produced a detailed calculation of the expected neutrino fluxes at this time.This work was carried out by a Princeton stellar-model specialist, P. Pochoda, and a nuclear physicist at the University of Montreal, H. Reeves. As we saw in Chapter 3, Reeves

was keeping a close eye on developments in solar-neutrino detection,
and had been in contact with Davis. Pochoda and Reeves constructed
a solar model using all the latest input parameters. Their neutrino-
flux predictions ($\phi_B 8$ = 2.6 x 10^7 neutrinos cm^{-2}sec^{-1}; $\phi_{Be}7$ =
1.2 x 10^{10} neutrinos cm^{-2}sec^{-1}) were in agreement with those of
the Caltech group to within 30%. This was, as Pochoda and Reeves
noted, 'well inside the model uncertainties' (Pochoda and Reeves,
1964: 120). They also attempted to make a more exact estimate
of the uncertainties in the prediction. However, this was a
difficult problem as it was not clear how all the various uncertainties
interacted with each other and back on the model. They wrote:

> It is very hard to estimate quantitatively (percentage-wise)
> the accuracy of these values. Although the uncertainties
> attached to the experimental parameters used to build stellar
> models are known to be rather small (usually less than 10 per
> cent), their effect on the models would be extremely difficult
> to estimate. The mathematical framework is indeed far too
> complex.(Pochoda and Reeves, 1964: 119).

Thus they were not able to make a precise quantitative estimate of
the error. Indeed, the detailed treatment of the various uncertainties
in the theoretical model is still a source of contention even today
(see Pinch, 1980a in Appendix II).

One of the areas of uncertainty which Pochoda and Reeves
looked at was the error in the nuclear-reaction rates; they noted
in passing that:

> The energy generation rates are known to about 10 per cent,
> but the (Be^7,p) rate (negligible for energy generation but
> important for neutrino fluxes) is known to be only 50 per
> cent. Much larger uncertainties come from the model itself.
> (Pochoda and Reeves,1964:125).

It will be recalled that the Be^7+p reaction cross-section had
been measured by Kavanagh in 1958. He had made measurements at
only two energies and hence the extrapolation to solar energies for
this reaction was rather uncertain.

Despite the uncertainty in S_{17} and the earlier noted uncertainty

in S_{33} the reasonable agreement between the Pochoda and Reeves

calculations and those of the Caltech group gave grounds for

confidence that the nuclear astrophysicists could predict the

solar-neutrino flux and that the uncertainties in the prediction

were not sufficient to prejudice the viability of Davis's proposed

experiment.

The Importance of the Analogue State

The next important step in the prediction of the expected

neutrino signal concerned the physics of the detector rather than

the Sun. Bahcall, as part of his research into the uncertainties

in the prediction, had made detailed calculations of the chlorine

thirty-seven cross-section for various neutrino fluxes. He presented

his results at a seminar at the Niels Bohr Institute in Copenhagen

(where he was spending part of the summer of 1963). At this seminar,

Nobel Laureate ,Ben Motelson ,pointed out that there was the possi-

bility of an enhanced cross-section for the energetic boron-eight

neutrinos because of 'excited state' transitions in the chlorine

thirty-seven-argon thirty-seven system. In particular, there was

an excited state of argon thirty-seven known as the 'analogue

state' which would play a dominant part. Transitions are part-

icularly likely to go to this state because it is a similar nuclear

state to the ground state of chlorine thirty-seven. The much

higher probability that this state would be excited greatly

enhanced the cross-section (by a factor of approximately twenty).

The importance of this breakthrough in terms of the funding of the

experiment was discussed in Chapter 3.

Bahcall's calculations, as presented to the conference on

'Stellar Evolution' held in New York in November 1963 (Bahcall and

Davis, 1966) and as given in his paper in Physical Review Letters ,

in March 1964 (Bahcall, 1964a) ,indicated a substantial detection

rate for Davis's experiment (40 ± 20 SNU).[39]   As we saw in

Chapter 3, it was with an expected detection rate of this magnitude

that the solar-neutrino experiment was funded in July 1964.   Thus

it seemed in July 1964 that the solar-neutrino flux could be pre-

dicted with confidence and furthermore that the signal would be of

sufficient magnitude to warrant an experiment. The confidence felt

by the nuclear astrophysicists is reflected in Fowler's letter of

support which, as we saw in Chapter 3, was important in convincing

the AEC to fund the experiment. Fowler wrote:[40]

> ...All of the solar reactions suggested theoretically have
> now been studied extensively in the laboratory, directly or
> indirectly, but at energies considerably greater than the
> effective energy of the sun. Extrapolation to solar energy ·
> involves very large reduction factors which are thought to
> be given quite accurately by current theories of nuclear barrier
> penetration. Detailed analysis of resonance effects is required
> in some cases. The position has been reached where little
> more can be done in the study of the nuclear reaction rates
> either theoretically or experimentally...With our present
> knowledge the neutrino flux at earth can be precisely
> predicted and it will not be a coincidence if the predicted
> value is shown to be correct.   In that case it will be possible
> to proceed with great confidence in further thermonuclear
> and astrophysical researches.   On the other hand if a dis-
> crepancy is established it will be necessary to review the basic
> ideas as well as the detailed calculations involved in the
> relevant nuclear and atomic physics.   The ramifications, in
> this case, are endless and I need not belabor the point.. (My
> emphasis) .

Bahcall's 1964 Prediction - its Relationship to Sears's Predictions

In view of the importance of Bahcall's prediction for the develop-

ment of the experiment it is of interest to ascertain in more detail

how he obtained his result. In Bahcall's 1964 paper, the boron-

eight flux ($\phi_B 8$) is given as (2.5 ± 1) x 10^7 neutrinos $cm^{-2} sec^{-1}$

and the beryllium-seven flux ($\phi_{Be}7$) as $(1.2 \pm 0.5) \times 10^{10}$ neutrinos $cm^{-2}sec^{-1}$. Sears's work is cited as the source for these particular values. Unfortunately, however, there is no indication of how, in detail, these fluxes were derived. A closer examination of Sears's work throws light on this issue.

It will be recalled that Sears (1964) computed a variety of solar models (ten in all, labelled A-J) with different choices of input parameters and derived neutrino-flux predictions for each of them. Sears felt that one model in particular (model J) was the best one. That is all the parameters were set at their optimum for this model. As he wrote in conclusion to the published account of his work:

> We regard Model J as being most nearly consistent with the available data for the Sun at the present time. This model has been used as a basis for theoretical calculations (Bahcall, 1964 a,b) on the solar-neutrino detection experiment currently being undertaken by Davis. (Sears, 1964: 482).

The main feature of Model J was that it had a lower value of Z (the heavy-element content of the Sun) than other models. Sears preferred this value because it matched the most recent rocket data on the composition of the Sun.

In view of the above statement by Sears concerning the relationship between his work and that of Bahcall,and Bahcall's own citation of Sears's work, it would be reasonable to expect the neutrino fluxes predicted by Model J to match those used by Bahcall. However, if the fluxes predicted by Model J are examined, they are found to be somewhat lower than those given by Bahcall - $\phi_B8 =$ 1.9×10^7 neutrinos $cm^{-2}sec^{-1}$ and $\phi_{Be}7 = 0.82 \times 10^{10}$ neutrinos $cm^{-2}sec^{-1}$ for Model J, compared with $\phi_B8 = 2.5 \times 10^7$ neutrinos $cm^{-2}sec^{-1}$ and $\phi_{Be}7 = 1.2 \times 10^{10}$ neutrinos $cm^{-2}sec^{-1}$ given by Bahcall. On the

basis of the fluxes for Model J, a detection rate of 30 SNU

is obtained as opposed to Bahcall's preferred rate of 40 SNU .

My attention was first drawn to this discrepancy by Sears himself.

He told me:

> If you want a bit of history, Bahcall had taken my paper
> and had not taken my best model but an average over several
> models. My thinking was that I have this very best model [J]..
> John's point of view was perhaps 'Well there is a variety
> of uncertainties in this game and Sears has done all this
> great work in showing all these 6 or 8 different possibilities
> and so why not take some average as being a representative
> number?' In other words, he didn't have the faith...that I
> did that my model was absolutely the best. I did because I
> had worked so hard on this thing and I really had the ultimate
> result. But it may also be that John wanted to predict
> as large a number as possible in order to get a sufficiently
> high flux for the AEC to support the whole business, but I
> don't know.

This last intriguing possibility volunteered by Sears, that Bahcall

may have hoped to make the flux as large as possible in order to

make the experiment look as attractive as possible will be discussed

below, and in some detail in Chapter 10. For the moment, the key

point of interest is that it seems that there is some flexibility

in the prediction and that different predictions can be got by

making different assumptions.

It does seem probable that Bahcall took an average over all

Sears's models. The limits of uncertainty Bahcall set on the pre-

diction (20 - 60 SNU) do indeed cover the range of fluxes predicted

by Sears. However, Bahcall's 'best' value, 40 SNU, corresponds

neither to Sears's 'best' model nor to a simple average of all the

models. In view of this it seems most likely that Bahcall derived

his 40 SNU by taking a weighted average.

I raised the discrepancy between the two predictions with

Professor Bahcall.[41]   Despite digging in his notes he was unable

to say what the exact relationship between the two predictions was.[42]

He agreed that he had certainly read Sears's paper and pointed out that the domain of values he had given corresponded to the range of values predicted by Sears. This of course does not explain the difference in the 'best values'. However, he also drew my attention to a later letter which Sears had written him which indicated their different viewpoints over how to derive a 'best value'. This letter lends support to the notion that Bahcall's 'best value' came from some sort of averaging procedure. In the piece of correspondence referred to, Sears is objecting to Bahcall's predicted boron-eight flux (this is a later prediction); he writes:[43]

> If you want the 'best' value of something, either (i) you
> ask an expert and he gives you the latest result, duly biased;
> or (ii) you decide to be impartial and survey the available
> numbers and brutally take an average. I personally preach
> the former procedure, arguing that the 'available numbers'
> are not stochastically related.

In other words, Sears claims it is spurious to take a statistical average over models which differ in the choice of input parameters. After all, the different choices made by the model expert may not represent a stochastic distribution, On the other hand, the pre-ferred model may also be distorted and represent the idiosyncratic choice of the expert. It would seem that both procedures are 'reasonable' but they do entail differing neutrino-flux predictions.[44]

Bahcall's 'best' value, as it is larger than the flux for Model J, must have been derived by considering models which produced larger fluxes than Sears's optimum model. Bahcall's preference for a model giving a larger flux may have been related to his views on the best value for the He^3-He^3 cross-section. It will be recalled from the discussion earlier in this chapter that Bahcall felt that S_{33} had been over-estimated in the 'standard' model (i.e. J), and hence he had got Sears to compute one model using a lower value for

S_{33}. As was noted above, this model (E) gave a larger flux. There

is one piece of correspondence (between Bahcall and Davis) which

suggests that this particular model may have influenced Bahcall's

thinking. Bahcall wrote:[45]

> I have always argued that Dick's preferred model gives too
> low a B^8 flux because he uses the standard S_0 for He^3-He^3
> [i.e. S_{33}] that WAF [Fowler] used to quote. When Pete [Parker]
> and I re-examined the He^3-He^3 data we were convinced that the S_0
> measurements were unreliable and the S_0 for He^3-He^3 could
> really be much lower...You will see the evidence of my urging
> in Dick's model E.

From this it would seem possible that Bahcall's views on the He^3-He^3

cross-section led him to place less weight on model J (which Sears

preferred) and more on Model E which happened to predict a larger

flux. Hence his overall prediction was larger than that favoured

by Sears.

Discussion of Sears's Prediction vis-à-vis Bahcall's Prediction

This account of the difference between Bahcall's preferred best

value for the neutrino flux and Sears's best value illustrates

the flexibility possible in neutrino-flux predictions. Both Bahcall

and Sears, for different reasons, arrived at different best values.

It would seem that, if he had so wished, Bahcall could 'reasonably'

have arrived at a lower prediction than he did.

Bahcall, himself, has pointed out the rather arbitrary nature

of the 'best' value. In a paper published in Physical Review which

was a sequel to his 1964 Physical Review Letters paper, he wrote:

> The choice of 'best' values for the fluxes presented...is
> somewhat arbitrary since the predicted fluxes depend upon
> nuclear and solar parameters that are imperfectly known.
> (Bahcall, 1964 b: B138).

Nevertheless, despite the arbitrary nature of the best value it

was this value which formed the basis for the funding of Davis's

experiment. Of course, as mentioned in Chapter 3, Davis's experiment

was designed with a possible error in this theoretical value in mind.

However, the whole project must have looked more compelling to the

funding agency the larger the signal-to-noise ratio that Davis could

expect.[46]

As was mentioned in Chapter 3, it was the dramatic increase in

the prediction in 1964 which was so crucial in convincing people

that the experiment should be funded. For instance, Fowler has

written in reference to Bahcall's work on the analogue state:

> Bahcall showed that the over-all detection cross-section was
> increased by a factor of 17. It was this more than any-
> thing else which convinced the powers that be to give Davis
> the go-ahead for the construction of his neutrino observatory.
> (Fowler, 1969: 365).

The importance of a large theoretical prediction (as well as a feasible

experimental procedure with low backgrounds) has also been stressed

publicly by Goldhaber (whose support, as we saw in Chapter 3, was

crucial). Goldhaber said:

> I used to resist this experiment when the predictions were
> ten times smaller and the background ten times larger than
> now so the effort did not seem worthwhile. But the more I
> resisted the more the theoretical value went up. (Goldhaber,
> 1967: 482).

Goldhaber also added the intriguing remark:

> I have often suspected that the theory overshot a little
> (Ibid)

When I interviewed Goldhaber, I asked him to elaborate on this last

remark. He told me:

> I thought they had a bit over-sold it....
> You see the first prediction was higher than the later one,
> and you can make the general remark independent of this
> particular case that usually when theorists would like to see
> their theories tested they are overenthusiastic and each factor
> is in favour of predicting a positive effect....

Similar views concerning the nature of theoretical predictions in

general and this one in particular were reiterated to me by several

respondents.   Certainly it can be said that Bahcall was highly

motivated to get support for Davis's experiment. As we saw in

the previous chapter, he and Fowler both played an active role

in seeking funds. As one scientist who had been at Caltech told me:

> I would say the way I remember the history, it was John
> Bahcall in particular who made propaganda for doing the
> experiment before Ray Davis started collecting the money
> to actually do it...I thought it was more the theorists
> pushing the experiment..you know rather than the theorists
> calculating things after the experiment has started.

Also, as Bahcall himself told me:

> 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 was so enthusiastic about it that I
> felt that as I continued to do work I wanted to sell the
> experiment; that is to get funding for it so it would happen.

In view of Bahcall's and Fowler's direct involvement with the

funding process ,it is evident that Bahcall was well aware of the con-

sequences that would follow from a significant increase in the

theoretical prediction. The analogue-state discovery provided such

a dramatic increase. However, as Bahcall finalised his calculations

in order to predict the most likely signal (the 'best' value), we

have seen that he averaged Sears's predictions and also interpreted

the S_{33} data in a way which may have led to an enhanced prediction.

The interpretation of S_{33} was shared by Parker who explicitly drew

attention to the 'general hopes at the time for a large flux' (see

above p. 140). As we shall see in Chapter 6, other interpretations

of the S_{33} cross-section data are possible. Although I would claim

that enough detailed documentary evidence has been brought forward

in this chapter to indicate a connection between funding considerations

and the 'best' value of the theoretical prediction,

care must be taken in the evaluation of the above argument.

Firstly,the argument is necessarily of the counter-factual type

and hence must to a degree remain hypothetical. For instance, we

have no way of telling that a smaller predicted flux would not also

have led to the funding of the experiment. A second qualification

concerns the interpretation of the evidence and, in particular,

the interview data. This is not straightforward since the interviews

were conducted in 1978 when it was possible with hindsight to view

Bahcall's 1964 prediction with suspicion. After all, by 1978,

his best value had fallen from 40 SNU to 4.7 SNU (see Fig. 1.1).

In such circumstances respondents might have been eager to find a

social 'bias' to account for the 1964 prediction being exaggerated.

Such a 'bias' would be his 'motivation' to get the experiment funded.

These reservations will receive more attention in Chapter 10.

Finally it should be noted that it is not being suggested that

Bahcall as an individual was in any way peculiar in his search for

ways of predicting large fluxes. As the quote from Parker (above, p. 140)

makes clear, there was a widespread feeling at the time that S_{33}

would be revised downwards. The sociological rather than psycho-

logical character of the explanation being offered will also become

more apparent in Chapter 10.

NOTES FOR CHAPTER FOUR

1. The development of a detailed solar model over this period was facilitated by the introduction of more powerful (and hence faster) computers. In 1956 it was estimated that a few hours of computer time would be needed to run a stellar-evolution programme (see C.B. Heselgrove and F. Hoyle, 'A Mathematical Discussion of the Problem of Stellar Evolution with Reference to the Use of an Automatic Digital Computer', Monthly Notices of the Royal Astronomical Society, 116, 1956, 515-26). By 1964 stellar-evolution programmes took only ten minutes to run on the latest IBM machine (see Sears, 1964, for example). Today they take only a few seconds on a CRAY machine.

2. Since the theoretical calculations made in the solar-neutrino field were mostly carried out on computers (rather than envelopes) artefacts such as programmes and print-outs are more likely to be available for historical analysis. Making sense of such material, even if it were available (most old programmes are discarded), would, however, be a daunting task. Even if the historian was competent in computer science, had access to a machine, and was prepared to re-run the programmes, he/she would probably not get anywhere. In view of the complexity of such programmes and their idiosyncratic character, usually only the person who wrote them has any chance of making them work. For instance, in 1966, when it became imperative to check over a programme run at Caltech in 1964, the only way it could be done was to try and persuade the person whose programme it was to come back to Caltech and re-run the programme. (This incident is discussed in a letter, R. Sears to John Bahcall and John Faulkner, May 18, 1965).

3. The locus classicus on the veracity of scientific papers is P. Medawar, 'Is the Scientific Paper a Fraud?' The Listener, 12th September 1963, 377-8.

4. Davis's experiment is sensitive to neutrinos with energies greater than 0.814 MeV. The boron-eight neutrinos have a spread of energies with a maximum energy of 14.1 MeV and an average of 7.3 MeV.

5. R.F. Christy and I. Duck, 'γ Rays from an Extranuclear Direct Capture Process', Nuclear Physics, 24, 1961, 89-101.

6. These values are to be found in: letter, R. Davis to W. Fowler January 15, 1958, and letter, R. Davis to A. Cameron, February 4, 1958.

7. Letter, W. Fowler to R. Davis, January 20, 1958.

8. R.W. Kavanagh, '$Be^7(p,\gamma)B^8$ and $Be^7(d,p)Be^8$ Cross-Section Measurements', Bulletin of the American Physical Society, 4, 1958, 444.

9. This value is quoted by R.W. Kavanagh, 'Proton Capture in Be^7', Nuclear Physics, 15, 1960, 411-20, and is given by Fowler in correspondence (letter, W. Fowler to F. Reines, January 13, 1960).

10. The lithium-four neutrinos would have maximum energy of 18.9 MeV and an average energy of 9.4 MeV. Fowler estimated a flux $\phi_{Li}4 = 4 \times 10^{10}$ neutrinos $cm^{-2}sec^{-1}$ (letter, W. Fowler to F. Reines, op. cit., note 9).

11. These arguments rested in part on the relative stability of He^4 (the 'rival' candidate for formation) and the known instability of Li^5.

12. Letter, W. Fowler to F. Reines, op. cit., note 9.

13. S. Bashkin, R.W. Kavanagh and P.D. Parker, 'Search for Li^4', Physical Review Letters, 3, 1959, 518-20.

14. R.L. Sears, 'An Evolutionary Sequence of Solar Models with Revised Nuclear Reaction Rates', Mémoires de la Societé Royale Sciences de Liège,3, 1960, 479-489.

15. Earlier models had assumed that the pp-chain terminated mainly through the $He^3 + He^3$ reaction (see Fig. 2.2).

16. Letter, R. Davis to J. Bahcall, February 19, 1962.

17. Letter, R. Davis to J. Bahcall, op. cit., note 16.

18. J.N. Bahcall, 'Beta Decay in Stellar Interiors', Physical Review, 126, 1962, 1143-9.

19. J. N. Bahcall, 'Electron Capture and Nuclear Matrix Elements of Be^7', Physical Review, 128, 1962, 1297-1301.

20. P.D. Parker and R.W. Kavanagh, '$He^3(\alpha,\gamma) Be^7$ Reaction', Physical Review, 131, 1963, 2578-82.

21. Letter, W. Fowler to R. Davis, October 17, 1962.

22. Ibid.

23. Letter, R. Davis to W. Fowler, November 16, 1962.

24. The beryllium-seven neutrinos are monoenergetic (0.816 MeV) and are just above Davis's detector threshhold (0.814 MeV). It should be noted that, although the beryllium-seven flux is much larger than the boron-eight flux, its contribution to Davis's experiment is not similarly greater because the capture cross-section for beryllium-seven neutrinos is much lower due to their small energy.

25. Letter, R. Davis to W. Fowler, November 16, 1962.

26. Letter, J. Bahcall to R. Davis, November 20, 1962.

27. Bahcall, it seems, used nuclear values for a key parameter rather than atomic values.

28. Letter, J. Bahcall to R. Davis, November 29, 1962.

29. Letter, R. Davis to J. Bahcall, December 20, 1962.

30. Letter, J. Bahcall to R. Davis, January 3, 1963.

31. It appears that a theoretical result is 'crafted' in much
the same way as an experimental result. The theoretician
has to use his expertise to decide which formalism to use,
which approximations can be legitimately made, and which
standard parameters to use. Such a complex activity is as
open to 'error' as the production of a result via a complex
experiment.

32. I would not want to suggest that these 'errors' are funda-
mentally any different from other allegations of theoretical
or experimental incompetence. It is just that consensus over
these routine parts of the calculation is so widespread that
it would be almost unthinkable for a scientist to refuse to
acknowledge such mistakes. In principle, Bahcall could have
made an argument for using nuclear rather than atomic parameters -
perhaps it made no difference in this case—but in practice
no-one wants to fight that sort of battle. Similarly we can
imagine that an experimentalist might, in some circumstances,
argue that his failure to switch on some part of his apparatus
was not important to his result and was a perfectly legitimate
procedure.

33. It is, however, amusing that Davis, the experimentalist, pointed .
out the beryllium-seven possibility which the theoreticians
had overlooked, and that he got one calculation 'right' which
Bahcall got 'wrong'. I was also told of another amusing case
concerning the proposed gallium detector (see Chapter 7).
Bahcall had made a mistake in calculating the amount of gallium
needed and this was only discovered when an experimenter made
the same calculation just to see if he could do that sort of
thing. Bahcall, who at first refused to believe he had made
a mistake, was, needless to say, rather embarrassed by this
incident, especially as it meant that the cost of the detector
doubled in price since twice as much gallium would be needed!

34. This error was not mentioned by any other respondent and there
has never been any published retraction of the FIBS result.
A lower-aged model would tend to predict smaller fluxes than
the fully-evolved model.

35. Similarly, many experimental results in science are never
replicated. For a discussion of the role of experimental
replication, see Collins (1975, 1976) and Travis (1981).

36. In addition to the beryllium-seven and boron-eight neutrino
fluxes tabulated in Fig. 4.1, there is a very small signal
expected from the reaction $p + p \rightarrow H^2 + e^+ + \nu$, and from the
CNO reactions $N^{13} \rightarrow C^{13} + e^+ + \nu$ and $O^{15} \rightarrow N^{15} + e^+ + \nu$.
Since the total signal from these reactions is an order of
magnitude less than from the beryllium-seven and boron-eight

36. contd.
    contributions they are usually ignored for the purposes of
    Davis's experiment. Sears (1964, 1966) and Pochoda and Reeves
    (1964) did, however, for completeness, calculate the exact
    effect of these smaller contributions.

37. Letter, J. Bahcall to R. Davis, August 23, 1963.

38. W.M. Good, W.E. Kunz and C.D. Moak, 'The He^3 + He^3 Reactions',
    Physical Review, 94, 1954, 87-91.

39. Pochoda and Reeves (1964) also added a note in proof to their
    paper where they took some initial calculations of the
    effect of the analogue state and combined it with their flux
    predictions. They estimated a total signal also of $\sim$40 SNU.

40. Letter, W. Fowler to R. Dodson, July 31, 1964.

41. Letter, T. Pinch to J. Bahcall, September 19, 1979.

42. Letters, J. Bahcall to T. Pinch, January 16, 1980, May 15, 1980.

43. Letter, R. Sears to J. Bahcall, June 27, 1966.

44. Another possible explanation of the difference between Bahcall's
    'best' value and Sears's 'best' value has been pointed out to
    me by Professor Bahcall (letter, J. Bahcall to T. Pinch, June
    5, 1980). I was referred to a passage in a later paper by
    Bahcall in which the 1964 calculation was discussed. This
    passage, published in 1971, reads:

> It is also of interest to note that Bahcall (1964),
> prior to the completion of Sears's model calculations,
> had estimated a $\Sigma^{37}Cl$ ($\phi\sigma$) = 40 ± 20 SNU by using the
> then newly derived neutrino absorption cross-sections
> and preliminary neutrino fluxes calculated by Sears.
> (Bahcall and Ulrich, 1971: 598). (My emphasis).

The implication of this passage is that the neutrino fluxes
published by Sears may have been different from earlier values
which Sears communicated informally to Bahcall. In which case
it was these earlier values which Bahcall used.

  This explanation seems unsatisfactory to me as Bahcall's
1964a paper was written after Sears had given his initial
results to the Stellar Evolution Conference held in New York
in November 1963 (referred to above and in Chapter 3). I have
a preprint of Sears's presentation there and his results then
were the same as in the published versions (Sears, 1964, 1966).
It seems unlikely that Bahcall would not have used Sears's
most up-to-date model predictions for his 1964a paper. In
addition, Sears himself has drawn my attention to the statement
he and Bahcall made concerning this episode in their joint
review of the field published in 1972. They wrote:

> Bahcall...adopted an average over several of Sears's
> [1964] models that gave $\phi_B 8$ = 2.5 x 10^7 ....(Bahcall and
> Sears, 1972: 33).

Sears (interview material) pointed out to me that Bahcall had
raised no objections to this description of events, a description

44. contd..
    which implies that it was the published values of Sears' models
    which were used.

45. Letter, J. Bahcall to R. Davis, May 11, 1965.

46. Of course, Bahcall's prediction had a formal error bar attached
    to it (± 20 SNU). It could be said that it was the size of
    this error bar, rather than the 'best value' which was the key
    parameter in considering whether the experiment was worth
    funding. However, all the emphasis at the time was placed on
    knowing the 'best prediction' rather than the error bar.
    The best prediction was the number that was mentioned in the
    correspondence at the time. No-one said 'Look the error bar
    on the theoretical prediction is ± 20 SNU, this means that
    the theorists don't really know what they are talking about'.
    Indeed the emphasis at the time (such as in Fowler's letter
    to the AEC - op. cit. note 40) was on the fact that the flux
    could be precisely predicted. Also Bahcall had informed Davis
    in August 1963 (op. cit., note 37) that the uncertainties
    could not lower the flux by more than 40% (this estimate was,
    however, for the 1963 rather than 1964 prediction).

CHAPTER FIVE

EXPERIMENTAL DEVELOPMENTS IN SOLAR-NEUTRINO ASTRONOMY 1964-1967

In this chapter the description of experimental developments is continued until the point in August 1967 when Davis was ready to make his first measurement. Although much of the detailed material presented deals with the Davis experiment, other, less-ambitious solar-neutrino detection projects, that were undertaken over this period, are also described. As in the previous two chapters the account is largely chronological and descriptive.

In Chapters 3 and 4 the partnership between the Caltech group of nuclear astrophysicists headed by Fowler and the Brookhaven group centred around Davis was outlined. As we saw, it was in the interest of both groups to get a solar-neutrino detection experiment performed, and, in 1964, after a detailed prediction of the expected signal was made, the funding for such an experiment was procured. After 1964, the project moved to the experimental-construction phase, and Fowler became less directly involved. He still kept a close eye on developments but he was happy to let the Caltech end of the partnership fall upon the shoulders of John Bahcall. One of the main developments over the period dealt with in this chapter is the increasingly close working relationship which developed between Bahcall and Davis. This relationship will be described in Chapter 6 where the theoretical developments over the period will be outlined. However, it should be borne in mind that none of the experimental developments described in the present chapter occurred in isolation from the Caltech group. Davis constantly reported on his progress in letters to Bahcall and, as we shall see in the next chapter, Bahcall even visited the site of the experiment to inspect the rig for himself.

Negotiations with Mining Companies

Davis, in late 1964, having got the money to construct his apparatus, now set about organising the project in earnest. It will be recalled from Chapter 3 that he had settled upon the Sunshine Silver Mine, Idaho, as the most favourable location for his detector. Negotiations between Brookhaven and Sunshine were to be commenced in December 1964. Before a contract could be drawn up, the Brookhaven administration (in the shape of a Procurement Review Board) delineated all the technical, contractual and legal problems which the neutrino experiment might raise. The Review Board met at Brookhaven on October 16, 1964. The complex nature of the venture upon which Brookhaven was embarking can be seen by the range of topics discussed at this meeting. The memorandum in which the meeting was reported included the following headings:[1] Types of Contracts (three contracts were required - one with Sunshine for the excavation of the chamber; one with a tank fabricator for the construction and installation of the tank in the mine; and one with Sunshine for a long-term lease of the mine); Expenditure of Money (details of the budget breakdown for the forthcoming financial year); Tentative Schedule for the Tank Procurement (the likely stages of negotiation with the tank fabricators); Insurance (discussion of liability should there be a leak from the tank); Removal of Perchloroethylene (Sunshine were insisting that the perchloroethylene be removed at the termination of the experiment); and Mining Consultant (it was suggested that BNL employ an independent mining consultant to assess Sunshine's work on the project).[2]

A further meeting of the BNL Procurement Review Board to discuss progress on all these matters was held a month later.[3] The

details of the contracts with Sunshine had by this stage been settled

and copies sent to Sunshine's attorneys. A meeting of the two parties

was scheduled for early December. This was to be held at the mine

and it was hoped that this would be when the final contracts were

signed. It was also planned that the companies tendering bi is for

the construction of the tank should attend so that they could see

the likely technical problems at first hand. All in all, the necessary

administrative and legal apparatus which accompanied the construction

of the scientific apparatus seemed to be in place. Davis himself

was very optimistic about developments; as he wrote to Philip

Morrison at the time:[4]

> We are making progress on the large scale experiment...Next
> week we will make detailed plans with the Sunshine Mining Co.
> (Idaho) for the excavation and sign a contract for this work.
> They will provide a room 30 x 60 feet with a 30 foot arched
> ceiling to hold our tank...Hopefully they will have this room
> ready by May 1st, and then we will start fabricating the tank.
> We estimate the tank will be finished December 1965, and the
> entire apparatus ready for the first run in March 1966.

This optimism was, however, to be short-lived. The meeting with Sunshine

did not go well and it became clear that it would be impossible for

Brookhaven and Sunshine to reach a mutually satisfactory agreement.

Contributing to the impasse were factors such as rising costs,

insufficient electric power and BNL not being given clear title to

the area selected for the experiment. However, the real sticking

point was a contractual problem. Brookhaven, as a Government-

sponsored laboratory,was required by law to write a general non-

discrimination clause into all its contracts. This was designed

to prevent discrimination (on grounds such as race, colour or creed)

against any potential employees of the Government. As Davis (who

was somewhat embarrassed by this issue) told me:

> That means that...if you're black, and that was what they
> were worried about, and you wanted to come and work in the

mine you had a perfect right to...Idaho apparently has had
a reputation...for not being too kind to black people...So
they were kinda scared by that. And so BNL and the Government,
they said 'You can't leave that clause out, that's impossible'.
So the whole thing fell down really on that basis.

The wider backcloth of political and commercial realities within
which solar-neutrino experimenters worked can be seen from this episode.[4a]

Homestake Changes its Mind

Davis was very disappointed by this set-back because, having got

the money budgeted for that financial year, if he did not spend it

at once, he was in danger of losing it altogether. As there were

only two other feasible sites for the experiment (the Homestake Gold

Mine and the Anaconda Copper Mine) Davis had little choice but to

once more review these possible locations. Because of the technical

advantages presented by the Homestake site (as discussed in Chapter

3) he decided to try there first. In view of the urgency of the

situation, Munhofen (who, it will be recalled, was Dodson's admini-

strative assistant) made a special trip over Christmas 1964 to

Homestake.   The company agreed to reconsider their estimate of

the costs for excavating a suitable chamber and, on January 2, 1965,

Davis heard the good news that they were now enthusiastic about the

project.[5]   They estimated the costs to be $125,000 - in other

words a very reasonable amount. Furthermore, they had no objections

to the non-discrimination clause. Davis expressed their attitude

on this point as follows:

> We [Homestake] are not worried about blacks, we don't care
> who comes and works in our mine. If blacks want to come, fine,
> we'll give them a pick and shovel and they can go to work![6]

The reasons for the change of heart by the mining company are

not entirely clear. Davis had the impression that the decision to

support the project was not taken locally but at Homestake's head

office in San Fransisco, and therefore might have been open to

influence from more general commercial considerations. In particular

he thought there mayhave been two contributory factors. Firstly,

the involvement of the company with a scientific project might be

good for public relations. Mining companies in the US have, in

general, a rather tarnished image and this might be improved by

their involvement with public 'cultural-type' activities.[7]   The

second reason Davis gave was that Homestake might have been parti-

cularly sympathetic towards any project connected with the AEC.

This was because Homestake also owned a uranium mine, and the main

purchasers of uranium were, of course, the AEC!

The Construction of the Tank

Whatever the reasons, Davis found that, from that moment onwards,

Homestake went out of their way to be cooperative over the project.

On January 8   , a satisfactory contract was signed and construction

of the chamber began almost immediately. By May, 7,000 tons of rock

had been blasted out to form a chamber 30 feet wide, 60 feet long

and 32 feet high. The floor was lined with concrete and, to ensure

stability, the walls and ceiling were covered with chain-link fencing

(see photograph, Fig. 5.1).   In addition, two smaller chambers

to house the pumps and the control room were excavated.

By this stage the contract for the construction of the tank had

also been settled. The successful contractors, the Chicago Bridge and

Iron Company, started immediately to prefabricate the various

sections of the tank.   In view of the restricted access, all the

sections would have to be taken down into the mine separately and

welded underground in the chamber (see photograph, Fig. 5.2).

The underground-construction work started in August 1965 and was

Fig. 5.1 The Excavated Chamber at Homestake,

Fig. 5.2 The Tank (partially constructed)   162.

Fig. 5.2 The Tank (partially constructed)
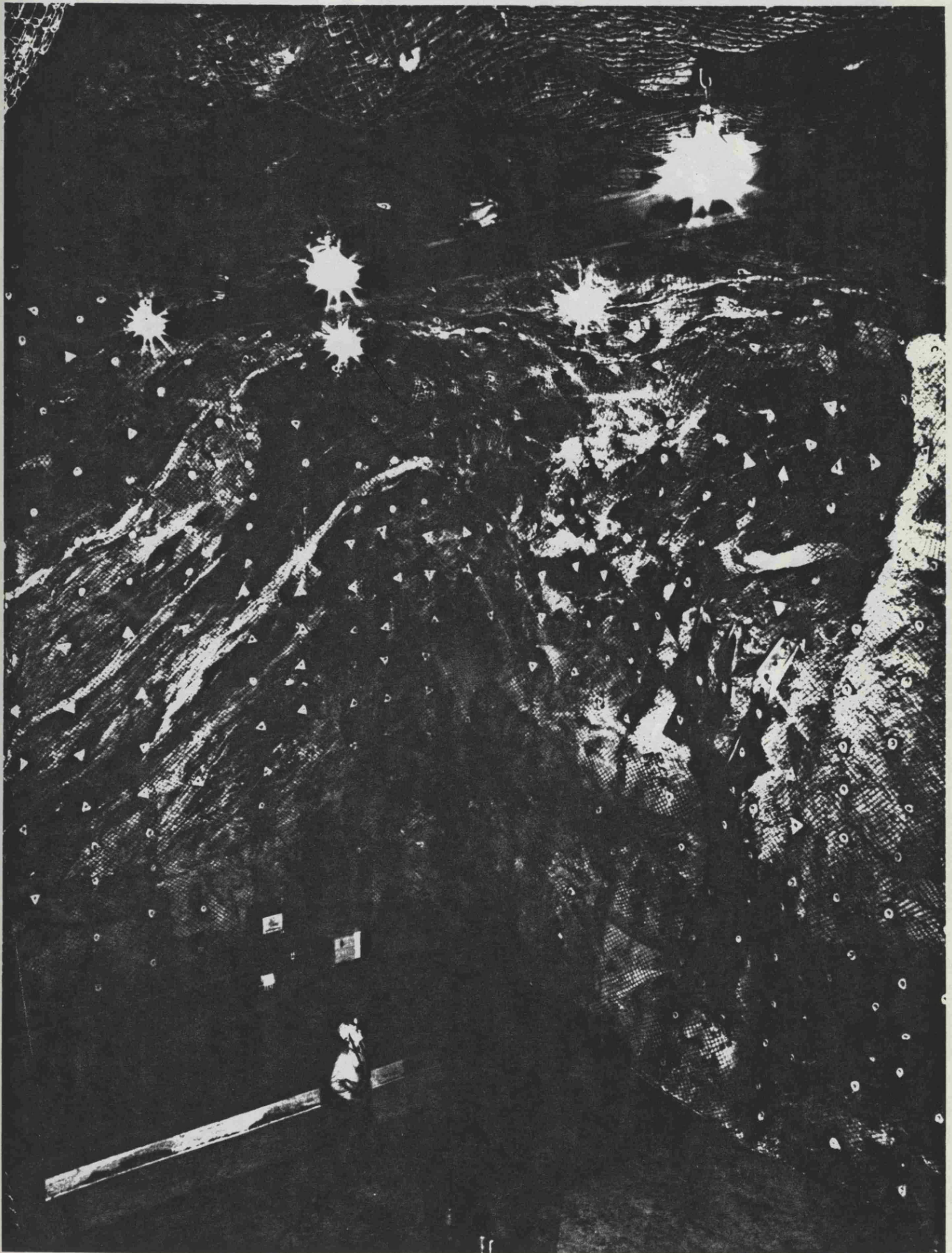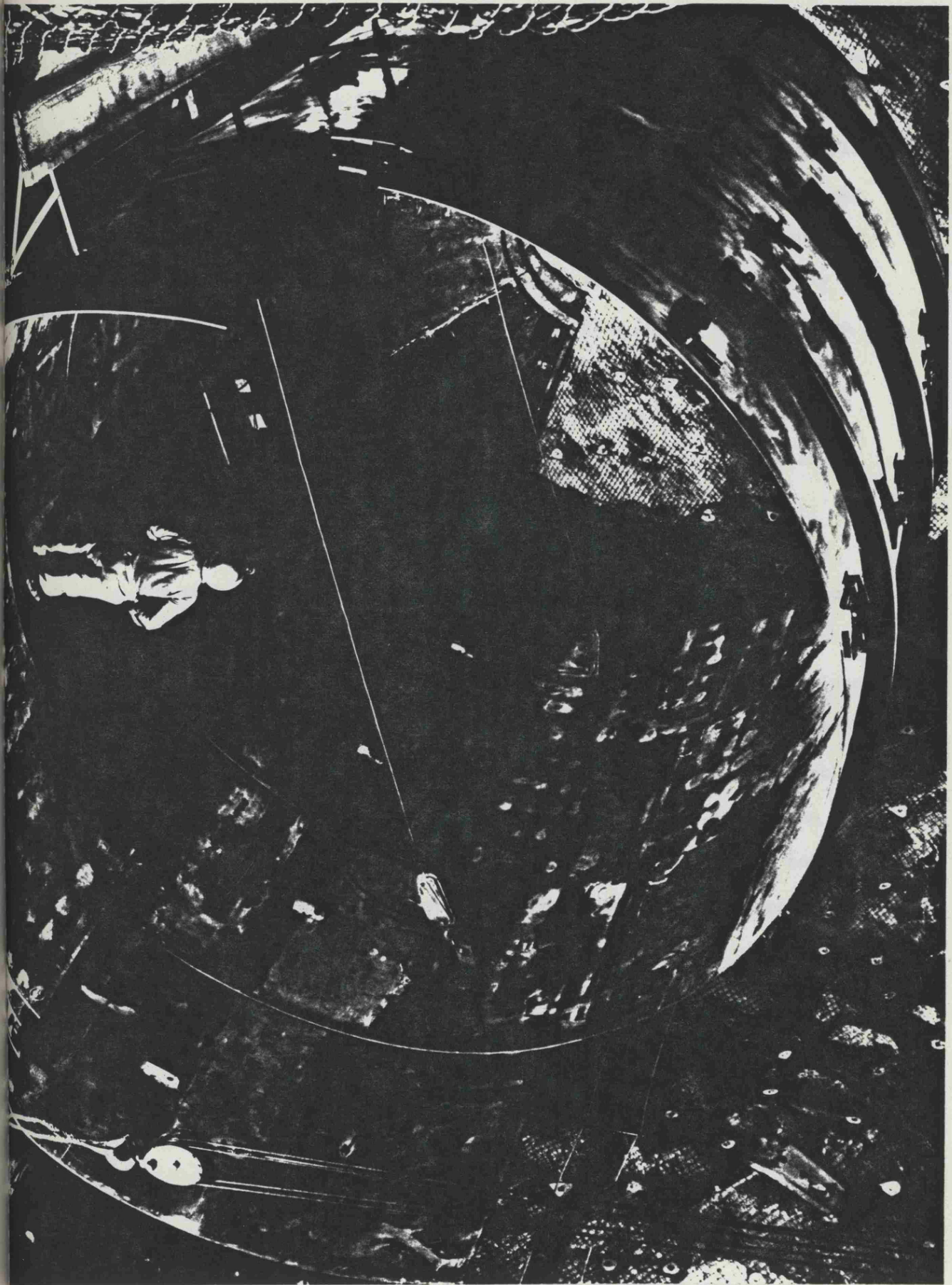
expected to be completed by December in order that the first measure-

ments could be made in early 1966.

By October 1965 the construction of the tank was nearly completed

and tests were begun to see if there were any leaks. As atmospheric

argon could contaminate the tank it was vital that the tank should

be air-tight. However, the plan to start the experiment in early

1966 had to be shelved when there was a delay in the supply of the

special pumps needed to purge the helium and argon from the tank.

These pumps were not expected to be ready until February 1966,

so reluctantly Davis, in the winter of 1965, had to postpone any

further work on the tank until March.

By the middle of June 1966 the pumps had been installed and

the tank was ready to be filled. By this stage, the inside surface

of the tank had been shot blasted and cleaned in order to remove

any traces of rock dust, which might contain radioactive contaminants.

Also the tests for leaks had been completed. Before the tank was

finally sealed, AEC-official John Pomeroy (Van Dyken's assistant)

visited the experiment to inspect progress.   In his memorandum

to Van Dyken in which he reported on the experiment, Pomeroy noted

one unusual difficulty which might affect the long-term prospects

for the experiment. He wrote: [8]

> It should be noted that the workers at Homestake have just
> become unionised for the first time in their history. The
> long-term effect of this on the experiments cannot be
> anticipated at this time. I am told that the mine is not a
> highly profitable operation, since the ore now being used
> runs $10 of gold/ton. Higher labour costs could possibly
> cause the mine operation to be shut down before all the
> experiments are completed.

Pomeroy included in his memorandum a copy of a story from the

Washington Post in which the unionisation of the mine is reported. [9]

In order to show something of the flavour of the Homestake operation

at this time part of this story is reproduced in Fig. 5.3.

It seems that the worry was that the union involved had a reputation

for militancy and they might push labour rates too high to make

the mine profitable.   Homestake could not raise the price of gold

to cover the increased costs because they were required to sell

all their gold to the Government at a fixed rate of $35 an ounce.

If the mine became unprofitable and closed down, the solar-neutrino

experiment would almost certainly have to be terminated as well.

This particular worry was, however, to be short-lived, because

in 1968 the US abandoned the gold standard and the price of gold

rose rapidly.   The gold mine became highly profitable and was

still operational in 1978. Again though, the importance of wider

political and economic factors for the future of Davis's project

can be seen.

Pomeroy, in his report to Van Dyken, also reviewed the technical

progress of the experiment.   He was happy with this, as he

reported:[10]

> My over-all impression of the operations at this site is
> very favourable.   Ray Davis and John Galvin (his technician
> from BNL) are doing a good job.

Technical Snags with the Processing Equipment

It took five weeks to fill the tank with perchloroethylene. This

was a laborious task because the liquid could only be taken under-

ground in tank-car lots of 650 gallons. With the tank filled, the

next stage in the project was the installation of the processing

equipment which would enable a small sample of argon to be separated

from the helium and perchloroethylene vapours pumped out of the

tank as it was purged.   It was with this part of the apparatus

that Davis ran into the first major technical snag of the project.

Fig. 5.3 Story from Washington Post, June 23, 1966.

UNIONIZED GOLD MINE FACES PROBLEMS

By George Moses

LEAD, S.D. (AP) - Don Theisen lives up Grizzly Gulch in a corner of the Black Hills near Deadwood, where Wild Bill Hickock was shot dead holding aces and eights in a poker game.

Theisen's business is gold mining. He works a mile or more underground in the largest gold mine in the western hemisphere . Nearly half the gold mined in the United States comes from Lead (it rhymes with "speed").

For nearly 90 years, Homestake Mining Co.,a San Francisco-based firm,has worked the ore-rich rock in Lead and for nearly 60 years it has done so with non-union miners.

But two weeks ago, in an election which shook Lead to its rocky foundations, the AFL-CIO United Steelworkers of America won the right to bargain for Homestake workers. It was the seventh attempt to organize Homestake since 1947.

Theisen, 35, was a leader of the organizing drive, climaxed by an 841 to 512 union victory in the face of long-established Homestake policies that touch every miner and his family intimately.

Homestake owns and staffs a hospital for the 8300 people in Lead. Miners and their families pay neither hospital nor doctor bills.

The land a miner's house stands on is often owned by Homestake and he pays no rent

Free Library.
Homestake has built a community center called the Homestake Club, where anybody can swim or bowl without charge. And Homestake runs, without tax dollars, a free public library.

So what happened when the Steelworkers broke the barrier, and why, and what happens next?

The company and the miners appear to agree on the biggest problem. Gold is getting incredibly expensive to mine and the government, which by law buys all the gold Homestake produces, has been paying the same rate, $35 dollars an ounce, since 1934.

Since 1960, net income from gold has been shrinking. Then it cost slightly more than $27 to produce one ounce. The figure last year was nearly $32, despite more efficient mining, the company says.

Fig. 5.3 continued

So what are the Steelworkers, one of the AFL-CIO's most militant unions, going to do about it?

Theisen, chairman of the union organizing committee, says the big answer will have to come from Washington.

"Copper, lead and zinc mining were all in bad situations, too" Theisen said the other day on the steps of his house, puffing pipe smoke into the pine-scented air. "But with the backing of labor unions, these metals all got subsidies." He doesn't say directly whether he thinks the United States might start paying more than $35 an ounce.

Theisen is what is called a contract miner. So, he says, are most of his fellows. From a ton of rock they bring to the surface, Homestake produces a bit of gold scarcely larger than a collar button, about a third of an ounce.

$35 a day

Theisen says contract miners are guaranteed about $16.72 daily, more if they produce ore beyond a given tonnage. Some have earned up to $35 daily, he says.

His last paycheck, covering 15 days, came to $226.45 before deductions. For this, he worked a six-day, 48-hour week. Homestake supervisory personnel also work a six-day week.

Thus far he had met few problems in scaling-up his apparatus. However, he found that, for use with the 100,000-gallon tank, the processing equipment was unsatisfactory. There were two major difficulties. One was that the condenser which removed perchloro-ethylene vapour tended to get clogged up. The second difficulty was that the condenser was found to use excessive amounts of its coolant, liquid nitrogen.

The technical problems of the processing equipment occupied Davis's attention for most of the summer of 1966. By early September, he and a co-worker, Don Harmer, had finalised the details of a new processing system, and had put in a request to Dodson for extra funding to construct the new equipment. However, this money was not immediately forthcoming as the chemistry department operating budget had been cut for the financial year 1967. Indeed the original budgeting for the solar-neutrino experiment seemed itself to be short by an amount of $\sim$ $100,000. Dodson could not give permission for the new processing system to be constructed until October and only then after he had persuaded BNL director,Goldhaber, of the urgency of the situation. He wrote to Goldhaber that:[11]

The experiment is in genuinely serious financial trouble. This, it seems, was enough to convince Goldhaber and the AEC to increase the support for the neutrino experiment.

The combined technical and financial problems had by this time put the experiment well behind schedule. In December 1966, the processing equipment had still to be installed and it was not now expected that measurements would be made until February 1967 at the earliest.

1967 - The Experiment is Finally Ready

The new processing system was eventually installed in the mine

in January and February 1967. The final layout and photographs

of the completed tank are shown in Figs. 5.4 and 5.5. At the

end of February Davis sent a letter to John Pomeroy at the AEC

reporting that the detector was finally complete. He wrote:[12]

> We have everything together now, and the whole system works
> beautifully. Tomorrow John Galvin [his technician] and I
> plan to go out for a few weeks and put on the finishing
> touches, and start everything operating.

The first purges of the tank in which the large quantities of

atmospheric argon which had accumulated in the system were removed.

were started soon after. Eventually, after several such purges,

a sample of argon was taken back to Brookhaven for analysis. This

sample was found to be too large to be placed in the small counters

designed for the experiment,but, as it was the very first sample

from the tank, it was suggested (by Dodson) that it should be

counted anyway. It was placed in a relatively large counter and,

to everyone's surprise, it was found to give an extremely high

count rate. This high level of activity was soon explained by the

presence of Kr^{85} which had been dissolved in the perchloroethylene

and had contaminated the sample. Davis had to introduce a small

modification to the processing equipment whereby this isotope

and another atmospheric isotope (Rn^{222}) could be removed.

Another measurement was commenced on May 5, 1967. A small

amount of Ar^{36} carrier gas was introduced and the tank was left

exposed for 48 days. On June 22, the helium purge was started

and the Ar^{36} was found to be recovered with a 94% efficiency.

The argon sample was taken back to Brookhaven and placed in a

small proportional counter.

Fig. 5.4   The Experimental Layout at Homestake.

NEUTRON SOURCE TUBE

COOLING JACKETS

WATER FOR NEUTRON SHIELDING

PERCHLOROETHYLENE PUMPS

WATER TIGHT DOOR

LIQUID NITROGEN STORAGE

VESSEL (100,000 GALLONS)

CONDENSER

VAPOR TIGHT DOOR

TO YATES SHAFT

VAPOR TIGHT DOOR

GAS PROCESSING & CONTROL ROOM
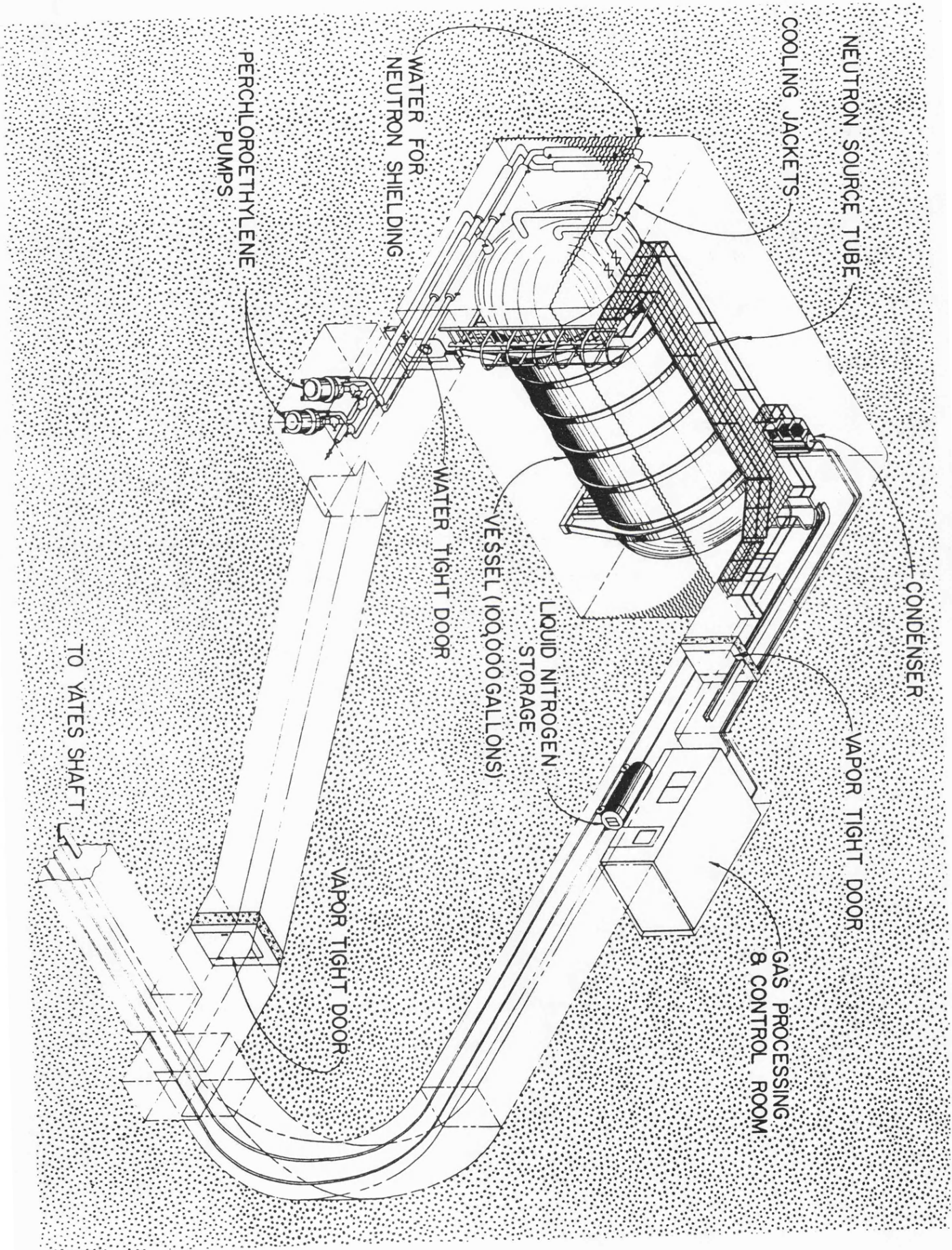
Fig. 5.5. The Completed Tank.
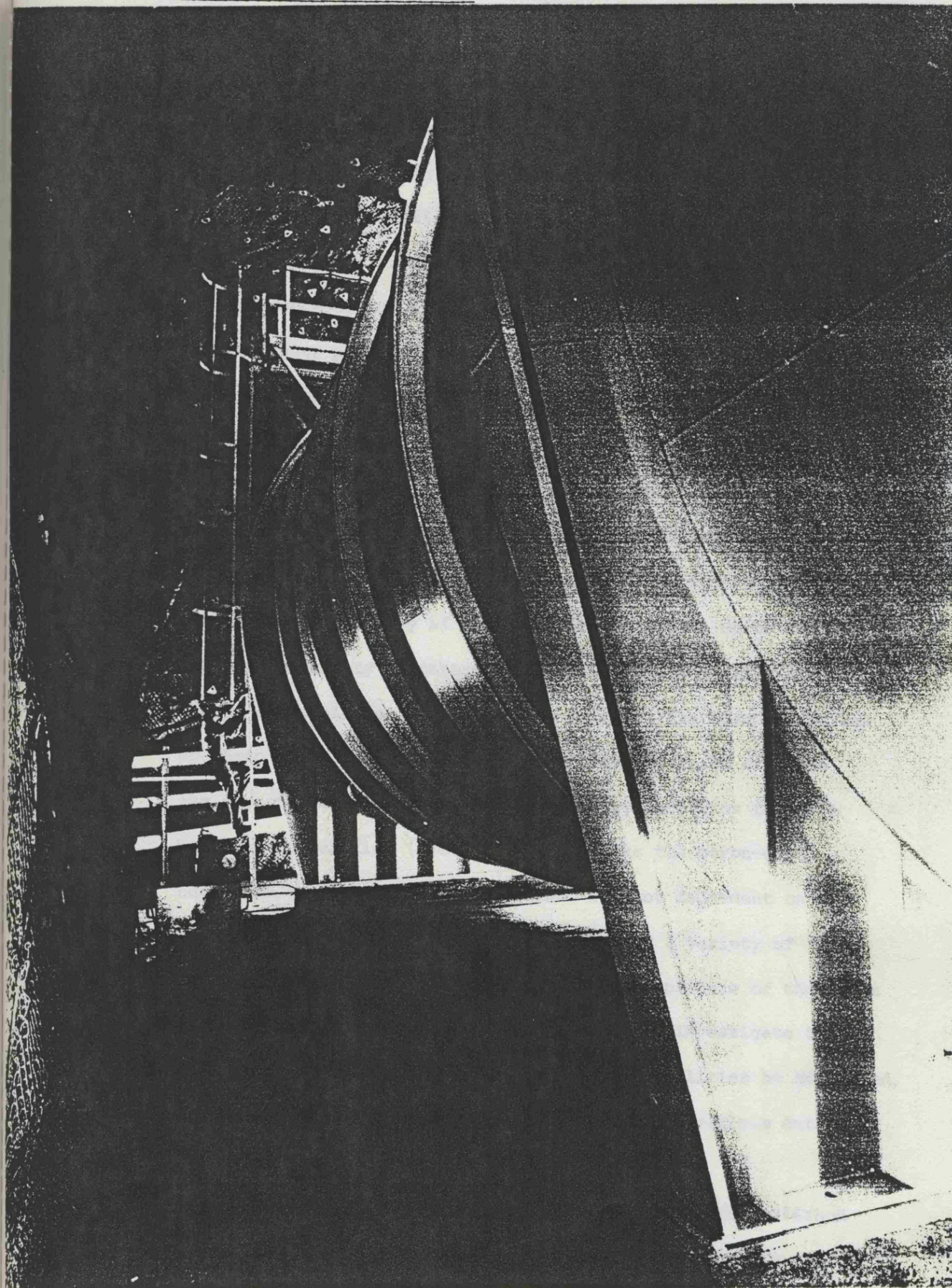
Whether the previous years of effort put in by Brookhaven

and the Caltech group were worthwhile depended now on the tiny

sample being counted in a heavily shielded part of the basement

of the Brookhaven chemistry department. Davis would soon know

whether or not his neutrino-detection programme was finally

to meet success, and the nuclear astrophysicists would know

whether their prediction had been confirmed.

Neutrino Spectroscopy

Once solar-neutrino astronomy became a more realistic

undertaking in 1964, as signified by the funding of the Davis

experiment, other possible detection processes were given detailed

investigation. Bahcall (1964c) published an article entitled,

'Neutrino-Spectroscopy of the Solar Interior', in which he

proposed a variety of detection experiments with different neutrino-

energy thresholds so that neutrinos from all the branches of the

pp-chain could be investigated. In particular, he stressed

the importance of measuring the low-energy neutrinos from the

basic p + p reaction (see Fig. 2.2). Unlike the boron-eight

neutrinos, the flux of these neutrinos was not dependent on

the detailed solar model. The detection of a variety of

neutrino fluxes would enable a very detailed picture of the solar

interior to be constructed. Bahcall did not investigate the

experimental feasibility of the various possibilities he suggested,

but he did calculate the cross-sections for the various detection

reactions.

Another notable article along similar lines was published

by the Soviet physicist, V.A. Kuzmin (1965). This article was

to prove of great importance for later developments in the field

because in it Kuzmin suggested that the $Ga^{71} + \nu \rightarrow Ge^{71} + e^{-}$

reaction could be used to detect the low-energy solar neutrinos

(pp neutrinos). This reaction forms the basis for the gallium

solar-neutrino detectors now under construction in the United

States and the Soviet Union (see Chapter 7).

Reines's Neutrino-Detection Programme:

The period 1964 - 1967 saw not only theoretical work on new

detection processes, but also the construction of a further

three detectors. These other experiments were all of the direct-

counting type and all were, in one way or another, connected with

that other pioneer of neutrino detection- Fred Reines.

Reines, having succeeded in detecting reactor neutrinos in

1956, had also looked for other areas into which he could extend

his technique. Rather than choose solar neutrinos, as Davis

had done, he had put his efforts into the detection of cosmic-

ray neutrinos. These are neutrinos produced by the interaction

of cosmic-ray protons with the Earth's atmosphere. The neutrinos

produced are of much higher energies (typically billions of

electron volts) than solar neutrinos and are therefore suited to

the direct-counting methods which Reines favoured. Another

attractive aspect presented by this source of neutrinos was that

they could be used for exploring problems in high-energy and

particle physics. The cosmic-ray neutrinos had higher energies

than could be produced in accelerators and reactors.

In the early 1960's, several groups proposed experiments

to detect cosmic-ray neutrinos. Reines, in collaboration with a

team from the University of Witwatersrand, S. Africa, was one

of the first to meet success.[13] Reines's experimental apparatus

consisted of 36 scintillation detectors housed in a tunnel

500 feet long and eight feet in diameter at a depth of approximately

two miles. The two miles of rock shielded the experiment from the

primary cosmic-ray muons. The experiment was located in the

East Rand Proprietary Gold Mine (the deepest mine in the world).

The muon secondaries produced by the interaction of muon neutrinos

with the rock could be observed by detecting their characteristic

scintillations. The experiment was on a scale similar to

Davis's solar-neutrino detector and Reines too received his

funding from the AEC ($\sim$\$500,000).

Reines's approach to neutrino detection since 1958 had thus

been similar to Davis's in that he too had used large underground

detectors. His programme was different, however, in that he

used direct-counting methods. Given his interests it is not sur-

prising that, when the importance of the comparatively energetic

B^8 neutrinos was established, he should attempt to mount a solar-

neutrino experiment, but with a direct-counting rather than radio-

chemical approach.

As mentioned in the previous chapter, in 1960 Reines was not

optimistic about solar-neutrino detection, and again, in 1962,

he had written that the solar-neutrino detection problem 'stops

me cold.'[14] Nevertheless, he was keeping in close touch with

the leading figures in the field such as Fowler, Bahcall, and,

of course, Davis. He had reason to be particularly familiar with

Davis's work, as Davis's Barberton Mine experiment was not far

from Reines's own institution, the Case Institute of Technology,

Cleveland, Ohio. Reines was thus well aware of the developments

around 1963-4 which made solar-neutrino detection look a more

attractive proposition.

The Elastic-Scattering Experiment

One of the experiments Reines was working on at that time was an attempt to observe the elastic scattering of neutrinos by electrons - $\nu_e + e^- \to \nu_e + e^-$. This reaction, which had been predicted by Feynmann and Gellman, had yet to be observed. He realised in early 1964 that the apparatus he was using to search for this reaction could also be used to set an upper limit on the solar-neutrino flux. The experiment consisted of a 200-litre liquid-scintillator detector surrounded by a large Cerenkov anticoincidence detector (to eliminate spurious background counts). The 'kick' which the electron received from the neutrino should be observable by the accompanying scintillation. In order to reduce the background further, the equipment had been installed 2,000 feet under the Earth's surface in a nearby salt mine. In this location the experiment might be sensitive to solar neutrinos. Reines, together with his student, W. R. Kropp, published the result of their limit on the B^8 flux ($10^9 \nu_e \, cm^{-2} sec^{-1}$ or 1000 SNU) in an article in Physical Review Letters in March 1964 (Reines and Kropp, 1964). In their paper, Reines and Kropp described their experiment as 'complementing' Davis's effort. They estimated that, with a similar detector of 10,000 gallons situated in a deep mine, they should be able to detect the predicted B^8 neutrino flux ($\sim$50 events/year).

The disadvantages of using this particular reaction to detect solar neutrinos were two-fold. Firstly, since elastic scattering of neutrinos by electrons had not itself been observed previously, a negative result with this process would have an ambiguous interpretation. The second disadvantage was that

the cross-section for the reaction was comparatively small; thus

not many events were likely to be observed and it would be diffi-

cult to separate the signal from the background. There was,

however, an important advantage to be gained from this type of

detector. This was that the recoil electrons would reflect

the direction of the incoming neutrinos and thus give a clue

as to their source. Thus, unlike Davis's experiment, Reines's

could, in principle, confirm that he was observing solar

neutrinos.

Reines eventually.went on to build a larger version (4,000

litres of scintillator) of this apparatus in collaboration with

the South African group of cosmic-ray neutrino experimentalists.

This detector was housed in the same mine in S. Africa which

had been used for the cosmic-ray neutrino experiments. The detector

consisted of five sections and was viewed by 35 photomultiplier

tubes located at each end (see Fig. 5.6). A large shield of

paraffin and borax was placed around the detector in order to

cut down the background from neutrons and other radiations

naturally occurring in the surrounding walls. This size of

detector was expected to register about 24 events/year from

solar neutrinos. In November 1966, Reines gave an important

review paper at a Royal Society meeting in London in which he

described the progress of the various experimental attempts

to detect solar neutrinos (Reines, 1967). (Davis had been

invited to this meeting but had been unable to attend because

BNL could not come up with the travel expenses!). In regard

to his detector in S. Africa, Reines was able to report that:

> The detector is now in place, filled and surrounded by
> the borax and paraffin shield. The electronics are being
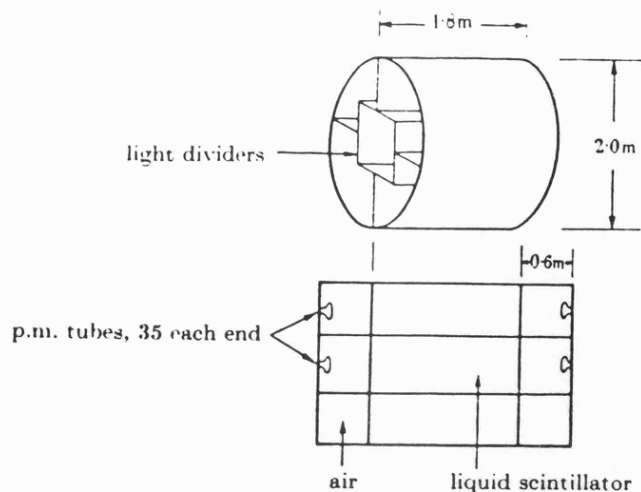
Fig. 5.6 The Reines Elastic-Scattering Experiment
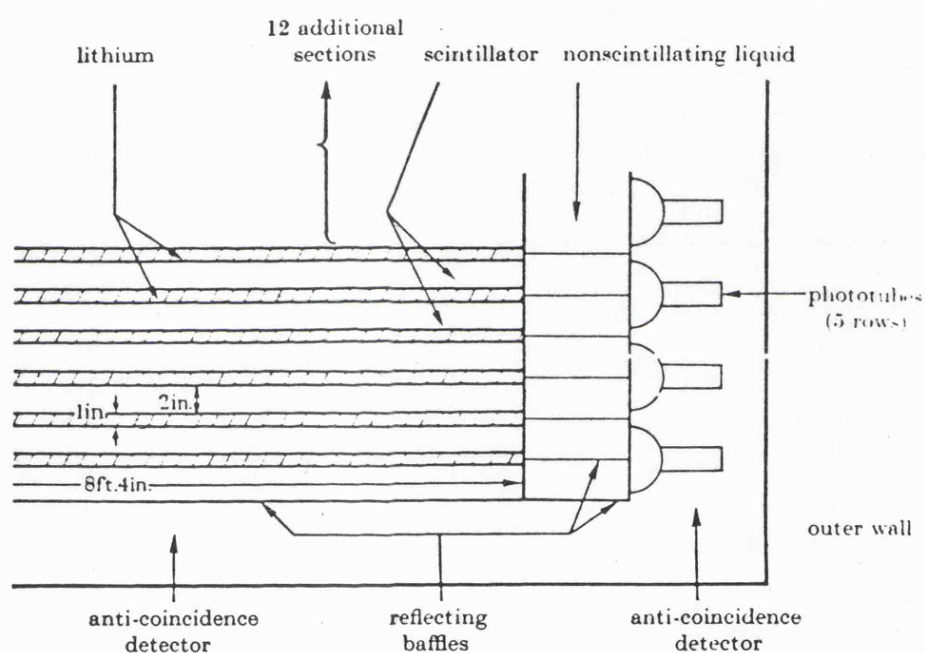
Fig. 5.7 The Reines-Woods Lithium Experiment
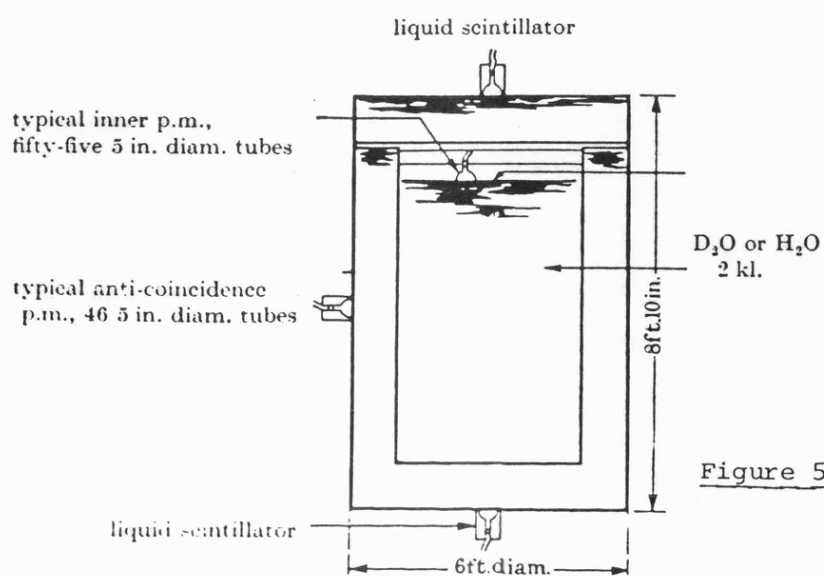
Figure 5.8. The Jenkins D_2O

Experiment.

All figures taken from Reines (1967)

debugged and installed, and we should, in the next few
months, have some better idea of the background levels...
(Reines, 1967: 169).

It can be seen that this experiment was expected to be 'on the
air' at approximately the same time as the Davis experiment.

The Lithium-Seven Detector

Reines was involved with another direct-counting approach
to solar-neutrino detection. This used the inverse beta-decay
reaction

$$\nu_e + Li^7 \rightarrow Be^7 + e^-$$
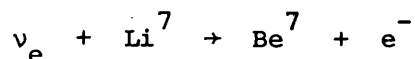
This detection reaction was proposed by Reines and Woods (a
colleague at Case) in Physical Review Letters in January 1965
(Reines and Wood, 1965). A detector based on this reaction was
constructed and consisted of large thin slabs of target surrounded
by liquid scintillator. Again the electrons produced should be
accompanied by characteristic scintillations. The planned
arrangement of the detector is shown in Fig. 5.7. With this
apparatus it was expected to see about 60 events/year against
a background of 20 events/year. The experiment was housed in the
same salt mine as had previously been used for the Reines-Kropp
elastic-scattering experiment. At the Royal Society meeting
Reines reported that:

...the essence of the experimental arrangement is
now nearing completion (Reines, 1967:165).

This experiment, too, was thus expected to be ready at about the
same time as the Davis experiment.

The Jenkins Experiment

The third solar-neutrino detection experiment to be discussed
in this section also came from the Case Institute of Technology.
Although the initiator of the project, T.S. Jenkins, was a member

of Reines'group and had worked in the past with Reines, both

on neutrino detection at Savannah River, and on the cosmic-ray

neutrino experiment, Reines was not directly involved with

this experiment. His attitude towards it was described to me

as 'luke-warm'[15] but he did agree to support some preliminary

work by Jenkins to study feasibility. The Jenkins experiment

is perhaps the most interesting of the three because it was

actually completed before the Davis experiment. There was thus

considerable interest in it at the time. Indeed there was an

air of competition between Jenkins and Davis. For instance,

H. Uberall, a theoretical physicist who had helped work on the

theoretical calculations for the Jenkins experiment, wrote in

early 1966:[16]

> I think Jenkins' experiment is an exciting story, and
> I wonder whether he will overtake Davis.

Jenkins himself has written to me that:[17]

> You might say there was a rivalry but it was never
> openly expressed.

The basis of the experiment was again an inverse beta-decay

reaction with direct counting.[18] The reaction to be used was

$$\nu_e + D \rightarrow p + p + e^-$$
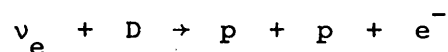
The experimental technique consisted of searching for the Cerenkov

light produced by the emitted electron in a large volume of

heavy water (D_2O). Thus the heavy water served as both the

target and the detector. 2,000 litres (550 gallons) of D_2O

were used in the experiment and this was viewed by fifty-five

photomultiplier tubes surrounded by an anticoincidence shield

consisting of liquid scintillator (see Fig. 5.8). The liquid

scintillator shield registered any spurious events induced in

the inner tank by charged particles entering from the outside.

In order to reduce cosmic-ray backgrounds the apparatus was

located 585 meters underground in a salt mine (the same one as

used in the other Case experiments). It was expected that

~96 solar-neutrino-induced events a year would take place in

the tank.

The main advantage of the detector was that it was compara-

tively cheap.[19] Apart from the D_2O itself, most of the apparatus

could be constructed from parts already available in the Case

physics department. The heavy water would have cost about

$100,000 but fortunately Jenkins was able to persuade the AEC to

loan him a sufficient quantity for the duration of the experiment.

By February 1966, Jenkins had the experimental apparatus

completed and installed in the mine. He initially filled the

tank with H_2O in order to see what the background was like. To

his surprise he found it was very high (of the order of one

count per minute!).[20] At the time Reines was delivering his

report to the Royal Society in November this background had not

yet been eliminated. Jenkins carried out exhaustive tests to

find its source. He managed to show that it came neither from

natural radioactivity in the rock nor from cosmic rays. Since

these were the principal background emitters in solar-neutrino

experiments the large signal was something of a puzzle. Eventually

the mystery was solved. It turned out that a colleague of Jenkins

had accidentally left a radioactive source in the vicinity of

the experiment![21] In the event, even with this background

eliminated, it was discovered that there was still a background

from cosmic rays and natural radioactivity in the rock of about

3000 events a year. As the expected signal was 96 events a year, it seemed that this experiment could not hope to detect solar neutrinos. By March 1967 it was clear that the Jenkins experiment had been unsuccessful. As Bahcall wrote to Jenkins:[22]

> I am unable to think of any likely new experimental possibility that can be achieved with your current experimental system. I wish that it were otherwise!

The deuterium experiment was continued throughout 1967 and Jenkins did manage to improve the sensitivity somewhat but never to the point where he could set a better limit on the flux than Davis was able to set with his Barberton experiment. By the time Jenkins had managed to achieve the same sensitivity as the Barberton experiment, Davis's new 100,000 gallon experiment was already in operation.[23]

Discussion of Direct-Counting Experiments

In conclusion to this section, it should be noted that all the direct-counting solar-neutrino experiments described above were designed to detect the boron-eight flux calculated by Bahcall in 1964 ($\phi_B 8 = 2.5 \times 10^7$ neutrinos $cm^{-2} sec^{-1}$). However, even with this flux they would be near to the limits of their sensitivity. The realisation, in early 1967 that the boron-eight flux was only half this value (because of new measurements of S_{33} - see next chapter) meant that these experiments were even less likely to succeed. In addition, none of the detectors had undergone the many years of experimental development which the chlorine experiment had undergone and hence experience of the likely backgrounds to be encountered was limited. As we have seen, it is the separation of the neutrino signal from the background which is the bugbear of solar-neutrino astronomy. In

such circumstances, it is not surprising that the above experi-
ments have made very little impact, and that it has been the
comparatively lavishly funded Davis experiment which has dominated
the field.   However, all the above experiments were serious
efforts at solar-neutrino detection. As Davis wrote to Bahcall,
in 1969, in urging him to include a description of these experi-
ments in a review he was writing:[24]

> I hope you did decide to include the experimental
> detectors of Reines, and others, and Jenkins.
> These detectors would perhaps now be giving results
> if it were not for the low flux of B^8 neutrinos.

The above experiments will be discussed again briefly in
Chapter 7 in the context of mapping out the response to Davis's
result.[25]

Summary

As we have seen, over the period 1964 - 1967 four solar-
neutrino detectors were constructed. Of these experiments,
Davis's was by far the most sensitive. However, if it turned
out that there was a larger flux of neutrinos than the theoreticians
expected, then the prospects for solar-neutrino astronomy as
a whole looked very rosy indeed.[26]   All the detectors would
be capable of measuring something. The immediate future of
solar-neutrino astronomy thus depended crucially on the results
of the most sensitive experiment - that of Ray Davis.

NOTES FOR CHAPTER FIVE

1. Brookhaven National Laboratory internal memorandum, B.W. Quinn to Procurement Review Board, October 16, 1964.

2. This idea was later dropped as it was felt that Sunshine might not agree to an independent consultant. Instead an official from the Bureau of Mines was appointed to supervise the work.

3. Brookhaven National Laboratory internal memorandum, B.W. Quinn to Procurement Review Board, November 24, 1964.

4. Letter, R. Davis to Philip Morrison, 3 December 1964, Morrison had been at Caltech and had published an article on neutrino astronomy in Scientific American - he was thus keeping a close eye on developments (see P. Morrison, 'Neutrino Astronomy', Scientific American, 207, August 1962, 90-98).

4a. The impact of the wider political reality in this case is not, however, a direct impact on the content of scientific knowledge (stage 3 of the empirical programme of relativism - see Chapter 1). The effect of the wider commercial/political reality was at most an inconvenience which slowed down the production of knowledge. However, it should be remembered that even if they do not impact on the content of knowledge, the wider political realities are as much a part of the environment in which science is carried out as for any other activity.

5. Letter, Donald T. Delicate to B. Munhofen, January 2, 1965.

6. Of course Homestake's attitude does not necessarily imply any more liberal attitudes. After all, S. African mining companies used predominantly black labour forces and no-one would say that this is evidence of their enlightened concerns.

7. It is worth noting in this respect that Homestake devoted four issues of the company magazine Sharp Bits to coverage of the neutrino experiment. See, Sharp Bits, 16, No. 8, September 1965; 17, No. 5, June, 1966; 17, No. 11, December 1966; and 20, No. 1, Spring 1969. Sharp Bits is obtainable from the Homestake Mining Company, 650 California Street, San Fransisco, California.

8. United States Government Memorandum, J.H. Pomeroy to A.R. Van Dyken, June 23, 1966.

9. 'Unionized Gold Mine Faces Problems' Washington Post, Thursday June 23, 1966.

10. Op. cit., note 8.

11. Brookhaven National Laboratory internal memorandum, R.W. Dodson to M. Goldhaber, October 7, 1966.

12. Letter, R. Davis to J.H. Pomeroy, February 28, 1967.

13. For details of this and other cosmic-ray neutrino experiments see F. Reines and J.P.F. Sellschop, 'Neutrinos from the Atmosphere and Beyond', Scientific American, 214, February, 1966, 40-48.

14. Letter, F. Reines to W. Fowler, November 29, 1962.

15. Letter, T. Jenkins to T. Pinch, April 30, 1980.

16. Letter, H. Uberall to J.Bahcall, January 3, 1966.

17. Op. cit., note 15.

18. The theoretical cross-section for this reaction was calculated by F.J. Kelly and H. Uberall, 'Absorption of Solar Neutrinos in Deuterium', Physical Review Letters, 16, 1966, 145-7.

19. This point was stressed by Jenkins to me in his letter, op. cit., note 15.

20. Letter, T. Jenkins to J. Bahcall, June 20, 1966.

21. I heard of this story quite by chance. Whilst having lunch at Princeton with Martin Schwarzschild, I was introduced to Jenkins's brother and it was he who told me this story. Jenkins's own comment on the incident was that the source had been stored near to his apparatus 'by a colleague who is notorious for his reluctance to communicate', op. cit., note 15.

22. Letter, J. Bahcall to T. Jenkins, March 12, 1967.

23. The full story of Jenkins's experiment has never been published. The account presented here is based upon copies of the relevant correspondence which Professor Jenkins has kindly supplied. Also an (unsuccessful) research proposal submitted to the NSF in 1973, in which a plan to build a larger deuterium detector is outlined, has provided much informative material.

24. Letter, R. Davis to J. Bahcall, June 18, 1969.

25. As it has turned out, direct-counting solar-neutrino detection has only become more of a feasible proposition recently with the development of the indium experiment (see Chapter 7 for details).

26. It should be remembered that the development of radio-astronomy was held up for years because theorists thought there would be no detectable sources!

CHAPTER SIX

THEORETICAL DEVELOPMENTS 1964-7

The theoreticians' final preparations for Davis's experiment

form the subject matter of this chapter. As was mentioned in

Chapter 5, the bulk of the theoretical work over this period

was carried out at Caltech by John Bahcall. Thus most of the

material presented here concerns Bahcall's involvement. However,

as we shall see below, other theoreticians (notably Ezer and

Cameron) also did significant work on the neutrino-flux predictions

over this period. The account will again largely be chronological

and takes us from the 40 SNU prediction of March 1964 to the

prediction on the eve of the experiment, of 19 SNU (see Fig. 1.1).

Neutrino-Absorption Cross Sections

As was emphasised in Chapters 3 and 4, the dramatic increase

in the expected signal in Davis's experiment, which occurred in

1964, was largely due to Bahcall's work on the analogue state.

When Bahcall had made his initial calculations in September 1963,

there was very little experimental information available on the

analogue state for the argon thirty-seven system. Indeed Bahcall

had persistently been urging nuclear experimentalists to make

measurements on the calcium thirty-seven system in order to

provide indirect confirmation of his calculations. As more

experimental evidence became available in 1964, Bahcall had to

modify slightly his original estimate of the absorption cross-

section. The first modification was noted by Bahcall (1964b)

in a paper published in Physical Review in July 1964. This paper

contained more details of the cross-section calculations than

his previous shorter <u>Physical Review Letters</u> paper.   Bahcall

estimated that, with the most up-to-date information on the ab-

sorption cross-sections, the total signal Davis could expect

was 36 ± 20 SNU. By the end of the year, with yet more detail

on the argon thirty-seven system available, Bahcall again

revised the prediction and obtained a value of 30 ± 20 SNU.[1]

In the light of these revisions, it seemed as if Bahcall's

original estimate of the effect of the analogue state had been

slightly over-optimistic.

S_{17}

As was mentioned in Chapter 4, Pochoda and Reeves had

drawn attention to the possibility of a large uncertainty in S_{17}.

Since this reaction leads directly to the branch of the pp-chain

where boron-eight neutrinos are produced (see Fig. 2.2), any

changes in S_{17} would also affect the rate of production of boron-

eight neutrinos.   The uncertainty stemmed from the paucity of

low-energy data on the reaction.   Kavanagh, in 1958, had only

been able to make measurements at two energies and hence a

variety of extrapolations to the zero-energy intercept were

possible. The question of this uncertainty gained renewed

urgency in late 1964 when a Caltech nuclear physicist, Tom

Tombrello, attempted to make a theoretical estimate of S_{17}

using data from the related reactions: $Li^7 + n \rightarrow Li^7 + n$

and $Li^7 + n \rightarrow Li^8 + \gamma$.[2] Tombrello's estimate came to half

the previous experimental value of Kavanagh (as calculated by

Christy and Duck).[3]   If Tombrello's calculation was taken

seriously then it would imply a solar-neutrino detection rate

of only 15 ± 10 SNU.[4]

Bahcall hoped that this uncertainty could be cleared up by a

new measurement of S_{17}, hopefully to be made by Parker who was

now at Brookhaven. The urgency of the situation can be seen

from Bahcall's letter to the Goldhabers of November 17, 1964

(this is the same letter in which Bahcall urged the Goldhabers

to search for Ca^{37}); he wrote:[5]

> Finally, I would like to express my strong hope that
> someone at Brookhaven will repeat the $Be^7(p,\gamma)B^8$ experiment.
> This is rapidly becoming the most uncertain link in the
> long chain of reasoning leading to the predictions for the
> solar neutrino experiments. It seems a shame that only
> two data points are available for this laboratory
> experiment which forms the basis of our study of the
> solar interior...Ray Davis has expressed his willingness
> to make a Be^7 target and Pete Parker has indicated plans
> to try the experiment. I hope you will encourage them.

The importance of this particular measurement can also be seen from

a letter Davis sent to Bahcall a few weeks later. He wrote:[6]

> I have been busy and have not had a chance to talk with
> Parker. Our whole story hangs on the (p,γ) cross section
> on Be^7!

The concern displayed over S_{17} _now_ reflects something of

a change in attitude. Before the experiment was funded, Bahcall

and Fowler had not been greatly worried by the uncertainty in

S_{17} and Fowler, it will be recalled, had even claimed in his

letter to the AEC that 'little more can be done in the study of

the nuclear reaction rates either theoretically or experimentally'.[7]

However, it now seemed that more experimental work had to be done

on S_{17} before the neutrino fluxes could be confidently predicted.

Eventually Parker, with the help of Davis (who prepared the

Be^7 target) made new measurements of S_{17} at Brookhaven over the

summer of 1965. The results were good for Davis's experiment

as Parker found a value that was even larger than Kavanagh's

$(S_{17} = 0.043 \pm 0.004$ keV-barns$)$[8], thus boosting the expected

neutrino flux. As Davis wrote to Bahcall:[9]

Peter did a very clean job on the Be^7 (p,γ) cross section...
Fortunately the cross section was high ...I am greatly relieved,
since I was prepared for the cross section to be a factor
of two below Kavanagh's value.

The impact of this new value on the neutrino-flux prediction will

be discussed below.

The Ezer-Cameron Solar Model

As we saw in Chapter 4, there were a variety of solar models

available for neutrino-flux predictions in 1964.   These models

gave what was considered to be a reasonable agreement over the

predicted neutrino fluxes. In 1965 another model was produced.

This was computed by A. Cameron, who, as we have seen, had a

longstanding interest in the solar-neutrino project, and his

student Dilly Ezer.[10] The Ezer-Cameron model caused quite a

stir when it first appeared because it seemed to predict lower

fluxes ($\phi_B 8 = 0.95 \times 10^7$ neutrinos $cm^{-2} sec^{-1}$, $\phi_{Be} 7 = 0.69 \times 10^{10}$

neutrinos $cm^{-2} sec^{-1}$) than both the Sears, and Pochoda and Reeves

models. The expected detection rate with the Ezer and Cameron

fluxes was found to be only 12 SNU .

The first news of this new model came from Davis, who had

met Cameron at a seminar. The worry caused by the lower prediction

can be seen in Davis's letter to Bahcall where he informed him

of the Ezer-Cameron results.[11] Davis was particularly concerned

at the apparent sensitivity of the neutrino fluxes to variations

in the hydrogen content (X) of the Sun. He wrote:[12]

> An important difference in their [Ezer's and Cameron's]
> calculations is that they take X = 0.739 and this is
> why their fluxes are so low...I did not fully appreciate how
> fast the $\phi(B^8)$ falls with increasing primordial H-content.
> In fact if X = 0.77 the flux of B^8 neutrinos would drop
> out of sight.

When Bahcall received Davis's letter, he immediately wrote

to Sears to see if he had any comment on the discrepancy between

the models.[13] In reply, Sears pointed out that his model

was not strictly comparable with the Ezer-Cameron model since

he had varied the parameter, Z/X, rather than X alone (Z is the

heavy-metal content of the Sun).[14] Nevertheless, he did have

some sympathy with Davis's worries. He wrote:[15]

> Ray's fears are not groundless; the dependence of N_B8
> [ϕ_B8] on abundances was emphasized in the pithy abstract
> of my paper. Oh well, even a negative result would be
> valuable, eh?

The situation was clarified to some extent when a preprint

of the Ezer-Cameron paper became available. Bahcall, John

Faulkner (a colleague of Bahcall's at Caltech) and Sears looked

at the Ezer-Cameron model in some detail and found that it

differed from Sears's model in several ways.[16] In particular,

Ezer and Cameron used a smaller value of S_{17} (0.2 keV-barns) than

that favoured by Sears (0.3 keV-barns). This smaller value

was, of course, quite permissible in view of the uncertainty

noted above in this cross-section. This choice of a different

value for S_{17} was thought to account for about half the difference

in the neutrino-flux predictions. The source of the rest of the

difference was not clear, but was held to result from a number

of small differences in parameters (in particular, different

mixes of opacities), and in the way the computation was performed.[17]

These differences pointed to a growing air of uncertainty over

the exact prediction of the neutrino fluxes. For instance, it

led Bahcall to comment (to Davis):[18]

> I think it only serves to illustrate how sensitive
> is the quantity which you will measure [the B^8 flux] and
> how uncertain the astronomical information really is when

> you seek quantitative statements about even the most
> thoroughly studied of stars, the sun...I don't think
> we know more about the B^8 flux than that it can be
> reasonably expected to lie in the range (4 to 1) x 10^7 vs
> $cm^{-2}sec^{-1}$...This is all the better for your experiment
> since it will give us valuable astronomical information
> and will saddle the models with a sensitive fact. (My
> emphasis).

This comment was contained in the same letter quoted in Chapter 4

(p.147) where Bahcall pointed out the uncertainty in S_{33}.

The uncertainties in S_{33} and S_{17}, combined with the various

solar-model uncertainties now apparent, made the predictions as

a whole seem somewhat less definite than they had appeared to

be in 1964. Then, it will be recalled, the argument was that

the fluxes were known well enough to justify an experimental

attempt to measure them. Now the argument seemed to be that the

experiment would clear up the doubts and uncertainties, and,

hence valuable astronomical information would be accrued. This

change in emphasis in the pre-funding and post-funding climate

will be discussed in more detail in Chapter 10.

The difference between the Ezer-Cameron prediction and the

Sears prediction was reduced somewhat when it became clear that

Parker's measurements supported a larger value of S_{17}. With

S_{17} = 0.035 keV-barns,[19] the total signal expected according

to the Ezer-Cameron model was 21 SNU as opposed to a signal of

27 SNU from Sear's model (J) (also with the new value for S_{17}

incorporated).[20]

In addition to calculating the neutrino fluxes expected with

a 'normal' Sun, Ezer and Cameron considered a Sun which had

evolved with a varying gravitational constant, G, as expected

according to Brans-Dicke cosmology. They found that if G varied

as Brans and Dicke expected,[21] then larger neutrino fluxes would

result ($\phi_B 8$ = 4.6 x 10^7 neutrinos $cm^{-2}sec^{-1}$; $\phi_{Be}7$ = 1.4 x 10^{10}

neutrinos $cm^{-2}sec^{-1}$).[22] It thus seemed that, even if Davis was

not able to detect boron-eight neutrinos from a Sun evolved

according to conventional cosmology, then at least he might be

able to test the unconventional assumptions of Brans and Dicke.

Bahcall's Close Relationship with Davis

Throughout the period that Bahcall was attempting to resolve

the various anomalies in the theoretical prediction, he kept

in close touch with Davis. He let him know of the latest develop-

ments on the theoretical front, and Davis, in turn, sent him

many letters reporting on his experimental progress.

The unique partnership which was developing between Bahcall,

the theoretician, and Davis, the experimentalist, did, however,

require delicate handling - especially as Bahcall was located

on the West coast and Davis on the East coast.   The sensitivity

needed can perhaps best be seen by their reaction to two

episodes where reporting of the collaboration gave the impression

that only one of them was responsible for the experiment. Davis

on one occasion expressed concern to Bahcall about a report in

Time magazine (this is the report referred to in Chapter 3):[23]

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

Later, it was Bahcall's turn to express concern when a report

appeared in Scientific American[24] which seemed to attribute the

whole project to him - the author of the article being under the

impression that Davis was just the technician who was supervising

the building of the experiment!   After Bahcall complained to the

Editor, <u>Scientific American</u> published an <u>erratum.</u>[25]   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 <u>Scientific</u>
> <u>American</u> that somebody complained about getting too much
> <u>credit!</u>

Although both sides in the collaboration had the same aim -

to make a measurement of the solar-neutrino flux - this particular

aim, as has been repeatedly stressed, formed part of two very

different research programmes. Davis's primary concern as a

neutrino experimentalist was the detection of neutrinos - he was

not so much interested in the test of nuclear-astrophysical

theory. For the nuclear astrophysicists on the other hand, it

was the test of their theories which made solar-neutrino astronomy

such an attractive proposition.

Apart from calculating the theoretical flux of neutrinos,

Bahcall also helped Davis by keeping him informed of wider

developments in theory. Of more immediate use, he would advise

Davis over particular theoretical issues raised by the experiment,

such as how often to take samples from the tank. Bahcall

described this working relationship to me as follows:

> I used to go back continually to talk to him about
> theoretical things, advise him how often to take samples,
> current levels of theory. He's always been interested in
> the theory and he wanted to talk to me about that all the
> time, I worked quite a while. We made our first calculations
> on the expected background from the cosmic-ray neutrinos.
> And so there was always questions coming up in the experiment
> and in the language, I was the'house theorist'. Whenever
> a wild article came out, then Ray would call me and I would
> have to go read it, and explain it to him.

The relationship between Bahcall and Davis was put on an insti-

tutional basis in April 1966. Davis sent a memorandum to Dodson

in which he outlined the case for Bahcall being made a consultant to Brookhaven.[26] On April 25, Dodson wrote to Bahcall as follows:[27]

> Ray Davis would like to have your very helpful interactions
> with the solar neutrino experiment recognized with a BNL
> appointment which would further facilitate these
> interactions.

The contract between Bahcall and Brookhaven stated that Bahcall would be paid $75 a day for his time devoted to the experiment, plus reimbursement of travel expenses and a subsistence allowance.[28] Bahcall was thus truly the 'house theorist' - he was paid for his services by the same house!

Davis was keen to have Bahcall visit the site of the experiment. In his letters he would often tell Bahcall in advance the dates of his projected visits to the mine in case Bahcall wanted to meet him there. In Dodson's letter to Bahcall inviting him to become a BNL consultant, this wish was again expressed.[29] This visit would not be of much practical significance since Bahcall was not an experimentalist, but it would be of some symbolic importance - it would further demonstrate the commitment of Bahcall and the Kellogg group to the experiment. Soon after being made a BNL consultant, Bahcall did indeed visit the site; as he told me:

> I went to visit the site, I believe I'm the only theorist
> who has ever visited the site...I didn't have much useful
> to say but I wanted to be involved in that...

The bond which by this time existed between Davis and Bahcall was recalled for me by Bahcall:

> I was also the guy that encouraged him. You know there's
> a lot of hand-waving that goes on in something like this...
> 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, by this stage, professional relationships
had become extended into personal ones as a result of the colla-
boration. Bahcall's reference to Davis and himself having both
staked their careers on the project shows that more than just
an experimental test of an abstract theory was involved. Both
scientists had put a lot of personal effort into the project.
Each can be said to have invested his scientific credibility
in the project - a notion to which we will return later, in
Chapter 10, where the sociological implications of the above
developments will be discussed.

Bahcall's 1966 Prediction

As Davis's experiment neared completion, Bahcall once more
reviewed the theoretical prediction with a view towards publishing
another paper in Physical Review Letters. There was by this time
a considerable amount of new information available on the neutrino-
absorption cross-sections. Although this new information did not
lead to any major changes in the cross-sections for neutrino
capture by chlorine thirty-seven, the range of the uncertainty
from this source was slightly reduced.   In addition, Parker's
new value of S_{17} had to be incorporated into the prediction.
Finally, Bahcall had to take account of the recent developments
on the solar-model front.   As well as the Ezer-Cameron model,
there was another new model computed by Weymann and Sears.[30]
All the solar models tended to use different sets of nuclear-
physics data. In order to compare accurately the predictions
given by the different models, Bahcall had to rework the models

using a standardised set of nuclear-physics data (these were the
data given in Parker, Bahcall and Fowler (1964), plus Parker's
new value of S_{17}). Bahcall put all this information together
in his paper and obtained a value of the boron-eight flux of

$\phi_B 8 = (2.1^{+2}_{-1}) \times 10^7$ neutrinos $cm^{-2} sec^{-1}$ (Bahcall, 1966:400).

Combined with the latest calculation of the neutrino-capture
cross-sections this gave an expected detection rate for the
Davis experiment of 30^{+30}_{-15} SNU. It seemed that the main uncertainty
in this prediction arose from errors in the solar composition.
It was to errors such as these that the Ezer and Cameron model
had drawn attention. As Bahcall wrote:

> The primordial (or surface) composition assumed in
> computing the solar models represents the largest
> recognised uncertainty in the predicted capture rate:
> the errors given on the theoretical prediction are
> no more than guesses for the magnitude of this uncertainty
> (Bahcall, 1966:400).

This frank admission of the uncertainties just being 'guesses'
is again rather different from the air of certainty which pre-
vailed before the experiment was funded.

It can be seen that the predicted 'best value' in 1966 (30 SNU)
was lower than the 1964 'best value' (40 SNU). This reduction
seems to have resulted in part from the new neutrino-absorption
cross-sections (discussed earlier) and from the lower fluxes
predicted by the more up-to-date solar models. However, in
compensation for some of this reduction, there was an increase
caused by the larger value of S_{17}.

It was mentioned in Chapter 4 that, at the heart of Bahcall's
derivation of the 1964 'best value', was an averaging procedure
which seemed to favour a larger prediction. In 1966, Bahcall
was again faced with a variety of model predictions - so how

did he derive a 'best value' this time? It seems that initially

he took some sort of average, as he had done in 1964, and ob-

tained a value of $\phi_B 8 = (2.9^{+3}_{-1}) \times 10^7$ neutrinos $cm^{-2}sec^{-1}$.[31]

This value was mentioned in correspondence between Bahcall and

Sears shortly before the 1966 paper was submitted. It was also

larger than the value given in the final version of the 1966

paper (see above). It seems that, in the period before the

paper was submitted, Bahcall bowed to pressure from Sears who

argued, as mentioned in Chapter 4, that Bahcall should not

average the fluxes from several models but choose the flux from

the 'best' model. Commenting on Bahcall's value of $\phi_B 8 = 2.9 \times 10^7$

neutrinos $cm^{-2}sec^{-1}$ Sears wrote:[32]

> I do not agree with it. Obviously the best value is 2.1,
> neglecting varying G and the now obsolete Sears J.

As the final version of Bahcall's paper did give a best value of

2.1 for the boron-eight flux it seems that he derived a lower

prediction by using the 'ask the expert' (in this case Sears)

means of combining different predictions together.[33]

Again the flexibility in the 'best value' can be seen. By

using a different averaging procedure from that which he had

favoured in 1964, Bahcall was able to derive a lower 'best value'.

CNO-cycle Versus pp-Chain

Bahcall (1966) also calculated the expected detection rate

in Davis's experiment if the CNO cycle was (improbably) the

dominant energy cycle in the Sun. He found the expected signal

to be 35 SNU. As this was the same order of magnitude as the

signal expected from the pp-chain (mainly boron-eight neutrinos),

it did not look as if Davis's experiment could distinguish

between the two modes of energy generation.[34] Bahcall suggested,

however, that the results obtained by the other proposed solar-
neutrino experiments of the direct-counting type (as discussed
in Chapter 5) which were sensitive in different degrees to
CNO and boron-eight neutrinos, could be used to decide which
cycle predominated in the Sun.

S_{33}

There was one more major development over this period which
affected the solar-neutrino flux prediction. This was the long
awaited results of the Caltech measurements of the He^3-He^3
cross-section. These results became available in December 1966.[35]
Much to everyone's surprise, the results indicated, not a lower
value of S_{33} as Parker and Bahcall had expected, but a value
approximately five times larger (5,000 keV-barns) than the
old 'standard' value (1,100 keV-barns).

The measurements were carried out by two groups: A.D.
Bacher and T.A. Tombrello[36] who, as we saw in Chapter 4, had
started measurements in 1964 and H.C. Winkler and M.R. Dwarakanath,[37]
who were the first to make measurements at low enough energies
to enable reasonable extrapolations to the zero-energy intercept
to be made. In addition, some results obtained by a Chinese-
Russian group became available at around this time, and they
also indicated a larger value of the cross-section.[38] The
emergence of these experimental attempts to measure this cross-
section at this time were stimulated in part by the increased
availability of He^3 for use in the target. This rare material
became more abundant as a by-product of the construction of
thermo-nuclear weapons (the tritium used in hydrogen bombs decays
naturally into He^3).

Although the new value for S_{33} seemed to be approximately

5,000 keV-barns, there was still considerable uncertainty over

the extrapolation to this value from the low-energy measurements.[39]

In order to resolve this uncertainty more experiments were

commenced at Caltech with a view to obtaining even better measure-

ments at even lower energies.[40]

The delay in the construction of Davis's apparatus (as

mentioned in Chapter 5) enabled Bahcall to incorporate the new

value of S_{33} into the prediction before the experiment was ready.

The new larger value had the effect of diminishing the importance

of the branch of the pp-chain leading to boron-eight neutrinos

(see Fig. 2.2), and hence the neutrino fluxes were reduced in

magnitude. In a note submitted to The Astrophysical Journal

in May 1967 Shaviv (a young solar-model specialist who assisted

Bahcall), Bahcall and Fowler estimated that the boron-eight

flux was reduced to $\phi_B 8 = 1.6 \times 10^7$ neutrinos $cm^{-2} sec^{-1}$.[41]

Discussion of Interpretation of S_{33} Data

It will be recalled from Chapter 4 that Parker and Bahcall

had interpreted the S_{33} low-energy data as indicative of a possible

new extrapolation which would result in an even lower value of S_{33}.

The latest results, reported in 1966, went the opposite way to

such expectation. The new results and the old data are both

shown in Fig. 6.1. Now, viewed with hindsight, the old data took

on a new meaning. As one nuclear physicist who had been involved

with the cross-section measurements, told me:

> One of the reasons that the original prediction has changed...
> one of the most important reasons is the fact that this is
> the extrapolation that was being used [Fig. 4.2, line a]
> In hindsight it was a dumb thing to do. They should have
> been a little more suspicious of what was going on...In some
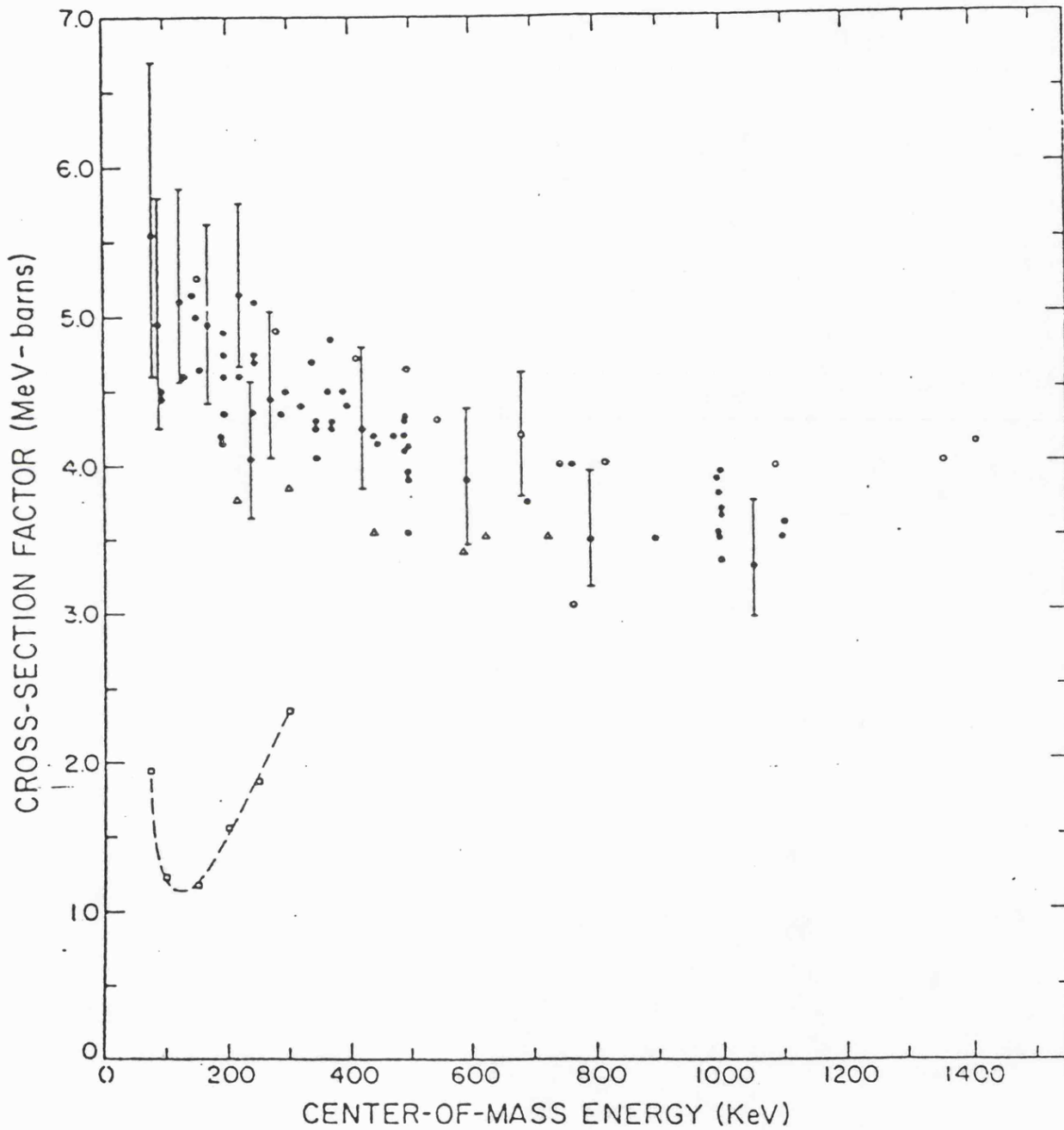
Fig. 6.1 The Cross-Section Factor, S_{33} from four Measurements

The data in the lower left region are from the early work of Good, Kunz and Moak. The triangles are from Neng-ming et al., the open circles from Bacher and Tombrello, and the solid circles from Winkler and Dwarakanath. This figure is to be found in Kavanagh (1972).

> sense I'm a little bit surprised that the people didn't
> try to redo the experiment before, earlier on...People
> had been going along rifely quoting this extrapolation
> and not really acting as if they were concerned, not
> really going on to say 'look this really ought to be
> remeasured; there's clearly something wrong'.

I asked another nuclear physicist, who had actually produced one

of the new measurements of S_{33}, whether he was surprised that his

result was five times larger than the previous 'standard' value.

He told me:

> I don't believe it was considered as a surprise, part
> of the reason being that it was suspect. Actually it
> was less of a surprise just interpreting the Oak Ridge
> data...The cross-section factor comes down and then
> sharply goes up and that was sorta worrying us. Why is
> it going up so sharply, what is going to happen at even
> lower energies when you extrapolate the thing? And it
> was really reassuring to find something behaving nice
> and smoothly. I do not believe that people could be
> surprised.

For these physicists the Oak Ridge S-factor plot indicates not

that a lower extrapolation is possible but that the data are

in themselves too suspect to draw any firm conclusions from them.

Some respondents commented to me that, as the Oak Ridge data

was produced in the 'early days' of nuclear physics, it was

not surprising that it was in error. Other respondents pointed

to possible experimental errors in the Oak Ridge data such as

'beam straggling'.[42]

It is, of course, easy to discard old measurements when

you have reason to believe the new measurements. One respondent

was disarmingly frank about this process of selection. This

respondent, who had been involved with the new S_{33} measurements,

told me why, for instance, he thought the data from the Chinese-

Russian collaboration (see Fig. 6.1) were correct and the Oak

Ridge data incorrect:

I don't think we would have believed them [the Chinese-
Russian data] except they fell right in with the stuff
that we were just getting...They weren't specifically
after the low-energy cross-section...There was nothing
wrong with the data...but after you have a cross-
section you believe, it is very easy to sort through
all the garbage and decide which of the ones made were
correct. Before the fact you have several things
that differ by an order of magnitude, you don't really
know which is right. And so it became very clear, for
example, that the [Oak Ridge] experiment was wrong
and the [Chinese-Russian] experiment was correct.
In actual fact, if you looked at the way the
experiments were done it would not have been clear that
either were particularly correct.

If nothing else, these statements show the interpretative

flexibility of the cross-section data. The interpretation is

constructed according to a variety of assumptions. In this case

we have seen that data which behave 'nice and smoothly' and

data which 'fit in with other results' are preferable to the

Oak Ridge data which have neither of these properties. It

is this interpretative flexibility which earlier enabled

Bahcall and colleagues to read the S_{33} data in one particular way.

Solar-Composition Uncertainty

Accompanying the note in the Astrophysical Journal, in

which the prediction with the new value of S_{33} was set out, was

another paper by Bahcall (co-authored with Cooper and Demarque).[43]

In this paper the uncertainty in the prediction which resulted

from the poorly known primordial solar composition was estimated.

As mentioned above, Bahcall had already drawn attention to this

uncertainty in his 1966 prediction. In order to obtain a more

precise estimate of this error, Bahcall had calculated the

neutrino fluxes predicted by solar models constructed by Demarque

and Percy which had a range of values of Z (the primordial heavy-

metal content of the Sun). The conclusion was that there was an

uncertainty in the neutrino flux of approximately a factor of two

due to solar-composition uncertainties.

The Prediction on the Eve of Davis's Experiment

As Davis prepared to make his first measurements in the late

Spring of 1967, the theoreticians checked over their predictions

once more. Ezer and Cameron made renewed model calculations using

the new value of S_{33} and obtained $\phi_B 8 = 1.18 \times 10^7$ neutrinos cm^{-2}

sec^{-1}.[44] Bahcall and Shaviv also worked throughout this period

to try to produce as precise a prediction of the neutrino flux

as possible before Davis made his first measurement. As Davis

informed me:

> John Bahcall and Willy Fowler were following every move
> as we were building it, so I saw quite a lot of them.
> John Bahcall took it as his personal task to do the best
> you can with the theory and have a forecasted rate when
> the experiment was done. So he kept a track of everything
> and he wanted to find out the number, and just before I
> tell him the number, he will say 'It's got to be that'.
> That was what he envisioned.

Bahcall was thus very anxious to know exactly when Davis

would start making measurements in order that he could prepare

his final prediction in time. For instance, on January 4, 1967,

he wrote to Davis as follows:[45]

> Dear Ray:
> Could you give me a call or drop me a card regarding
> the possibility that you will actually be taking data
> early in March. I will be on my way back to Pasadena
> from Israel at that time and, as we discussed earlier,
> could stop off at Brookhaven if the time seems really
> ripe.

It can be seen that Bahcall wanted to be there at the moment

towards which his activity over the previous four years had

been directed.

A lot depended on the measurement Davis was about to

make. Bahcall, it seems, felt increasingly apprehensive as that

date drew nearer; as he told me:

> I can remember being enormously nervous before the results
> came out...Between '64 and '65 there was a burst of
> activity, which a number of us at Caltech participated
> in...And after that nobody else was involved theoretically,
> and that was my thing. I had calculated the background,
> I had defined the predictions. I was the only guy working
> on that. 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.

As the moment approached, Bahcall did more and more calcul-

ations in order to make his prediction as exact as possible. With

the aid of Shaviv, he constructed a range of solar models using

the most up-to-date input data. This included not only the new

value of S_{33} but also a new estimate of the Be^7-electron capture

rate which had just been revised with the consideration of bound-

electron capture (Iben, Kolata, and Schwartz, 1967).[46] Iben,

who had initiated this calculation, found that the beryllium-

seven capture rate was reduced by about 20% with the consideration

of this effect. Bahcall and Shaviv predicted $\phi_B 8 = (1.4 \pm 0.8)$

x 10^7 neutrinos $cm^{-2} sec^{-1}$ and a total signal of 19 ± 11 SNU.[47]

This corresponded to a neutrino event rate in Davis's tank of

2 - 7 events/day. Their paper was submitted to the Astrophysical

Journal on August 10, just as Davis's first measurement was being

completed.

Although Bahcall's prediction on the eve of Davis's experiment

was lower than that of 1964 and Bahcall now placed more stress on

the uncertainty in the prediction due to the possible error in the

solar composition, it would be wrong to give the impression that

Bahcall was no longer confident that his prediction would be

verified. His confidence was, for instance, demonstrated by

two symbolic bets he had with other scientists at this time that

Davis would detect a signal within the error range of the prediction.[48]

Also, at a conference held in February-March 1967 Bahcall re-
iterated the unambiguous nature of the prediction. As he stated:

> A measurement of the solar neutrino flux provides an
> opportunity to test quantitatively unambiguous predictions
> of the theory of solar models for a supposedly well-understood
> star, the sun. (Bahcall, 1967:248).

Although Bahcall fully expected his prediction to be confirmed

he did speculate over the possibility that Davis would refute

the prediction and detect nothing at all. Indeed Bahcall (at this

same conference) claimed that one of the arguments that Davis had

put to Goldhaber in 1963 in order to try to persuade him to support

the experiment was that the most scientifically interesting result

would be a null result. However, Bahcall did not really expect

this to occur as his concluding comment at the conference reveals:

> In preparing this talk I came across some remarks made by
> Eddington in the same address in which he proposed hydrogen
> fusion as the ultimate energy source for the sun which
> express more accurately the true sentiments of both Davis
> and myself as we await the first results of his experiment.
> Eddington said: "I suppose that the applied mathematician
> whose theory has just passed still one more stringent test
> by observation ought not to feel satisfaction, but rather
> disappointement - 'Foiled again, This time I <u>had</u> hoped
> to find a discordance which would throw light on the points
> where my model could be improved'.   Perhaps this is a
> counsel of perfection; I own that I have never felt very
> keenly a disappointment of this kind". (Bahcall, 1967:249).

Bahcall had not put all the hard work into his prediction in the

expectation that it would be refuted. To reiterate an earlier

quote from Bahcall:

> I had staked my career on my ability to predict the response
> of the instrument...and he [Davis] in spending his major,
> almost his entire effort in building the equipment.

The drama that was unfolding as Davis prepared to make his first

measurement is clear. A lot of time, effort and human resources

had, by this stage, been invested in the project.   The partnership

between the theoretician and the experimenter was entering its most

crucial phase. In a sense, it was Davis's and Bahcall's scientific
careers which were at stake, as much as the flux of solar
neutrinos.

NOTES FOR CHAPTER SIX

1. This value is quoted by Bahcall: see letter,J. Bahcall to R. Davis, December 3, 1964. Unfortunately none of the detailed calculations of the above revisions appeared in the correspondence. Thus it is difficult to assess the why and wherefore of each of these revisions.

2. See T.A. Tombrello, 'The Capture of Protons by Be^7', Nuclear Physics, 71, 1965, 459-64.

3. See R.F. Christy and I. Duck, 'γ Rays from an Extra Nuclear Direct Capture Process', Nuclear Physics, 24, 1961, 89-101.

4. This value can be found in J.N. Bahcall, 'Observational Neutrino Astronomy: A ν-Review', in K.N. Douglas, I. Robinson A. Schild, E.L. Schuckling, J.A. Wheeler and N.J. Woolf, Quasars and High Energy Astronomy, New York: Gordan and Breach, 1964, p. 322. This article is an early version of Bahcall (1965).

5. Letter, J. Bahcall to M. Goldhaber and G. Scharff-Goldhaber, November 17, 1964.

6. Letter, R. Davis to J. Bahcall, December 3, 1964.

7. Letter, W. Fowler to R.W. Dodson, July 31, 1964.

8. P.D. Parker, 'Termination of the Proton-Proton Chain via the $Be^7(p,\gamma)B^8$ Reaction' The Astrophysical Journal, 145, 1966, 960-961; and P.D. Parker, '$Be^7(p,\gamma)B^8$ Reaction', Physical Review, 150, 1966, 851-856.

9. Letter, R. Davis to J. Bahcall, January 21, 1966.

10. D. Ezer and A. Cameron, 'A Study of Solar Evolution', Canadian Journal of Physics, 43, 1965, 1497-1517.

11. Letter, R. Davis to J. Bahcall, April 2, 1965.

12. Ibid.

13. Letter, J. Bahcall to R. Sears, April 6, 1965.

14. Letter, R. Sears to J. Bahcall, April 8, 1965.

15. Ibid.

16. Letter, J. Faulkner to R. Sears, May 14, 1965.

17. Letter, R. Sears to J. Bahcall and J. Faulkner, May 18, 1965.

18. Letter, J. Bahcall to R. Davis, May 11, 1965.

19. In Parker's published papers (see note 8), S_{17} is given as 0.043 keV-barns. Provisional analysis of his experiments, however, seemed to indicate S_{17}=0.035 keV-barns.

20. These values can be found in: Letter, R. Davis, to C.D. Finney, March 11, 1966.

21. Brans and Dicke had proposed a scalar-tensor form of relativity theory; see C. Brans and R.H. Dicke, 'Mach's Principle and a Relativistic Theory of Gravitation', Physical Review, 124, 1961, 925-35.

22. These values are given in a letter, R. Davis to J. Bahcall, April 2, 1965. For further details see, D. Ezer and A. Cameron, 'Solar Evolution with Varying G', Canadian Journal of Physics, 44, 1966, 593-615.

23. Letter, R. Davis to J. Bahcall, January 21, 1964.

24. 'Neutrino Trap', 'Science and the Citizen', Scientific American, 212, February 1965, p. 53.

25. 'Erratum', Scientific American, 212, April 1965, p. 8.

26. Brookhaven National Laboratory, internal memorandum, R. Davis to R.W. Dodson, April 15, 1966.

27. Letter, R.W. Dodson to J. Bahcall, April 25, 1966.

28. Letter, R. Christian Anderson to J. Bahcall, May 25, 1966 (Anderson was Assistant Director of BNL).

29. Op. cit., note 27.

30. R. Weymann and R.L. Sears, 'The Depth of the Convective Envelope on the Lower Main Sequence and the Depletion of Lithium', The Astrophysical Journal, 142, 1965, 174-81.

31. This value is quoted in a letter, R. Sears to J. Bahcall, June 27, 1966.

32. Ibid.

33. This letter is the source for Sears's opinion on the best way to average predictions mentioned in Chapter 4.

34. (Bahcall 1966) also contains a discussion of the $He^3 + H^1 \rightarrow He^4 + e^+ + \nu$ reaction which was generally thought to play a negligible role in the Sun.

35. The first mention in the correspondence of the new value for this cross-section is in a letter, R. Davis to D. Trümper, December 19, 1966.

36. The Bacher-Tombrello results are discussed in T.A. Tombrello, 'Astrophysical Problems', in J.B. Marian and D.M. Van Patler (eds.), Nuclear Research with Low Energy Accelerators, New York: Academic Press, 1967, 195-212.

37. H.C. Winkler and M.R. Dwarakanath, 'He^3 + He^3 → He^4 + 2p Total Cross Section at Low Energies', _Bulletin of the American Physical Society_, 12, 1967, p. 16.

38. W.Neng-ming, V.N. Novatskiĭ, G.M. Osetinskiĭ, Chien Nai-Kung, and I.A. Chepurchenko, 'Investigation of the Reaction He^3 + He^3', _Soviet Journal of Nuclear Physics_, 3, 1966, 777-781.

39. The difficulties in this extrapolation are discussed by Tombrello, op. cit., note 36.

40. The later measurements confirmed a value of ∿5,000 keV-barns. See, M.R. Dwarakanath and H. Winkler, '^{3}He(^{3}He,2p)^{4}He Total Cross Section Measurements below the Coulomb Barrier', _Physical Review_, C, 4, 1971, 1532-40.

41. G. Shaviv, J.N. Bahcall and W.A. Fowler, 'Dependence of the ^{8}B Solar Neutrino Flux on the Rate of Reaction ^{3}He(^{3}He,2p)^{4}He', _The Astrophysical Journal_, 150, 1967, 723-4.

42. This refers to the beam spreading out through the target.

43. J.N. Bahcall, M. Cooper and P. Demarque, 'Dependence of the ^{8}B Solar Neutrino Flux on Heavy Element Composition', _The Astrophysical Journal_, 150, 1967, 723-4.

44. Letter, D. Ezer to R. Davis, May 2, 1967.

45. Letter, J. Bahcall to R. Davis, January 4, 1967.

46. Bound-electron capture is the possibility that occurs if Be^7 exists as an atom rather than as a nucleus, and has one or two bound K-shell electrons. The capture rate is increased when these electrons are taken into account. As the electron-capture competes with the proton-capture, any increase in this rate must decrease the proton-capture rate and hence the flux of solar neutrinos.

47. For the source of this prediction see Chapter 8.

48. One bet, made on February 13, 1967, was with Fowler. Bahcall offered to pay Fowler one US dollar if the signal Davis detected did not lie between 3SNU and 300 SNU. He also had a similar bet with another colleague, Jon Mathew. The jokiness surrounding such bets was nicely summed up for me by Davis who pointed out, that, whilst the theoreticians were prepared to risk one dollar, he was risking six hundred thousand dollars!

CHAPTER SEVEN

EXPERIMENTAL DEVELOPMENTS   1967-1978

In this chapter, the description of experimental developments

is continued from the point in 1967, when the first results of

Davis's experiment became available, until the point in 1978 when

his experiment met with widespread acclaim. As well as looking

at Davis's activities over this period, the reception accorded

his result amongst the wider community is mapped out.   Also the

development of 'second generation' solar-neutrino experiments is

briefly discussed.

Given the scope of this chapter it is convenient to divide

the material into two parts.   In Part I, the focus is on the

central experimental developments over the period and the general

reception accorded Davis's results. In Part II, the one serious

attempt to challenge Davis's result is looked at in some detail.

Again, in this chapter, descriptive material is well to the

fore. The account of activities is based upon correspondence

files, interview material, and scientific publications. Another

important resource is the recent account of the history of the

solar-neutrino problem written by Bahcall and Davis (1980).

The material presented (particularly in Part II) has also been

chosen, however, so as to illustrate an important sociological

theme. This relates to one of the central contentions of this

work - that scientific knowledge (and in this case the outcome

of the Davis experiment) can be looked at as a product of the

social world rather than the natural world.

It was argued in Chapter 1, that in order to show the social

basis of scientific knowledge the sociologist is faced with two

tasks. The first task is to deconstruct scientific facts in such

a way as to show that they are not necessary outcomes of the

natural world. Along with this task of deconstruction, is the

need to show the social processes of construction whereby a

socially contingent fact becomes widely accepted as a certified

scientific fact. In other words, the sociologist must also show

the social processes of construction such that a consensus is

reached that a valid fact of the natural world has been established.

In this chapter these tasks are accomplished by looking at

the reception accorded Davis's results amongst the wider community.

Attention is focussed initially on the issue of replication. Davis's

result is further deconstructed by following the attempts of one

scientist to claim that Davis's result was merely an artefact of

his detection procedure. The attempt is made to show that

scientific arguments alone cannot settle the issue of whether

Davis's result is fact or artefact. Furthermore, by monitoring

the acceptance of Davis's result amongst the wider community

and the eventual defeat of the efforts to challenge the

result, it is to be hoped that some of the processes of knowledge

construction can be revealed. The processes which are illustrated

by the material presented in this chapter will be discussed again,

in more detail, in Chapter 10.

PART I: DAVIS'S ACTIVITIES AND THE RECEPTION OF HIS RESULT, 1967-1978

The First Result

As mentioned in Chapter 5, the first sample recovered from

the Davis experiment that was suitable for analysis was taken to

Brookhaven in June 1967 after the tank had been exposed for 48

days. The radioactivity produced by the sample was measured in

a small Geiger counter, surrounded by anticoincidence cosmic-

ray counters in a heavily-shielded area of the chemistry

department basement.   The sample was counted over the period

June 26 - August 7, 1967.   The 1/10 cc of argon thirty-six

carrier gas,[1] which had been placed in the tank prior to exposure,

was found to be recovered with a 94% efficiency (the amount of

carrier gas recovered was determined by mass-spectrometer

measurements).   Thus, if any argon thirty-seven formed, it too

was expected to be recovered with a high efficiency.

There was by this stage considerable excitement in the

scientific world as the long-expected result was awaited.   Fowler,

who was at Cambridge at the time, wrote to Davis on August 10,

that:[2]

> There are the usual batch of rumours concerning the preliminary
> results of your observations and I have been besieged with
> questions on all sides...If you are...divulging your
> preliminary results to others I would very much like to ...
> be in the know.

At least one person, apart from Davis, knew already how the

counting was progressing. This was John Bahcall, who, as we saw

in Chapter 6, was keenly following Davis's progress and who had

had several telephone conversations with him over this period.[3]

Bahcall knew of the result almost as soon as Davis himself did.

The first written communication of the initial result seems

to have been in a letter which Davis sent to Fowler on August 11.[4]

This letter contained the counting data for the first run. The

pulse-height spectrum for the sample (the histogram of counts in

a range of energy bins) showed no evidence for an excess of

argon thirty-seven in the expected energy range. In other words,

it seemed as if there was no signal above the background. The

background rate was estimated by observing the counter for a

period before the sample was introduced (in later runs the

background was measured by observing the counter for a period

after all the sample should have decayed). The Ar^{37}-production

rate in the tank could be calculated from the signal observed

in the counter after allowance had been made for the exposure

period, the counting period, the recovery efficiency and the

counter efficiency.

As in this first run there was no evidence for argon thirty-

seven decay above the background rate, the result could only

be expressed as an upper limit. This allowed for the possibility

that the Ar^{37} counts were produced entirely by background effects.

Davis estimated a result of $\Sigma\phi\sigma$ less or equal to 6 SNU (equiva-

lent to a boron-eight flux of $\phi_B 8 \lesssim 0.5 \times 10^7$ neutrinos $cm^{-2} sec^{-1}$).

It will be recalled from Chapter 6, that the theoretical

prediction which Bahcall and Shaviv had made on the eve of the

experiment gave a total rate of $\Sigma\phi\sigma = (19 \pm 11)$ SNU and a boron-

eight flux of $\phi_B 8 = 1.4 (1 \pm 0.6) \times 10^7$ neutrinos $cm^{-2} sec^{-1}$.

Davis's first result thus seemed to be well below the 'best

value' for the theoretical prediction and on the border lines

of the lower limit of this prediction. Davis commented to

Fowler that this result seemed 'quite low'.[5]

The result was, at this stage, highly provisional, as Davis emphasized to Fowler:[6]

> Please regard these results as very preliminary. There are several points that must be checked before we are certain this is a bona fide observation.

That Davis should be initially cautious is hardly surprising given the importance of the experiment. Davis's worries were also amplified by the particular context in which the experiment was being performed. The location of the apparatus, a mile under the Earth's surface in the backwoods of South Dakota, did not facilitate inspection of his procedures by interested parties. Also, he was the only experimental scientist working on the project. His collaborators, Don Harmer and Ken Hoffman, who had helped design and build the apparatus, were both engineers, and Bahcall was, of course, a theoretician (there was also a technician, John Galvin, who had worked with Davis throughout the entire project).

One of the first things Davis did, after reporting his initial result to Fowler, was to get two of his immediate colleagues at Brookhaven (both of whom were nuclear chemists) to visit the mine with him and check over his experimental procedure.[7] After this careful scrutiny by his colleagues Davis felt confident that there was nothing obviously wrong. At this point he decided to make his preliminary result public.

He presented his result to a meeting of the American Chemical Society in Chicago on September 14, 1967.[8] Davis chose this forum because there was a drive on at Brookhaven at the time to get people to join the American Chemical Society and participate in its activities. Davis got very little reaction

to his paper which concentrated on details of the design and

operation of the experiment; as he told me:

> I just gave a regular contributed paper, and I gave it to
> an audience of about six or eight chemists sat in this
> little room...and I mean they didn't care less [laughter].

It seems that one of the most important results in nuclear

astrophysics could hardly have been presented in less exciting

circumstances.

The result Davis reported ($\phi_B 8 < 0.9 \times 10^7$ neutrinos $cm^{-2} sec^{-1}$)

was slightly larger than the upper limit given in the previous

letter to Fowler. It seems Davis's initial analysis had been

somewhat pessimistic. In the intervening period his upper

limit had risen slightly.

Davis's decision to present his result initially to his

immediate professional colleagues in chemistry was understandable

since his experimental technique was first and foremost a piece

of chemistry. The lack of reaction of his colleagues gives some

idea as to just how standard a piece of chemistry it was considered

to be. His work was similar (although on a larger scale)to that

carried out by other radiochemists in the identification of

elements from the decays of just a few atoms. Davis, as an

acknowledged expert in this technique for argon thirty-seven

(an expertise gained over his previous fifteen years in the field),

was unlikely to meet much criticism from his colleagues in chemistry.[9]

In that his result was of little consequence for chemistry per se,

there was little motivation for chemists to give his results

the close scrutiny which, as we shall see below, some astro-

physicists felt was necessary.

The Second Result

Although his first result did not show any evidence for
argon thirty-seven activity and hence the presence of solar
neutrinos, Davis, it seems, remained optimistic. He wrote to
the Homestake Mining Company on September 23 that:[10]

> We will improve our sensitivity in future experiments,
> so we are still hopeful.

The exposure for the first run had been 48 days. By exposing
the tank for 110 days, Davis hoped the sensitivity would be
increased somewhat because there would be more opportunity for
argon thirty-seven to form. On October 9 a new sample was taken
from the tank for analysis.

This second measurement, which would hopefully clarify
the situation, was eagerly awaited not least because the scientific
media, having heard of the first measurement, were anxious to
publicise the result. During October Davis had requests for
information from the British magazine, New Scientist, and the
house journal of the American Physical Society, Physics Today.

The results of the second measurement were communicated by
Davis to Bahcall on October 31, 1967.[11] The argon thirty-six
recovery efficiency was again found to be satisfactory (95%).
After an exposure of 113 days and counting for 13 days, Davis
estimated the total signal was < 4 SNU and that $\phi_B 8 < 0.3 \times 10^7$
neutrinos $cm^{-2} sec^{-1}$. This corresponded to a capture rate of
< 0.8 neutrinos/day. Again, the pulse-height spectrum for the
sample looked little different from the spectrum for background
alone, thus indicating that no excess argon thirty-seven was being
formed.

Davis described the result as 'very low'. It seemed that the

expected neutrino flux was not there.   The negative result

caused Davis to comment in his letter:[12]

> It's too bad we do not have a positive result for Bethe
> this year.

By coincidence Hans Bethe had that year been awarded the Nobel

prize for his earlier work on the nuclear reactions in stars.

It was indeed ironic that the first direct test of his theories

of nuclear synthesis in stars, should be made in the same year

and, furthermore, indicate that something was amiss.

Although Davis appeared not to be detecting solar neutrinos,

there was some small hope that in the future he might be able to

increase the sensitivity of his experiment. This could be

achieved by the use of smaller counters, which would reduce

background effects at the counting stage, and by the accumulation

of more data and hence better statistics.   However, he was near

to the absolute sensitivity of the apparatus which was governed

by the backgrounds in the tank. He estimated there to be a

background of 0.2 Ar^{37} atoms/day formed by cosmic rays, 0.6

Ar^{37} atoms/day formed by fast neutrons, and 0.02 atoms/day

formed by internal alpha contamination (these figures should be

compared with his detection rate of ≤ 0.8 Ar^{37} events/day).

The fast-neutron effect could eventually be eliminated by

flooding the tank chamber with water - Davis planned to do this

at a later date.   He also planned to obtain a more accurate

estimate of the cosmic-ray background by exposing tank cars

of perchloroethylene at various depths in the mine.

One other test Davis hoped to perform was to irradiate the

tank with a neutron source. This would produce argon thirty-seven

by the reactions, $Cl^{35} + n \rightarrow S^{35} + p$, $Cl^{37} + p \rightarrow Ar^{37} + n$. This test would serve as a calibration of the apparatus, because the number of argon thirty-seven atoms produced, although not known absolutely, could be shown to scale-up proportionately with similar tests carried out with smaller tanks.

Davis was very concerned to make every test possible to check on the operation of his experiment. This can be seen in his request to Bahcall that:[13]

> If there are any other tests you or your colleagues can think of that we could perform, please let me know.

The need to convince the Caltech group, in particular, of the correctness of his result was uppermost in Davis's mind at the time. As he told me:

> I was very concerned about Willy Fowler and Caltech and I always welcomed someone from their lab coming to visit the experiment . You see he has a lot invested in this... So it meant quite a lot to him and I wanted to be thoroughly satisfied that we were doing things right. I've had lots of discussions with Willy from the beginning and they continued to this day.

In a sense the Caltech group formed the most important audience for Davis's result. As we have seen, it was their interest and involvement which was largely responsible for Davis getting the funding for his apparatus. It was this group that Davis was most concerned should be convinced of the correctness of his experiment - especially as his experiment seemed to disagree with their theory.

As we shall see later, one of the things that has most impressed the nuclear astrophysicists about Davis's experiment has been the fact that he has been prepared to listen to all their suggestions and, where possible, make checks on his procedures. Thus, Davis's deliberate policy of openness which

started with his offer to Bahcall does seem to have paid dividends

in the long run.

By early December 1967, Davis had further refined the

analysis of his second run. After an additional period of counting

he reached the conclusion that $\Sigma\phi\sigma < 0.3$ x 10^{-35} sec^{-1} (3 SNU)

and that $\phi_B 8 < 2$ x 10^{-6} neutrinos cm^{-2}sec^{-1}. The neutron-source

test had by this stage also been completed successfully - the

correct number of argon thirty-seven atoms having been recovered.

Davis, in his letter to Bahcall reporting these results, wrote:[14]

> I am quite convinced the experiment is giving a correct
> result, and that I shall publish these results fairly
> soon.

Thus, in December 1967, as far as Davis was concerned, he

was convinced of the validity of his result and was now ready

to formally publish it , (his earlier presentation to the American

Chemical Society was not the same as a scientific publication).

He did this by means of a paper in the May 20, 1968, issue of

Physical Review Letters (Davis, Harmer and Hoffman, 1968).

His reported result, an upper limit of 3 SNU, meant that he had

thus far failed to detect any solar neutrinos.   From the

experimental viewpoint it seemed that his attempt to extend

his neutrino-detection programme into astronomy had met with

little success. Here too, as with his earlier work at reactors,

he had found nothing to detect.

The Response of Other Solar-Neutrino Experimenters to Davis's

Result

It seems that other solar-neutrino experimenters were soon

convinced of the validity of the Davis result. By early December

1967, they had enough faith in the correctness of his experiment

to be prepared to give up their own experiments.   As Davis

commented to Bahcall, in his letter of December 8:[15]

> I have discussed the present results with Fred Reines
> and Tom Jenkins [the other experimenters - see Chapter 5]
> They seemed inclined to discontinue their experiments
> particularly if they have difficulty pushing their
> backgrounds down.

It will be recalled from Chapter 5, that around this period
there were three other experiments built to detect solar neutrinos.
Two of these were carried out by Reines and co-workers - the
other one being performed by Jenkins. Reines's experiment to
detect solar neutrinos by elastic scattering of electrons
actually came into operation in a mine in S. Africa at about
the same time as the Davis experiment came on the air. The
Reines experiment was, however, dogged by a much larger back-
ground (thought to be caused by gamma radiation) than had been
expected. In order to get down to the sensitivity Davis had
already achieved, a new lead shield would have to be added to
the apparatus. Reines's attitude to this prospect was recalled
for me by Davis, who remembered this conversation with Reines
particularly well as it had been carried out in unusual circum-
stances. Davis spoke on the telephone to Reines's secretary
(in California) who then sent Davis's comments on to Reines
(who was in S. Africa) by telex. Davis told me:

> So they were wondering what to do about that, should
> they abandon the experiment or should they do a lot
> more work to build a shield...and I think that another
> thing that was involved was that the mining company let
> them have a certain tunnel for a certain period of time
> and then the mining company wanted to use that tunnel.
> So they were going to provide them with another tunnel...
> And the question is, is it worth moving the apparatus?...
> So he would type out a little question... That was the
> way of our conversation of what our results were...and
> he finally did abandon the experiment.

With Davis already reporting a negative result at a sensitivity

below that which he could easily achieve, Reines felt it was

not worthwhile continuing the experiment.

Reines's other detector - built in association with Wood,

and based on the interaction of neutrinos with lithium seven -

was never put into operation, although the apparatus was completed.

The experiment had been designed to look for a much larger flux

than seemed likely in autumn 1967. (It should be recalled that

when these detectors were designed Bahcall was predicting a

flux of 40 SNU - Davis's latest upper limit was 3 SNU and

the theoretical limit had also fallen to 6 SNU by this time -

see next chapter).

The amount of confidence Reines attached to Davis's work

can be seen from the fact that he was prepared to abandon his

observational programme at this point and not even put into

operation a detector that was already built.[16]   Given the un-

expected nature of Davis's result, it might seem surprising

that Reines did not pursue his programme further in order to at

least provide some sort of check on Davis's result. However,

Reines had every reason to trust Davis. It must be remembered

that Reines and Davis, as the two pioneers of neutrino detection,

were well acquainted with each others' work. They were also

good friends.   Reines has always had a high opinion of Davis's

experimental abilities and he described the Davis experiment

to me as having been performed with 'exquisite care, thought-

fulness and humility'. Furthermore, Davis's earlier neutrino-

detection results at Savannah River, although negative ones,

were consistent with the Reines-Cowan positive result. Finally,

it would be wrong to assume that experimentalists necessarily

feel compelled to question a result which conflicts with

theoretical expectations.   For Reines,the lack of fit

with the theory gave no special reason for doubting the merits

of the Davis experiment.

Jenkins discontinued his experiment for much the same

reasons.[17] As mentioned in Chapter 5, he was already encountering

a large background and again, like Reines's experiments, his

had been designed to detect a much larger flux. Davis's low

result was the final blow which made him give up.

It seems that Davis's fellow neutrino experimentalists had

no real doubts about his competence. Indeed, as one of the

pioneers of what was generally considered to be a difficult

and demanding (some would say hopeless) enterprise, Davis was

much admired. Naturally other experimentalists were disappointed

that he had not detected any solar neutrinos because this meant

that the field as a whole would not take off as had other new

areas of astronomy.[18] The attitude of the other neutrino experi-

mentalists is reflected in remarks made by Arnold Wolfendale

(a cosmic-ray neutrino specialist). When he learnt of Davis's

result he wrote to Davis:[19]

> I was most surprised to hear that you had not had any
> neutrino counts and that the neutrino flux is much less
> than had been predicted. How very disappointing...
> I admire your courage in continuing the search
> despite the background problems...

For the neutrino experimentalists Davis's experimental abilities

were never in doubt and the main consequence of his result for

them meant that it had made a difficult enterprise even more

difficult.

The Response of Theorists to Davis's Result

As mentioned above, Davis, although convinced of the validity

of the result himself, was apprehensive as to how the theorists,

and in particular the Caltech group, would greet his work. After

all, they were hardly expecting Davis to find a negative result

since the whole thrust of their work had been to argue, that

solar neutrinos could be detected. In general, their attitude

has been to accept the result - but not until they had given

Davis's work very careful scrutiny.[20]

The attitude of Bahcall to the first results was described

to me by Davis:

> When we got a low result of course he [Bahcall] knew about
> it right away because we talked on the phone.   And I
> remember going to Caltech and giving a seminar on the
> result. I remember going to John Bahcall's house, Willy
> came over and we were all talking about these things...
> And we discussed it and he used to argue about why was
> I sure, and what are the arguments and how did I know
> that.   Sort of what John Bahcall used to refer to as the
> nuts and bolts of the whole thing.

And, on another occasion, Davis informed me:

> I remember when I first got this result, John Bahcall
> was very upset that we didn't see what we should.   I
> remember being there at Caltech for a couple of days,
> discussing 'How do you know that you did that?   How do
> you know you got the argon out?   How do you know what
> the yield is?' and so on.

I asked Bahcall if he could recall his immediate reaction

to Davis's results. He told me that he could not remember but

he felt that it would not have been unreasonable for him 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 to have
> wondered about that at the time...If the experiment had
> proved to be much less sensitive for reasons that 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 very much further out of
the stream than I was. So it would have been reasonable
for me to worry about that, certainly before all the
subsequent checks were made.

From piecing together other accounts, it seems that Bahcall

did have the 'good sense' to be worried. As Davis mentioned

above, Bahcall went over his work with a fine tooth comb.

Bahcall himself, in the context of relating another story

concerning his relationship with Davis, described in passing

the sort of examination he gave Davis's results:

He [Davis] came to Caltech to talk to me...and he wanted
to discuss a variety of things, including his latest
experimental results, and he spent a day and a half with
me...And I talked to him so forcefully and at such length
about analysing his data statistically in detail, he
wasn't so much interested in that, he got a headache.
And my wife told me, I had better lay off him!

That Bahcall still had worries in early 1968 can be seen

from the correspondence between him and Davis as Davis prepared

his <u>Physical Review Letters</u> paper. The publication format was

to be the same as that which they had used in 1964, when they

first proposed the experiment - that is an experimental paper

by Davis published 'back to back' with a theoretical paper by

Bahcall and his associates. As in 1964, preprints of the two

papers were exchanged between Brookhaven and Caltech. In a

letter which accompanied his preprint, Davis wrote:[21]

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

This refers to the possibility that argon thirty-seven was formed

in the tank but was not properly extracted because it binded

with something in the tank. Most chemists thought that this was

very unlikely as the chemistry of argon was considered to be

very simple; however, a number of theoretical physicists,

including Bahcall, have been worried about this possibility,

as we shall see below. Davis attempted to allay Bahcall's

fears on this score by discussing the trapping possibility

(and rejecting it) in his paper.

After sending his paper to Bahcall, Davis visited Caltech

the following weekend to discuss it. Bahcall then sent a letter

to Davis on the Monday, in which he expressed further worries

about the experiment. The letter commenced:[22]

> I have redone the calculations we looked at together last
> Saturday. I am more convinced now, than then, 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 in the counter for the most recent run

was really lower than Davis measured it to be, and hence that

the signal in the counter from Ar^{37} was in reality, larger

because too many background counts were being subtracted. The

basis for this argument was a difference between the background

rate recorded when no sample was present in the counter and

the rate recorded when the sample was present, but after a

sufficiently long enough period for it to have all decayed - thus

in theory, leaving just background counts. There was some

debate as to which of these two background rates was the better

one to take.[23]

This worry was, it seems, eventually answered by Davis to

Bahcall's satisfaction as Davis made more experimental runs and

managed to produce counters with self-consistent background rates.[24]

However, it seems that for a while Bahcall (and Fowler) both had

hopes that Davis's negative result would turn into a small

positive signal because of his over-estimation of the background

contribution in the counter.[25]

Part of Bahcall's concern over this point was also reflected

in his questioning of Davis's presentation of his result as an

upper-limit.   It seemed to Bahcall to be important to assess

the probability of the experimental value exceeding the upper

limit. This issue had been raised by Richard Feynman in con-

versation with Bahcall.   As Bahcall wrote Davis:[26]

> I have been talking to Feynman about your experiment...
> He questioned...your method of estimating $\Sigma\phi\sigma$ upper
> limit. He pointed out that it would have been very
> different to have seen the difference between what
> you actually saw and a $\Sigma\phi\sigma$ of say 0.5×10^{-35} sec^{-1}.
> It is clear...that you could tell the difference
> between what you saw and 2.4×10^{-35} sec^{-1} ...

A similar point was raised by Bahcall in a later letter concerning

Davis's proposed paper for <u>Physical Review Letters</u>.   Bahcall

wrote:[27]

> ...<u>you should say explicitly what the probability is
> that the actual rate is above any limit you set.</u> (His
> emphasis).

This point warranted particular attention in Bahcall's mind at

the time because, as we shall see in the next chapter, he was

hopeful that the theoretical 'prediction' was actually lower

than had been thought to be when the experiment had been under-

taken.   It was not clear to Bahcall that the results did contradict

the theory.   Clearly if the upper limit of the experimental

result was seen as a statistical limit with a finite probability

of being transgressed, then a contradiction with a theoretical

prediction (say 0.5×10^{-35} sec^{-1}) just above this upper limit

would appear less compelling.[28]

It can be seen from the above interchanges that Bahcall, anyway, was not convinced of the validity of Davis's results quite as quickly as Davis himself and the other experimenters had been. In this regard it is important to note that the publication format of separate papers that Bahcall and Davis adopted permitted Bahcall to map out the theoretical implications of the experiment without actually having to commit himself to the validity of the experimental results. By the time Bahcall and Davis published jointly-authored papers on the result (the first was in 1976)[29] Bahcall was confident that the experiment was working correctly.

Other theorists seemed willing to express more confidence in Davis's work in 1968, especially after they had had a chance to read his Physical Review Letters paper. The seriousness with which Davis's work was being taken can be seen from the following two comments of theorists. The first is made by solar-model specialist, R. Sears. After receiving a preprint of Davis's paper, Sears wrote:[30]

> Thank you very much for your preprint...I am most grateful to get the official word on your data, after hearing only gossip from Fowler, Hoyle, Dicke, Cameron, and Physics Today!
>   The results of your elegant and careful work are certainly exciting...
>   I certainly hope you will continue your beautiful experiment.....

The second comment comes in a telegram sent to Davis by the highly influential Princeton theoretical group centred around Robert Dicke and John Wheeler. The telegram, sent by Robert Dicke, Donald Morton, James S. Mark, Remo Ruffini, and John Wheeler, reads:[31]

> To (sic) bad theoreticians encouraged you to undertake such a difficult experiment but now you have done it and

> shown we were all wrong we all consider it one of the most
> challenging problems in present day physics...Congratulations
> on what you have done and best wishes for the future.

It seems as if Davis had, by this stage, impressed several important

theorists with his work.[32]

Summer 1968 - Davis Decides What to do Next

With the publication of his first results, Davis settled

down to the more mundane tasks of trying to improve the sensitivity

of his apparatus by making more and better measurements, and

of trying to get a more accurate estimate of the cosmic-ray

background. Davis was hoping to enlist Wolfendale's assistance

in analysing the measurements he was making of the cosmic-ray

background at different depths. In a letter to Wolfendale[33],

sent in early May, Davis reported that he had completed measure-

ments at a depth of 800 feet; he estimated, however, that it

would be two years before enough measurements had been made to

calculate the background accurately. It seemed that any future

progress on the experiment would be slow and painstaking.

At the end of July 1968 a further sample was taken from the

tank and counting commenced. It soon became clear that the

result was similar to the earlier ones and confirmed the

previous upper limit of 3 SNU.

Davis spent the summer of 1968 visiting Reines at Irvine.

At the end of the summer he presented his latest results at an

international conference on solar-neutrino astronomy held

in Moscow and organised by the Soviet physicist, George Zatsepin.[34]

Reines and Wolfendale also attended this meeting. Bahcall was

invited but was unable to attend as he had just become a father.

The Moscow meeting, as well as providing a forum in which

to discuss the Davis result, was evidence of the continuing Soviet

interest in solar-neutrino astronomy. Of course, the original

idea for the chlorine experiment came from a Soviet physicist -

Bruno Pontecorvo. As well as contributing to theoretical

developments, the Soviets were following experimental develop-

ments closly.[35] Many young Soviet physicists quizzed Davis

in some detail at this meeting concerning his experimental

procedures.[36] Eventually the Soviets were to start work on

building their own chlorine detector. If this experiment is

ever completed (it had not been by 1978) it will represent the

only attempt thus far to repeat the Davis experiment.[37]

Soon after returning to Brookhaven, Davis sent Fowler a

copy of the paper he had read at Moscow. He informed him that

he did not think he would be able to improve on his results unless

he could reduce his counter background to zero. Davis also

sent Fowler a copy of the Dicke telegram (mentioned above) which

he had just received. It will be recalled that Dicke's message

opened with the cryptic remark:[38]

> To bad theoreticians encouraged you to undertake such
> a difficult experiment...

Davis obviously thought that the telegram warranted some comment

as Fowler was the head of the group that had encouraged him

to perform the experiment. He summarised his feelings as follows:[39]

> It is very clear that we have learned a lot from this
> experiment, and I am very happy that we forged ahead
> in 1964. I feel that we can get more out of the
> experiment in the future, though it may be slow going.

Davis certainly would not admit that the experiment was a failure.

After all it had worked as planned - it was just that the flux

he had hoped to detect did not seem to be there. There is no

doubt, however, that Davis felt some disappointment. As Bahcall,

who probably knew Davis as well as anyone over this period, told

me:

> Davis's reaction was even sharper than mine...because he
> had spent many years measuring nothing at a reactor.
> And the thing we were most worried about...in funding
> it, was that it was just a pipe dream of astronomers
> and they'd measure nothing anyway.

In view of the overall goal of his research programme - to

detect neutrinos - Davis now had to consider what to do next. As

he wrote to Reines at the time:[40]

> I have reached a plateau with the chlorine experiment and
> must consider some new approaches to solar neutrinos,
> or perhaps start out in a new direction. There is much
> more that has to be done to finish the Homestake
> experiment, but this is on the ways, and I can see the
> end ahead.

It appears that Davis used the opportunity of the summer spent

at Irvine and the Moscow meeting to reflect on the prospects

for the future. It seemed that a new approach would be required.

After much discussion with Reines, Davis concluded that the

most promising new detector would be one based on the reaction

$Li^7 + \nu \rightarrow Be^7 + e^-$. This was the same reaction Reines and

Wood had earlier planned to use in their detector. However,

Davis planned (possibly as a joint project with Reines) a large

radiochemical version with the lithium in the form of lithium-

chloride solution. The Be^7 could be separated from the solution

and its radioactive decay monitored in much the same way as

the Ar^{37} was observed in the chlorine experiment. The great

advantage of this detector was that it was likely to be sensitive

to lower energy fluxes of neutrinos, such as the pep neutrinos,

the flux of which had been shown to be independent of detailed

solar models (Bahcall, Bahcall and Shaviv, 1968; see next

chapter for details).

It can be seen that, by the end of 1968, with three experimental runs reported, as far as Davis was concerned his experiment was substantially complete. He was convinced of the correctness of his result, as were his fellow experimentalists. Even the theoreticians seemed to be taking his result seriously. Although modifications and further work could be carried out to tighten up on and improve the result (e.g., flooding the tank chamber, making more runs, measuring the cosmic-ray background, and building new counters and electronics), the main 'news' was in. The detection of solar neutrinos - the long term goal of Davis's research - seemed unlikely to be fulfilled with the chlorine experiment.

1968-1972, Davis Sets an Even Lower Limit on the Solar-Neutrino Flux

Although Davis started serious work on planning a lithium detector, the main developments of interest in this period were still associated with the chlorine experiment. In particular, Davis achieved a dramatic increase in sensitivity with the introduction of a new technique for measuring the Ar^{37} decay counts.

The new technique was a means of measuring the pulse-rise time of decay counts. The pulse in the Geiger counter produced by argon thirty-seven has a faster rise time than background events (this is because of the very short range of the argon thirty-seven Auger electrons). If the pulse-rise time could be measured, the signal from argon thirty-seven events could be distinguished from the background more easily. This technique was in use in X-ray astronomy where it enabled x-ray events to be

detected in the presence of large fluxes of cosmic rays. The idea
of using this system for the chlorine detector had first been
suggested to Davis in 1968 by a Caltech x-ray astronomer, Gordon
Garmire. Davis was given the idea by Garmire as they relaxed
at the Caltech pool after a seminar presentation by Davis. It
took the BNL electronic engineers two years to develop an
amplifier fast enough for Davis's needs, and the pulse-rise
facility was not finally introduced until late 1970.

The increased sensitivity brought about by the introduction
of this new counting technique gave Davis his first indication
of a positive signal. In reports of his results in 1970 and
1971 (Davis, 1970, 1971), he tentatively concluded he was seeing
a signal of 1.5 ± 1 SNU. Because of the uncertainty in the
background Davis was reluctant to conclude that this signal was
produced by solar neutrinos.   In late 1971 he obtained a series
of very low counts (one actually being zero) and this led him
to reaffirm an upper limit - the new upper limit (Davis, 1972)
was given as 1 SNU. With this very low signal Davis finally felt
it was worthwhile to flood the tank chamber with water in order
to shield the detector from fast-neutron background effects.
He did not expect the water shield to have much effect since
measurements of this background by other means indicated a
negligible contribution.[41]   The first runs with the water shield
in place were reported in early 1972.

Throughout this period Davis continued his measurements of
the cosmic-ray background. It was not possible to make measurements
at very deep locations (because of the size of tank needed) so
the cosmic-ray effect at depths equivalent to the location of his

detector was extrapolated from a series of measurements made at
lesser depths. By 1972 this analysis had been completed by
Wolfendale and the background in the tank calculated (O.24 Ar^{37}
atoms/day - Wolfendale, Young and Davis, 1972). This value was
close to that which Davis himself had estimated earlier .

It was clear by this stage, with the experimental improve-
ments and with the completion of the background measurements,
that not only was Davis reporting a signal well below that
expected from theory, but also there might not be any neutrinos
at all - a result which would pose a very severe challenge to
the theorists (see next chapter for details).

The concern over what was now widely referred to as 'the
solar-neutrino problem' was reflected at a small informal meeting
held at the Institute for Advanced Study, Princeton, in early
1972. Bahcall had just taken up a post at Princeton and it
was he who organised this meeting between Davis and leading
Princeton theorists,including Martin Schwarzschild and John Wheeler.
The subject discussed can be seen from a subsequent letter of
Davis's to Bahcall where he wrote:[42]

> The question we discussed at length...of whether or not
> our program is sufficiently vigorous to solve the problem
> in a reasonable time, say four years, is of primary concern.

Amongst other possibilities Davis outlined a four-year plan to
build a lithium radiochemical detector.

It can be seen that with the new lower limit on the solar-
neutrino flux, fresh impetus was given to the possibility of
launching new experimental approaches. However, new approaches
were not to prove feasible until nearly a decade later (see
below for details). Meanwhile the chlorine experiment once more
came under careful scrutiny.

1972 The Irvine Conference on Solar Neutrinos

It was no coincidence that a special conference on solar neutrinos was held at this time. The goals of the conference, which was organised primarily by Reines were'to clarify the present status [of solar neutrinos], to formulate a long range program and to elicit agency support'.[43]   And to this end, amongst the fifty scientists who attended the meeting held at the Western White House, San Clemente and the University of California, Irvine, were representatives from the National Science Foundation and the Atomic Energy Commission.   The conference was an informal 'chat shop' and the remarks made were tape-recorded and the edited transcripts appeared as the Irvine Conference Proceedings (Reines and Trimble,,1972).

Although the organisers had hoped that new experimental approaches would be uppermost, with so much new data available from the chlorine experiment (results obtained with pulse-rise time and water shield), much of the discussion centred on Davis's results. It became clear that some of the nuclear astrophysicists present at the meeting felt there were further tests Davis might perform to show his experiment was working as he claimed. In particular, it was suggested (by Fowler) that he put a fixed number of argon thirty-seven atoms into his tank (preferably the same number as was expected to be formed from neutrino interactions); leave them for a short while, and then recover them. If he recovered the same number as he initially put in this would be a good test of his recovery procedure.

It was also felt that he should test the possibility that Ar^{37} ions were somehow chemically trapped in the tank and hence not extracted in the helium purge.   This was the same possibility

that Bahcall had raised in 1968. Davis pointed out that this

could be tested with a further experiment using a tank of per-

chloroethylene labelled with Cl^{36}. The Cl^{36} would decay into Ar^{36}

and the number of Ar^{36} atoms expected to be formed could be

estimated. If he extracted the correct number of Ar^{36} atoms

this would rule out the trapping hypothesis, as Ar^{36} was

expected to behave in a similar way to Ar^{37}.

Not everyone at the Irvine conference was agreed that Davis

should spend further time on such tests. For instance, Goldhaber

stated that he thought there was no need for yet more tests of

the chemistry.[44] However, Davis himself, in line with his policy

of taking all suggestions seriously, no matter how unlikely

they seemed, stated that he was prepared to make such tests.[45]

Despite the probing of Davis's experimental procedures, it

was clear that the atmosphere of the conference was not hostile

towards Davis (most of the participants were on first-name terms)

and, indeed, there was much praise for his efforts in embarking

upon the experiment at all. The suggested tests were put forward

more in the guise of exploring every avenue rather than pointing

to serious holes in the experiment which Davis had overlooked.[46]

This renewed questioning of Davis's procedures, by the

nuclear astrophysicists, in particular, can be seen to stem from

the grave consequences which Davis's latest results spelt for

the theory. The air of crisis in nuclear astrophysics meant

that, whilst up until this point most theorists were satisfied

with the validity of Davis's results (with the exception of

Bahcall's initial scepticism), they now had more reason to be

critical. Even Bahcall now called for an independent check

on Davis's result be made.[47]

The radiochemistry was not the only area to be scrutinised

at the Irvine meeting. Suggestions were made as to loopholes

in both the nuclear physics and astrophysics (see next chapter

for details). As there were further checks to be made in both

the theory and experiment, and no other experimental approach

yet seemed feasible (the separation of counts from background

was still a problem in the planned-lithium experiment), the

atmosphere at the end of the meeting was very much one of 'wait

and see'. Cameron summed up the feeling of the conference as

follows:[48]

> As to the long range future, I think we should exhaust
> a lot of these avenues of research before we commit
> ourselves to major and expensive new facilities.

And, as Trimble and Reines stated in their review of the

conference:[49]

> The critical problem, is to determine whether the
> discrepancy is due to faulty astronomy, faulty physics,
> or faulty chemistry.

Davis was rather disappointed with the outcome of the Irvine

meeting since he had hoped more time would be spent on the

discussion of new experimental approaches rather than chewing

over his data.[50] As far as he was concerned, his experiment

was working correctly and had been since 1967. However, he

realised the importance of satisfying his colleagues in astro-

physics, however, far-fetched some of their suggestions seemed,

and thus he agreed to make the further checks requested.

Further Testing of the Radiochemistry

By June 1972, Davis had successfully carried out the test

of his Ar^{37} recovery procedure. He had introduced ~ 500 atoms of

Ar^{37} into his tank and after a short period had recovered them

with the expected efficiency. Also, two colleagues of Davis

at Brookhaven carried out some experiments to test for the

formation of stable argon molecule ions which might lead to argon

being trapped in the tank. They were not able to observe any

ions with their mass-spectrometer observations (Leventhal and

Friedman, 1972).

In addition to these tests, Davis ran another neutron-

source calibration test to check his earlier results. Again,

the result was consistent with his experiment working properly

and being capable of recovering a small number of Ar^{37} atoms.

Davis also explored the possibility of making a more

direct calibration of his experiment using a known source of

neutrinos. One suggestion, made by Alvarez, was to use neutrinos

produced by the decay of Zn^{65}. A sufficiently strong source

could be made by irradiating zinc in a nuclear reactor for a

year.[51] The source could then be placed in or near the tank.

Davis was unenthusiastic about this proposal as it would

be very expensive (he estimated a cost of $400,000 just to make

the source), and hazardous (the source would be very powerful

and would have to be transported over land and manoeuvered

in confined spaces underground).[52] In addition, the irradiation of

the tank might prevent Davis selling the perchloroethylene back

to the dry-cleaning company. He hoped in the long-term to

do this in order to recover some of the cost of the experiment.

Another approach to calibration considered in some detail

by Davis was to use neutrinos produced at the Los Alamos accel-

erator, LAMPF (Los Alamos Meson Physics Facility).[53] This

neutrino source would not only test the radiochemistry but also

the theoretically calculated neutrino-capture cross-section for

Cl^{37}. This idea had, however, eventually to be dropped when

it was discovered that the background in the experimental area

at LAMPF was too large to warrant such a test.

Davis eventually carried out the Cl^{36} test for chemical

trapping of argon ions. As we shall see in Part II, some of the

motivation for this test came from further questioning of his

experiment by nuclear astrophysicists. This test turned out

to be harder to carry out than originally planned because of

difficulties encountered in the preparation of the specially

labelled perchloroethylene. The test was finally completed in

1978 and again the results were consistent with the argon-recovery

procedure working as expected.[54]

It seemed with the success of these various tests, that

Davis had answered all the objections which had been raised at

the Irvine meeting. Over the period 1972-78, as Davis success-

fully carried out the tests, confidence grew amongst the astro-

physicists that the solution to the solar-neutrino problem was

not to be found in Davis's chemistry.

New Results

In addition to making tests of his procedures, Davis con-

tinued to take data from his main experiment (see Fig. 7.1, where

his results from 1970-78 are given). There was some excitement

over a run made July 7 - November 5, 1972 (run no. 27) which was

much higher than any previous result. There was much debate

at the time[55] as to whether or not Davis should include this

run in the overall analysis of his data. With run 27 included,

his limit was raised to 1.9 SNU as opposed to the previous upper

Fig. 7.1. Davis's Data 1970-78
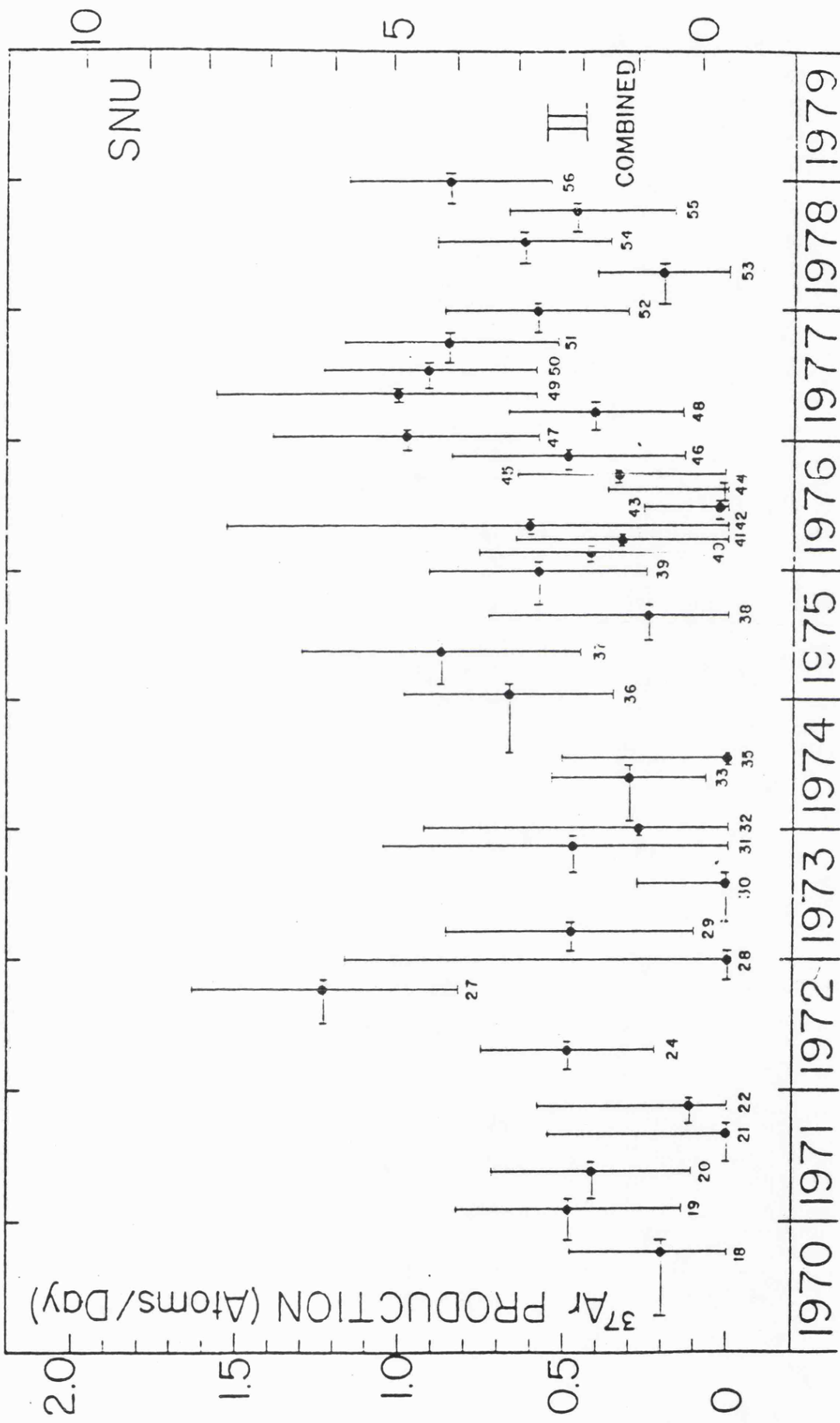
limit of 1 SNU.   A statistical analysis of run 27 (Pallister

and Wolfendale, 1974) indicated that it was anomalously high.

The large signal detected in this one run could be explained in

many ways. For instance, there might have been an increase in the

background for that run or else a fluctuation in the solar-

neutrino flux itself.   Other astronomical events, such as a

large solar flare (known to have occurred during run 27) or

a collapsing star, might also have been connected with the result.

Another possibility, which, as we shall see in Part II, was pursued

by one scientist, was that there was something untoward happening

with the argon chemistry in this run.

The possibility that run 27 was caused by a collapsing star

gained heightened interest in 1974 when an antineutrino detector,

run by Kenneth Lande and his group from the University of

Pennsylvania, and housed in the same mine as the Davis detector,

indicated the presence of a very large signal (Lande et al., 1974).

When this signal was detected on January 4,,1974, Davis, after

consultation with Bahcall, immediately decided to sweep his

tank in case he too had a large signal. Although antineutrinos

would not trigger his detector, most theories of collapsing stars

indicated that neutrinos and antineutrinos should be emitted

in approximately equal numbers.   Davis found only a small amount

of Ar^{37} for this particular run (Evans, Davis and Bahcall, 1974).

In view of this, considerable ambiguity has remained over both

the interpretation of the Lande event and Davis's earlier run

27.[56]

Throughout the period 1972-78, Davis continually made im-

provements to his experiment. The design of his counters was

modified so that they could be calibrated more accurately.   The

composition and total pressure of the filling gas were changed

in order to optimise energy resolution and pulse-rise-time

discrimination. A new anticoincidence counter and shield

arrangement were introduced.[57]   By the variation of cleaning

techniques and choice of materials, counters with even lower

backgrounds were introduced.[58]   Finally, in 1977, the counting

system was installed in the mine alongside the experiment. Thus

the need to fly the sample from Homestake to Brookhaven was

avoided.[59]

In order to collect more 'standardised' data, Davis initiated

a series of ten runs (nos. 39-49) where the tank was exposed

for 35 days and then swept. Up until then the tank had been

left for various periods of exposure according to contingencies

such as availability of counters and access to the tank.

As Davis continued to collect more and more data he found

that he had several runs in which the signal was larger than

average   (e.g. runs 36, 37, 47 and 49)[60]. The effect of these

larger values was to make run 27 appear less anomalous and

more as a statistical fluctuation. The larger results also

tended to raise Davis's upper limit slightly. In 1976 he reported

an upper limit of 1.5 SNU (Davis and Evans, 1976).

As more and better data became available, more sophisticated

statistical techniques were used in the analysis. Davis, it will

be recalled, had initially used the method of counting the

sample for a period when he expected signal plus background and

counting for a period when he expected background alone. The

signal was obtained by subtracting the background counts from

those obtained with the sample present. Fowler and other nuclear

astrophysicists, however, constantly urged Davis to attempt a

more sophisticated analysis of his data. Davis did not think

that the data warranted it but eventually he bowed to such

pressure and persuaded a colleague, Bruce Cleveland, to analyse

his data. Cleveland used a maximum-likelihood method which

utilized the time occurrence of all the decay events. From

this the most likely time distribution of Ar^{37} events could be

determined. He found that this technique gave a slightly larger

signal than Davis had obtained previously (this is because

negative counts are not permitted in the maximum-likelihood

method). Also, by this stage, enough data had been gathered

for many decay counts to be averaged together. Such an analysis

of all the events in the expected envelope of pulse-rise time

and energy gave a firm indication of the presence of argon

thirty-seven.[61]

With these more sophisticated analyses and improvements in

the sensitivity of the experiment, Davis, in 1978, was able to

give his result in the form of a number with an error, rather

than as an upper limit. His result was reported to be 1.6 ± 0.4

SNU.[62]   He did not claim that this result was definitely caused

by solar neutrinos since there was still a possibility that

background events in the tank were responsible and the source

might not necessarily be solar.[63]   However, it seemed less

likely now, than in 1972, that there were no solar neutrinos at

all to detect.

1978. The Completion of the Davis Experiment - The Planning of

New Experiments

By 1978, it was widely believed that Davis's experiment had

achieved all that could be expected of it. It was felt to be

unlikely that any major increase in sensitivity could be obtained.

Indeed, as far back as 1975, Davis had expressed the view that

he would have stopped taking data but for the continuing theore-

tical interest.[64]   The only point in continuing to take data

seemed to be to measure the fluctuations in the signal. The

small positive signal which Davis reported in 1978 was felt to be

a remarkable achievement, especially considering that the detector

had been designed for a signal an order of magnitude greater.

The high regard with which the Davis experiment was held

in 1978 was reflected at an informal conference on the status

and future of solar-neutrino research held in early January 1978

at Brookhaven (Friendlander, 1978a,b). The earlier criticisms

of the experiment made at the Irvine conference were not repeated

here and there were many laudatory comments concerning Davis's

work (see Part II). The status . the experiment had reached

by 1978 can be seen from the report of the conference in Physics

Today. There it was claimed:[65]

> Unlike the first data that hinted at the unexpectedly low
> value, the current results have gained general acceptance
> because the careful refinements and checks in the inter-
> vening years have dispelled most doubts.

With the completion of the chlorine experiment there was much

discussion at the conference of new experimental approaches

(indeed, the conference had been organised with this purpose

in mind). Most of the new experiments were designed to measure

the lower energy neutrinos fluxes either from the basic pp

reaction or the pep reaction. These fluxes were thought to be

less dependent on the details of the solar model and hence their

detection could be expected to be given a less ambiguous theoretical

interpretation than the measurement of the boron-eight flux.

The two new approaches to solar-neutrino detection which seemed

the most feasible were the 'gallium experiment' and the'indium

experiment.'[66]   The gallium experiment, with which Bahcall

and Davis are both involved (Bahcall et al., 1978), was proposed

as a collaborative venture between the Brookhaven National

Laboratory, the Institute for Advanced Study, the University

of Pennsylvania, the Max Planck Institute for Nuclear Physics

(Heidelberg) and the Weizman Institute (Behovot, Israel). The

planned detector is based on the reaction $Ga^{71} + \nu \rightarrow Ge^{71} + e^{-}$.

This reaction had first been suggested for the detection of

solar neutrinos by the Soviet physicist ,Kuzmin (1965).   At the

time it was first proposed an experiment did not look feasible

as gallium was a very rare metal and many tons would be needed.

However, with the increasing use of gallium in the electronics

industry (it is used in LEDs), it has become available in larger

quantities and at a cheaper cost.   Nevertheless to build a

sufficiently large detector to measure the flux of pp-neutrinos

will require an immense amount ($\sim$70 tons or the total world

consumption for one year).

The planned detection technique is very similar to that

used in the chlorine experiment. The gallium will be in a solution

of gallium chloride contained in a vast underground tank.   The

radioactive germanium can   be extracted from this tank and

counted.

The proposed indium experiment is a direct counting experi-

ment and was first suggested by Roger Raghaven and a group at

Bell Labs (Raghaven, 1976).   It is based on the reaction

$In^{115} + \nu \rightarrow Sn^{115} + e^{-}$.   The detector is expected to be sensitive

to pp neutrinos, pep neutrinos and Be^{7} neutrinos. As it is a

direct=counting experiment (making use of new counters developed

in particle physics), it should be able to distinguish between the different fluxes of neutrinos (because of their different energies) and hence provide a measurement of the neutrino-energy spectrum. Again, the planned experiment is very large; it will consist of $3\frac{1}{2}$ tons of Indium and 100,000 photomultipliers.

These second-generation solar-neutrino detectors are large scale projects and are expected to cost anything up to 25 million dollars each (the cost of a small accelerator). The battle to get these projects funded has already started and pilot experiments are under way. It seems that sometime over the next decade a measurement of the low-energy fluxes of solar neutrinos will be made.

In addition to the two experiments mentioned above and several other experimental proposals put forward at the Brookhaven conference, there is a vast Soviet effort in this area. A chlorine detector and a gallium detector are being constructed under a mountain in the Caucasus. Again huge sums of money are involved.[67]

Thus, as Davis nears the end of his research programme with the chlorine experiment (and also nears the end of his career) solar-neutrino astronomy seems to be on the dawn of becoming a 'big science' with large international groups and investments of many millions of dollars. As solar-neutrino astronomy enters the 1980's its prospects look very rosy indeed.

PART II. THE DECONSTRUCTION OF THE DAVIS RESULT

The Politics of Replication

In Part I, the slow acceptance of Davis's result was outlined. We saw that, although Davis was himself convinced of the validity of his experimental claims within a month of completing his first measurement, and other experimentalists were convinced almost equally as quickly, the nuclear astrophysicists were more sceptical and it was not until 1978 that Davis's result won widespread acceptance. This acceptance was achieved despite the experiment never having been repeated. As we saw, the major new experimental effort (apart possibly from the Soviet work) was aimed towards measuring different components of the solar-neutrino energy spectrum than those which Davis attempted to measure. However, Davis was able to increase the veracity of his experimental claims by carrying out exhaustive tests of his own procedures. These tests in some senses, served as a substitute for replication because Davis was able to take other peoples' doubts and test them on his own apparatus. It seems that by the use of 'exhaustive experimental method' Davis was able to establish his result as a fact of the natural world.

It was argued in Chapter 1, that the task of the sociologist of science is to deconstruct the facticity of seemingly immutable scientific results in order to show their social basis. Something of this process of deconstruction can be achieved in the case of Davis's result if the issue of replication is considered more closely.

As a starting point, let us imagine that replications of

Davis's experiment were attempted and failed to confirm his

result. In such circumstances, it is highly likely that Davis's

result would appear to be much less solid. This counter-factual

question is not as far-fetched as it might seem, especially if we consider

what happened in another case. The most obvious case for

comparison with Davis (and one to which many respondents them-

selves drew attention) is the Weber case (Collins, 1975, -

see also, discussion of the Weber case in Chapter 1). Weber,

in 1969, after years of apparently hopeless pioneer work, claimed

finally to have detected gravitational radiation.[68] This claimed

result, like Davis's was obtained with an experiment of great

technical difficulty, and also seemed, again like Davis's result,

to be theoretically unexpected. If reports of Weber's progress

are studied, it can be seen that, before the results of repli-

cations of his experiment appeared, the original claim had

achieved a degree of acceptance. For instance, in a report in

New Scientist in 1970, it was noted:[69]

> Earlier this week, Weber...published new data to support
> his claim that gravitational radiation is being observed.
> His arguments are very convincing and will no doubt
> convert many of the sceptics.

This solidity, however, quickly vanished once other groups started

to report negative results.[70] Today, most scientists would say

Weber's results were artefacts rather than facts (Collins, 1981b).

It is quite possible that Davis's result could meet a similar

fate if other scientists failed to confirm it. Such a possibility

was raised by one respondent who, in comparing Weber and Davis,

told me:

> I think that kind of acrimony [associated with the Weber
> episode] is not present in this field because there is not
> another experiment. To be absolutely honest, if there was

> possibly another experiment that gave a different result,
> you could possibly see that kind of debate raging in
> this field also.   When we have two alternatives, one
> doesn't respect what you did before in your life, but
> when there is only one result all these social feelings
> are strong arguments whether each individual person wants
> to accept a result or not. And, we are in that state I
> think.   And that's where Ray's personality, his openness,
> as well as his past accomplishments, are a strong
> indication that there can't be anything wrong.

The 'social feelings', referred to by this respondent, which have,

in a large measure, led most scientists to trust Davis, will be

discussed later.   From the above comment, however, it does

seem possible that the failure to confirm Davis's results could,

in principle, lead to changes in the current feelings that his

results are correct.

Of course, replication is not any more of an agent of

natural-world facticity than a single experiment, as Collins

(1975, 1981b) has shown for the Weber case and for replication

in general (Collins, 1976;   Travis, 1981). Experimental repli-

cation seems to be a thoroughly social constituted process.

However, it is interesting to ask why such a social process,

which is usually taken to be the corner-stone of scientific

method, has not been a central concern in the solar-neutrino

field.

Most respondents, when asked about the need for a replication

of Davis's experiment agreed that replication was a good thing

in principle and also pointed out that care was called for in

the interpretation of a result which had not been replicated.

However,they were still prepared to believe Davis's result.

A typical expression of this position is Bahcall's comment that:[70a]

> I know of no reason to doubt the ^{37}Cl results, and Davis
> has reported absolutely convincing tests that establish
> the validity of the ^{37}Cl detection method as well as one
> can for any single experiment, but scientists are more

confident when results are confirmed by independent
experiments. It would be very useful, for similar
reasons, for some other group to try to repeat the ^{37}Cl
experiment.

However, given that there were limited resources for such experi-

ments, most respondents felt that it was more important to carry

out a new experiment altogether rather than repeat Davis's

experiment. Typical comments were:

> If one could do everything that scientifically should be
> done there is absolutely no question that it should be
> repeated , but I think, then, the value would only be if
> it could be done by a quite independent group. I would
> not give it anything like the same importance as going
> after the pp neutrinos directly. I trust Ray Davis's
> experiment sufficiently that I would give an experiment,
> really quite fundamentally different, a higher weight.

> Well it would be nice to have a second detector, but it's
> such a big experiment that I think maybe it's just not
> likely, not reasonable, to have to repeat it...What we
> really want to see now is a different experiment. I
> don't think anybody wants to repeat the chlorine experiment.
> I think it would be nice to see the gallium experiment done.

Some respondents felt that the gallium experiment would

itself, in some senses, serve as a replication of the Davis

experiment; as one told me:

> One should repeat it but not repeat precisely the same
> experiment. Try to do a better experiment, one where you
> get slightly better statistics so as to advance the
> field...Just suppose, for instance, that the Russians
> were able to do the gallium...That would be an additional
> point on the curve and one could make some sort of
> intercomparisons, although we recognise that it's at a
> different spectrum and so on. So it would perhaps be more
> interesting if one had the choice to do the gallium first.

It was even suggested that the involvement of several different

groups in the plans to carry out the Brookhaven gallium experiment

would give the project the status of an 'independent' replication.

Another indication of the interpretative flexibility associated

with the term 'replication' came from one respondent who felt that

Davis's tests of his own experimental procedures in some ways
constituted replication. I had asked this particular respondent
whether Davis's experiment had ever been repeated; he replied:

> How do you mean; repeated by other people or just
> repeated with different apparatus? If you mean repeated
> with different apparatus it's not true to say. You know
> the improvements he made in shielding, you know a
> different way of having water around it and so on. That's
> almost like a new technical set-up.

One respondent felt there was little point in repeating
Davis's experiment anyway because even if a different result
was obtained, people would prefer to believe Davis:

> Say a second experiment was done, and it was different
> from Davis's, it would take a lot of convincing to be
> convinced that that experiment was right and Davis was
> wrong. That means you would need a third experiment
> and so on. So I really think for the progress of science,
> since they are relatively major efforts, that it would be
> better to put the effort in an experiment that gave you
> some kind of differing information.

In addition to these kinds of reason, I was told that the
cost and time-consuming nature of such experiments made repli-
cation improbable. Also it seems that there was little scientific
credit to be obtained from checking upon someone else's work
rather than attempting to make a new discovery. By way of
contrast, respondents pointed out that others had attempted to
replicate Weber's experiment because he had found a huge signal.
There was more incentive to repeat a positive result because
new discoveries could be made, such as subtleties in the experi-
mental properties of the new radiation. It seems likely that if
Davis had found a significant flux of neutrinos, then the Jenkins
and Reines experiments (discussed in Chapter 5 and Part I of
this chapter) would not have been discontinued.

This cautious attitude again reflects the confidence in the
veracity of Davis's result. If he had been mistaken, then a

replication might easily have led to such a new discovery!

The unwillingness of other groups to repeat Davis's experiment, again puts paid to the myth that scientific truth is established by independent replication. The politics of replication are far more subtle than that. Experimentalists do not enter the arena simply to conform with the statements of philosophers - although scientists still, it seems, occasionally pay lip service to the rhetoric of replication. Replication is a social process and, whether or not to embark upon a replication, is a decision which depends on other factors than a simple willingness to assert scientific truth. For instance, experimenters entered the gravity-wave field to carry out 'quickies' and disprove Weber,[71] and, in the solar-neutrino field experimenters have avoided repeating an experiment where they see little to be gained from such an enterprise.

As was emphasised above, the lack of replication has not proved to be a barrier to the acceptance of Davis's claims. In the rest of this chapter it will be shown that Davis's own tests of his procedures, which in some senses were a substitute for replication, are also thoroughly socially constituted. Happily the argument can now move from the counter factual to the factual. This is because an attempt was actually made to show that Davis's own tests did not guarantee the truth of his result. By following these arguments, it will be shown that 'exhaustive testing of experimental procedures' can be no more of a touchstone of scientific truth than replication.

Davis's Experiment Comes Under Challenge

As mentioned in Part I, there were two prominent occasions when nuclear astrophysicists became concerned over the experimental

validity of the Davis result.   The first such time was in late

1967, when Davis reported his initial observations. These

came under critical examination from Bahcall, in particular.

The second occasion was the Irvine conference in 1972, by which

time Davis was reporting a result which seemed to indicate

that no nuclear fusion reactions were occurring in the Sun at

all. On both occasions the criticisms were made directly to

Davis in a largely informal setting. Davis was able to satisfy

his critics by agreeing to make further checks of his procedures

and none of the doubts about the experiment were ever published.

Davis's personal relationship with the theoreticians and, in

particular, the partnership he had entered into with the influen-

tial Caltech group as far back as 1958, held him in good stead

and such criticisms were not made ad hominem. Rather, they

were put forward as loopholes in the (impersonal) chemistry

which normally would not merit attention, but, given the extra-

ordinariness of the result it was felt Davis should check them

out. Any personal remarks about Davis were always positive and

expressed admiration for his experimental achievements in tackling

such a difficult project in the first place.

The informal means of dealing with criticism of the

experiment only broke down on one occasion.   This was when

Kenneth Jacobs, an astrophysicist at the University of Virginia,

felt compelled to put his reservations concerning the experiment

into print.   He did this by way of a letter to Nature, published

in 1975 (Jacobs, 1975).

Jacobs's criticisms centred on the familiar problem of

'chemical trapping' of the argon in the tank.   Davis had hoped

that the experiments performed by his colleagues Leventhal and

Friedman after the Irvine conference which purported to show that

argon ions did not form molecules in combination with the perchloro-

ethylene would put an end to the matter.[72]   In the long-run he

planned to perform the Cl^{36} experiment which he felt would be

the definitive test of chemical trapping, but, as mentioned in

Part I, this was proving to be more difficult to carry out than

first planned.   When Jacobs started to worry about the chemistry

of the solar-neutrino problem in 1974 the Cl^{36} experiment had

not yet been performed.

Jacobs was not the only person concerned by the possibility

of chemical trapping at this time. Robert Rood, another astro-

physicist at Virginia, shared his concerns, and Rood was actually

a co-author of the original version of the Jacobs paper. Another

astrophysicist to consider the problem was Don Clayton, of Rice Univer-

sity, Texas. He too wrote to Davis in early 1975 expressing

worries about the possibility of chemical trapping.[73]

Jacobs's interest in the solar-neutrino problem seems to

have been stimulated by Rood. Rood is a solar-model specialist

who had worked at Caltech with Fowler. He has had a long-standing

interest in the solar-neutrino problem since studying for his

PhD under the supervision of Icko Iben at MIT (as we saw in

Chapter 4, Iben had been part of the original Caltech team to

work on the theory). Before moving to Virginia, Rood had

published several theoretical papers on the solar-neutrino

problem (Rood, 1972; Rood and Ulrich, 1974). He had also

attended the Irvine conference. Rood was thus an established

member of the network of ex-Caltech physicists who worked on

the problem. As such, he often received copies of Davis's

letters to Fowler which contained copies of the experimental

'raw data' (e.g., pulse-rise plots for individual runs).

Rood's own reasons for looking at the chemistry of the problem stemmed from his failure to find a satisfactory astrophysical solution to the dilemma. He felt the chemistry of the experiment was one of the least investigated areas and, as he had been a radiochemist . as an undergraduate, he was enthusiastic about finding a radiochemical solution to the problem.

Jacobs had also been at Caltech before moving to Virginia. His thesis had been in the area of relativity theory and cosmology (supervised by Kip Thorne). As had Rood, he had been an undergraduate chemist and organic chemistry was his hobby. Rood and he were close friends at Virginia and they both used to discuss Davis's raw data. It was from these discussions that their joint paper arose.

The main new idea of Rood and Jacobs was the possibility that perchloroethylene might, under certain conditions, form polymers. For instance, it was known that C_2H_4 polymerizes to form polyethylene and C_2F_4, which is a close analogue of C_2Cl_4 (perchloroethylene), polymerizes easily to form Teflon. They felt that gamma radiation might induce polymerisation and that Ar^{37} ions could be encapsulated into tiny polymer globs which would not be extracted from the tank by sweeping with helium gas. Rood and Jacobs suggested some tests for this possibility and concluded that the best test would be the Cl^{36} test which Davis planned to carry out.[74]

Rood's and Jacobs's paper was sent to Davis by Rood for comment. (Rood knew Davis from the Irvine meeting). Davis's unenthusiastic response can be gauged from the following extracts

of his letter written to Rood in reply:[75]

> ...I think it is very unlikely that these mechanisms are
> operating in our detector and reducing its sensitivity.
> However, I appreciate your feelings about the apparent
> conflict between the theory and experiment ...We have
> placed most of our efforts into developing the ^{7}Li detector
> to test the PeP flux and increasing the sensitivity of the
> ^{37}Cl experiments. Apparently several astrophysicists
> feel we should spend more effort in checking further our ^{37}Cl
> experiment. These additional checks are difficult, expensive
> and time consuming, but we are doing them. My personal
> feeling is that no new chemistry or physics will come of
> these tests whereas something new could come from a
> second solar neutrino experiment.

Having pointed out that he regarded such tests largely as a waste

of time, Davis went on to assure Rood that he would nevertheless

carry out the Cl^{36} test which would further check for trapping.

Rood also sent the paper to Fowler for comment. Fowler

passed it on to a Caltech chemist, Norman Davidson, who commented

that what was being suggested was 'pretty unlikely'.[76]

In his letter to Rood, Davis also suggested that he come to

Brookhaven and discuss the experiment in detail with him. In

fact Rood was not able to take up this offer until 1977, but

in March 1975, Davis visited Virginia (Jacobs was, at the time,

on sabbatical in England) and Rood talked with him extensively

about his experiment. After this discussion Rood decided to

drop out of the joint paper with Jacobs. As he told me:

> I communicated with Davis. That was the first time I
> had really talked to him at length. I sorta lost
> enthusiasm...Because it turns out that a lot of the ideas
> that I felt were fairly clever, he had considered and he
> had actually done some preliminary-type experiments to
> show the thing worked right...
> We sent copies to Fowler, and Fowler had passed it on to
> chemist friends and having had Davis explain very carefully
> to me all the things that had been done I lost enthusiasm...
> It seemed to me no point in casting doubt on the experiment...
> What I'm saying is it's very unlikely that you would
> produce any action that you felt was very beneficial anyway.

Thus the type of informal procedures which Davis had used in

the past to disarm his critics in nuclear astrophysics once more

seemed to have worked. Rood could see that publication of his

reservations could 'cast doubt' upon the experiment, and he

was thus prepared to wait and see what the tests would show.

Rood continued his informal interaction with Davis and the

eventual success of the Cl^{36} experiment meant that ultimately he

became very enthused about Davis's experiment. He remarked:

> Since that time I've gotten actually to see Davis a number
> of times...and I've been to Brookhaven to look at the
> experiment....At the moment I have no doubts at all that
> the experiment is good and I have the utmost admiration
> for Davis as an experimenter.

Undeterred by Rood's loss of faith, Jacobs pressed ahead with

the trapping hypothesis alone. As well as the idea that perchloro-

ethylene might form polymers, Jacobs explored the possibility

of some sort of weak binding between Ar^{37} ions and perchloro-

ethylene which might prevent the Ar^{37} being extracted from the

tank. He felt that there might be some theoretical justification

for this; as he told me:

> I went another step further, which was to look at rare-gas
> chemistry and try to extrapolate downwards to argon
> theoretically. We know that krypton and xenon bind with
> things like chlorine and oxygen....At that time it wasn't
> completely known how the binding occurred in terms of
> molecular orbitals...I never had the time or money to
> do the computer studies on it, or didn't know enough
> chemistry about how to really get it to go. But I did
> figure out that if you could bind the thing by more than
> approximately 1/10 of an eV you could account for the
> effects seen and also the fluctuations in the counts.

Jacobs had the idea that, with binding at this level, the

thermal agitation in the liquid would account for the breaking

of bonds and that this could lead to fluctuations in the signal,

such as the higher run 27. Jacobs felt that this high run could

not be accounted for in terms of a cosmic source of neutrinos
(such as a collapsing star) as run 32 which was coincident with
the antineutrino events detected by Lande's group (See Part I,
p.238) had been low.

As well as attempting an embryonic theory of argon binding,
Jacobs also did some experiments with a group of Dutch molecular-
beam specialists to try and see if they could get argon to bind
to chlorine. These experiments were inconclusive as the X-ray
source being used blew up!

Jacobs, in the winter of 1974 when he was in Cambridge,
redrafted the original paper he had written with Rood. His plan
was to submit this paper to Nature and he first sent a copy to
Davis. In response to the paper, Davis reiterated the points he
had made in his earlier letter to Rood.[77] He also remarked that
he felt it was unreasonable to use runs 27, 32 and the Lande event
which were all subject to a variety of interpretations, to argue
that the argon formed a chemical compound in his tank. Davis also
felt that Jacobs should give more weight to the work of Leventhal
and Friedman. Finally, Davis once more asked Jacobs to come out
to the mine and see the experiment and go over all the details
with him personally.

It is interesting to speculate as to why Jacobs pressed on
with the trapping explanation and did not back down as Rood had
done. One reason for this may have been to do with Jacobs's own
position on the fringes of the solar-neutrino physics community.
Rood was part of the Caltech network which had worked on the
problem. Although Jacobs had been at Caltech,his interests lay
more in the direction of relativity theory and cosmology and
he had never worked directly on the problem. Jacobs spent the

year in which he developed his chemical-trapping hypothesis on

sabbatical in Cambridge and Holland.   He was thus even further

out of the North American network which might have constrained

his action. Also Jacobs, unlike Rood, never talked directly to

Davis about his experiment.   Jacobs felt Davis, as a person,

was largely irrelevant, as he told me:

> ...It has nothing to do with Davis. I have never even
> met him so I hope it's not taken negatively by people...
> Several people have said to me they believe Ray Davis
> is a good experimentalist, the best in the world to do
> this, and they trust him. They don't come right out and
> say I shouldn't disbelieve him. They are just very
> positive about it, no-one says anything the other way...
> If it comes down to a question of a democratic vote
> versus science - I realise now that a lot of science
> is democratic vote or consensus--but deep in my gut I'm
> not willing to believe the majority, if I really believe
> that something is going on in the experiment.

In the published version of his paper, which appeared in

Nature in August 1975, Jacobs examined all the chemical tests

carried out by Davis and others (including Leventhal and Friedman)

which were meant to exclude trapping. He pointed out that

two of the tests - the Ar^{36}-recovery   efficiency test and the

recovery of a fixed number of Ar^{37} atoms from the tank- both

excluded trapping of neutral argon but not argon ions (in

neither experiment were argon ions produced in the same manner

as by neutrino interaction). Jacobs also argued that the neutron-

source calibration test did not rule out trapping, as the amount

of Ar^{37} extracted in such a test could not be determined

absolutely since the Ar^{37} production rate from neutrons could

not be calculated.   It could only be estimated by comparing it

with the recovery rate of the carrier Ar^{36}.   Since Jacobs had

argued that the Ar^{36}-recovery test did not rule out the possi-

bility of Ar^{37} ions being trapped, neither too did the neutron

test, which dependent on the same Ar^{36}-carrier data. Another

argument against trapping which Jacobs considered was one which

rested upon the cosmic-ray background estimates made at the

Homestake mine. It had been found that the data, taken at

six different heights with small tanks of perchloroethylene, fitted

the appropriate theoretical curve for the penetration of cosmic-

ray radiation with depth (Wolfendale, Young and Davis, 1972).

Since all these experiments required the extraction of Ar^{37}

from tanks it might be argued that this fit with theory showed

the chemistry of the experiment to be unproblematical. However,

Jacobs pointed out that this was, again, not a definitive test,

since the absolute cosmic-ray background could not be calculated

and hence all the experiments might have a constant error caused

by trapping of Ar^{37}. Finally, Jacobs briefly discussed the work

of Leventhal and Friedman. Again, he found this not to be a

strict limit on the trapping hypothesis since all their data on

the lack of formation of Ar^{37} molecules were relevant to the

gas phase.[78] Jacobs wrote:

> To extrapolate these gas-phase findings to a liquid C_2Cl_4
> environment ($\simeq 10^6$ times denser), however, is totally
> unwarranted. (Jacobs, 1975: 560).

Jacobs's conclusion was:

> From this investigation of experimental constraints I
> conclude that the chemical hypothesis remains a viable
> solution to the solar neutrino problem only if the Ar^{37}
> is 'chemically trapped' in Davis's system before neutralising
> to free ^{37}Ar atoms. (Ibid: 560)

The version of Jacobs's article which finally appeared in

Nature was much shorter than the original paper. Jacobs had

difficulty in getting it through the referees, not only because

it was too long, but also because of what the referees thought

was a provocative introduction.   As Jacobs told me:

> It made allusions to other experiments of a marginal nature,
> where the phenomena being sought was very elusive and
> where there was only one experiment involved. And they
> didn't like that sort of an introduction...Well I was
> thinking of things like the solar-oblateness experiment
> and what was called the old A_1A_2 problem in particle
> physics...Theorists spent a long time trying to explain
> that whole phenomena and they discovered that with more
> data it went away, that sort of thing. And they didn't
> like the insinuation that if you have, what I call a
> marginal detection experiment, and only one experiment,
> nothing else, that it can be dangerous...

Indeed, Jacobs has informed me that part of his motivation

for questioning the experiment itself stemmed from these other

well-known 'mistakes' where there was only one experiment. That

scientists refer to other infamous cases of error as part of a

general debunking ploy against an experiment is a well-known

phenomenon from other cases.[79]   However, in this case Jacobs

was attempting to debunk what was becoming regarded by most

scientists as an exemplary example of a well-done experiment.

It is no surprise that his allusions to other 'bad' experiments

were thought to be inappropriate.

Jacobs soon received a reply to his paper. This was published in

Nature by a group of Indian physicists: Banerjee, Chitre,

Divakaran   and   Santhanam   (1976).[80] They pointed out

additional experimental evidence which indicated that the poly-

merisation of perchloroethylene in a way which would trap argon

was extremely unlikely.   They also stressed that it was unlikely

that the result of the neutron-source test and the cosmic-ray

data were in error by the order of magnitude which, they maintained,

was necessary to make trapping a plausible way to explain Davis's

results.   In the same issue of Nature, Jacobs, in turn, replied

(Jacobs, 1976). He pointed out that polymerisation of perchloro-

ethylene was just one of the hypotheses he had suggested and that some sort of weak binding of Ar^{37} was more likely to be the trapping mechanism. Thus, the additional evidence against polymerisation, was not fatal to his case. He also rejected Banerjee et al.'s order-of-magnitude argument, pointing out that Davis's most recent data were much nearer the theoretical lower limit and hence that a small amount of trapping only would be necessary to explain the discrepancy. Thus the neutron-source test data and the cosmic-ray data need not be out by as much as an order of magnitude. His conclusion was:

> Finally, I conclude again that my chemical solution to the solar neutrino problem remains a viable alternative. (Jacobs, 1976: 557).

It can be seen that, despite the attack on his trapping hypothesis by Banerjee et al., Jacobs was still able to argue for the feasibility of his position. It seems as if the tests of Davis's procedure which were so convincing to other scientists were not compelling for the determined critic.

It would be wrong to give the impression that the debate between Jacobs and his critics has been vitriolic. The best scientific manners have been displayed on both sides.[81] Neither has the debate been particularly vociferous. The response to Jacobs in Nature and a later article by the same authors expanding upon and reiterating their arguments (Banerjee et al., 1977), was almost the full extent of the attention which Jacobs's comments received. The only other place where Jacobs's arguments are discussed is in a review article published in Science by Bahcall and Davis (Bahcall and Davis, 1976). There, the implausibility of any chemical-trapping mechanism is reiterated.[82]

It seems that although Jacobs, unlike other astrophysicists, made his criticisms public he was to all intents and purposes ignored or suffered the fate of, what has elsewhere been referred to as, implicit rejection.[83]   Jacobs himself felt that it was a major victory even to get Bahcall and Davis to refer to his work. As he told me:

> As far as I know the only other published reference to this at all was an article by Bahcall and Davis in *Science*, where they got so they couldn't avoid the fact that I published that thing finally.   I'd been talking with them and they finally agreed to write something in this review up to that date.   Basically to say that they didn't agree, but that's OK.   At least they acknowledged its existence.

The lack of response to Jacobs is a sign of the consensus view that Davis's experiment was correct.   Any lingering doubts were finally dispelled in 1978, when Davis eventually carried out the long-promised Cl^{36} test and found that he recovered the correct number of Ar^{36} atoms. For most scientists this was the final nail in the coffin of chemical trapping, especially as this was the same test which Jacobs himself had suggested as being capable of settling the issue. Given the similarity between the dynamics of the beta-decay of Cl^{36} into Ar^{36} and of the Cl^{37} inverse beta-decay, it seemed unlikely that there could be chemical trapping of Ar^{37} and not Ar^{36}.

It is important to note, however, that this test does not necessarily rule out chemical trapping, any more than the previous tests had done. Ar^{36} is different from Ar^{37} and if anyone was sufficiently motivated and ingenious enough they could perhaps claim that the difference was significant enough to invalidate the test as a means of ruling out trapping. This, of course, would mean going against contemporary cultural wisdom,

but the fact that Jacobs had in the past been prepared to do
this, points to the possibility that an even-more-determined-
critic might be able to get around the Cl^{36} test too.[84]

What of Jacobs himself? Given his reservations concerning
the experiment could we not expect him to be that'even-more-
determined-critic'? It is unlikely that, having suggested the
Cl^{36} test himself, Jacobs could, with any plausibility, now
refuse to accept it as a good test. It is much more likely
that, if he still had doubts, he would claim that the test was
improperly performed. Unfortunately the question can only be
asked in the abstract because Jacobs, in 1976, ceased to
be a practising physicist when he failed to get tenure at the
University of Virginia (an episode which does not seem to be
connected directly with his work on solar neutrinos).[85] When
I talked to Jacobs in 1978, he had been a financial analyst
at Bell Labs for over a year and hence was no longer in a position
to pursue his interest in solar neutrinos. In fact he had been
so upset by his failure to get tenure that he had burnt all
his notes and correspondence on this problem along with his
other papers! Jacobs had, however, heard of the Cl^{36} test via
Rood. He certainly was not prepared to accept it at face
value without at least seeing the results. As he told me:

> He got supposedly a statistical result that said nothing
> bound. I think that's a pretty good test, unfortunately
> I haven't seen it in print anywhere.

In that as of 1978 the results of the Cl^{36} test had not been
published Jacobs again can be seen to display greater scepticism
than his colleagues who were mostly prepared to accept the Cl^{36}
test based on Davis's word and despite it not having been
published.

It seems that Jacobs's efforts came too little and too late

to have any real chance of damaging the credibility of the Davis

experiment. He was always faced with an uphill struggle as

by 1975 most scientists were already convinced of the validity

of the experiment. Jacobs's efforts could largely be ignored,

especially as he was in a marginal position vis-à-vis the solar-

neutrino physics community. He was also taking on an experiment

in which there was a lot at stake. As has been emphasised before,

the Davis experiment was widely regarded as one of the most

important experiments in nuclear astrophysics carried out over

the last decade.

Jacobs felt some of what he was up against, himself; as he

told me:

> The simplest thing would be to say everybody is keeping
> quiet, you know.They believe Davis and that's the
> establishment, so this [his idea of trapping] is
> unacceptable.

The implications of the reaction to Jacobs' ideas for the social

construction of science will be discussed further in Chapter 10.

The degree of doubt and scepticism which Jacobs and Rood

(for a short time) felt about the Davis experiment is difficult

to gauge with hindsight. However, the following extracts

from my conversation with Jacobs in 1978, as he relived the period

when he worked on the problem, are informative:

> Jacobs: The average counting rate with time, it seemdd
> to have funny trends in it. If I recall back
> before a certain number of runs, they weren't
> convinced they had anything at all and their
> little Auger counters began to get better. They
> began to see things and then, all of a sudden,
> you have got run 27 or thereabouts and things
> begin to go up to the sky and then they begin
> to oscillate around. So the results seem to be
> a function of time of technique as opposed to real
> answers:.... [My emphasis] .

Oh yes, there was a problem still about the water shield. If I remember that now. I never really consider anything worth looking at till after the water shield went on. It's just too much of a chance for bad background.

Pinch: But Davis says the water shield doesn't make much difference.

Jacobs: But its still a dangerous thing to leave off... From the rumours I've heard they were having trouble with their little tiny Auger counters. It's such a tiny thing, and they are very delicate and very difficult to make reproducible....In the early days, from what I heard, and I can't even say where I heard it, but it was a very distinct impression, you couldn't really trust anything that was coming out. It was only later that they began to feel confident that they had decent counters. Like I say, this is a very marginal experiment, right on the very edge.

Pinch: But most people are confident that his data are good.

Jacobs: I think it's more a question of trusting him and his ability, which is a good thing, people should trust, but if there is a difficulty in the solar-neutrino problem you should look to see where it is.

From these extracts a much less solid view of the Davis experiment starts to emerge. Rumours about trouble with the equipment and allegations that incorrect procedures have been followed cast a general air of suspicion over the whole thing. If the above view was widespread amongst the solar-neutrino physicists rather than just being held by one, not very influential, astrophysicist then it would not be hard to imagine a climate of opinion being built up in which the chemical-trapping hypothesis would be welcomed as a vindication of more generally held suspicions. In such circumstances Davis's results, rather than being fact, could become artefact.

Any experiment, if probed exhaustively enough, can be made to look shaky and doubts can be cast upon the competence of the

experimenter. The routine errors and uncertainties involved in
getting any difficult and complicated experiment to work can
take on special significance for the critic. What is seen by
the experimenter as part and parcel of the 'trial and error'
nature of research becomes, for the critic, an important indication
of 'lack of care' or 'sloppiness'.[86] Such criticisms can easily
escalate and engender an air of paranoia and mutual suspicion,
as has been seen in other cases.[87] In the face of such criticisms,
the experimenter may become more cautious about divulging details
of his procedures. This, in turn, can reinforce the critics'
doubts and be taken as evidence that there really is something
to hide. If such criticisms become widespread then the experiment
can quickly lose credibility.

This loss of credibility has not happened in the case of
the Davis experiment. Although, as we have seen, Davis faced a
determined critic in the shape of Jacobs, Jacobs was not able to
make much headway. Davis was able to respond to Jacobs's
criticisms in much the same way that he had dealt with previous
objections. His policy of being completely open with his data
and offering to consider any suggestions had worked in the
past and it was unlikely that at this late stage that he would,
himself, become embroiled in a mutual-reinforcement cycle of
criticism and paranoia.[88] Indeed, Davis's openness and his
willingness to consider all criticisms is mentioned by most
respondents as one of the main reasons why they believe his
experiment is good. They often contrast Davis's attitude with
that of Weber. Many respondents felt that Weber's gravity-wave
experiment (and Dicke's solar-oblateness experiment)[89] had lost

credibility because they had become so secretive about their data.

The Weber case ,in particular, appears to be a classic example

of criticism leading to paranoia which, in turn, reinforced the

criticisms.

Typical comments concerning Davis's openness were:

And he [Davis] always responded to people, and if his
first response didn't satisfy people he would frequently
go out and do an experiment to show that he was right,
and he was always incredibly open to telling you what he
did...Whereas, from what I can see, Weber and Dicke
enshrouded their experimental technique in a cloak of
mystery of some kind. They would not help, as far as I
know, the critics with the details that the critics needed
to pursue their criticisms. It was always 'Well we'll
tell you this later'.

I think the attitude was, as I stressed earlier, Davis
always disarmed his critics by doing what they suggested -
Weber didn't. Weber was very defensive, very sure he
was right, and for a long time resisted any attempt
by people to look at the data.

One can contrast this to other exciting discoveries where
in some cases the experimenter sorta gets his back up if
people seem to be asking '.Did he do this right or that right?.'
Ray has always been so grateful for any criticisms that
people make or any suggestions for something he might try
to test.

Discussion of the Acceptance of Davis's Result

As was mentioned in Part I, there is little doubt that by

1978 Ray Davis and his experiment were held in very high regard.

Although one or two scientists still expressed lingering worries

to me that somewhere in the vast tank the few atoms of argon thirty-

seven were somehow being lost (see Pinch, 1981a, in Appendix II),

the consensus was generally in favour of Davis. Perhaps some of

the status in which Davis's work is held can be seen by remarks

made by two eminent members of the North American physics community.

The first comment comes from Fowler, who, as we have seen, has

had a long-term interest in the area and who, it has been widely

rumoured, could have expected to receive a Nobel prize if Davis had found the predicted signal. Fowler, in a recent article[90] on solar neutrinos, referred to Davis as 'one of the greatest experimentalists in the world'. The other comment comes from Martin Schwarzschild, the Princeton theorist who has played a large part in the development of stellar-evolution theory. Schwarzschild summarised the feelings of the Brookhaven conference towards Davis, and, from his remarks, it can be seen why Davis is so widely admired:[91]

> There is one, exactly one, more feature of this conference
> that I feel I should summarise. It's that feature that
> we usually refer to as "Ray Davis". It's a feature of
> quite singular character. It differs from most of us in
> three respects. One is straight ability in this wide
> field...Singularity number two is his stubbornness. He
> has been for two decades under a barrage of criticisms...
> Ray Davis has not let himself be driven into a corner.
> He has smiled when silly criticism has come, let it roll
> off his shoulders and when serious criticism has come,
> he has quietly gone and invented a test and at the next
> session everything was in order on that point....There is
> a final characteristic and that is the unusual caution
> with which Ray Davis presents his results. It is a
> marvellous caution for those who understand what he is
> talking about because it doesn't prevent the insider from
> feeling the excitement that lies behind his very, very
> quiet and cautious work. But it does have the consequence
> that Ray Davis is not what he should be, namely a public
> hero in science.
> Ray it is difficult to quite put in words our admiration
> for you, for you both as a scientist and as a person. The
> only words that I know to say and they are much too weak
> are, "From all our hearts, congratulations for the milestone
> you have achieved."

Schwarzschild's comment that Davis has impressed with his caution, was echoed to me by many of the nuclear astrophysicists I spoke with in 1978.[92] They admire the way that he refuses to indulge in theoretical speculation concerning his result and limits his presentation to matters of experimental detail.[93] Again the publication format adopted by Davis and Bahcall which permitted separate experimental and theoretical papers can be

seen to have facilitated this.

From the above comment of Schwarzschild's, in particular, it is very clear that Davis's personal success and his scientific success are closely intertwined.

In any attempt to explain Davis's success in eventually convincing the theoreticians of his claims, it is important to bear in mind the personal relationship he had built up with the Caltech group over the years. As we have seen in earlier chapters, Davis had entered into this partnership with the theoreticians as far back as 1958. Both sides of the partnership had kept in close touch over the intervening years and the Caltech group had played a key role in funding Davis's experiment. Bahcall had eventually become so closely involved with the project that he became the 'house theorist'. Given the close links between the two groups before 1968, it was exceedingly unlikely that the groups should fall out with the disclosure of Davis's result. Davis's good informal relationship with the nuclear astrophysicists helped him in winning acceptance for his result. He could give them details of his experiment at first hand and they, in turn, could put their criticisms directly to him without having to publish them formally - a publication which would have done the experiment little good. By the time a criticism appeared in print (Jacob's paper), the battle had largely been won by Davis.

Davis's partnership with the theoreticians meant that he was obliged to test all sorts of implausible hypotheses. Although such tests were largely a waste of time in terms of his experimental goals (he was convinced the experiment was working back in 1967), they did serve an important ritual function in satisfying the theoreticians and thereby boosting the credibility of his

experiment.   The importance of ritualised scientific rationality

in the social construction of science is familiar from other cases

(Wynne, 1976;   Collins, 1981b).

The role of the partnership with the theoreticians has been

acknowledged by Davis;   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...

The importance of the partnership between theorists and experi-

menters in the social construction of science is one of the most

important findings of this thesis and is further discussed in

Chapter 10.

Whether Davis's low profile and concili .atory stance has

meant that he has got less rewards than he should have, as

Schwarzschild suggests above, is a difficult question to answer.

Certainly, compared with the leading theoretician (Bahcall)

Davis has not done as well. Bahcall holds one of the most important

chairs in astrophysics and is also a fellow of the National

Academy of Science. Davis on the other hand had (in 1978 anyway)

not been elected to the National Academy and still works in a

National Laboratory. Such comparisons are, however, almost

impossible to make since Bahcall has done much work in other fields.

Davis's lack of institutional recognition may stem from him

falling between the twin stools of chemistry and physics and

also, such rewards are not usually made for negative discoveries.

It is possible, however, that, when there is scientific consensus

over the explanation of his result, Davis may receive more acclaim

(the parallel case might be the Michelson-Morley experiment). In

any case, Davis's own modest nature has meant that he has not sought

insitutional rewards. The main reward for Davis is knowing that

he has done a competent job and is widely respected for this.

In summary then, in 1978, ten years after he started to

collect data, Davis's result was widely taken to be a fact of

the natural world. Although his experiment has never been

repeated it was felt that, by his exhaustive tests of his procedures,

he had done more than enough to show the validity of his result.

However, as has been argued here, his knowledge claim can be

deconstructed and the social processes involved in its construction

revealed. Davis's achievements can be said to have been located

in the social world.

NOTES FOR CHAPTER SEVEN : PART I

1. In later runs Davis alternated between using argon thirty-six and argon thirty-eight as the carrier gas.

2. Letter, W. Fowler to R. Davis, August 10, 1967.

3. This is reported in a letter, R. Davis to W. Fowler, August 11, 1967.

4. Ibid.

5. Ibid.

6. Ibid.

7. These two chemists were G. Friedlander and M. Perelman.

8. A report of this meeting is to be found in, C & E N (Chemical and Engineering News),September 25, 1967, 13-14.

9. Apart from his work on solar neutrinos, Davis carried out work on the analysis of meteorites and lunar rocks and dust. This work was based on radiochemical techniques too. His uncontroversial work in this field must have provided further evidence to his colleagues of his experimental competence.

10. Letter, R. Davis to D. Delicate, September 22, 1967.

11. Letter, R. Davis to J. Bahcall, October 31, 1967.

12. Ibid.

13. Ibid.

14. Letter, R. Davis to J. Bahcall, December 8, 1967.

15. Ibid.

16. This was a sizeable piece of apparatus, as Reines told me:
    It's quite large and ambitious piece of equipment though. It still exists but it's mouldering and decaying. We never used it. It's about the length of this room and a good fraction of the width.

17. For instance, in a letter written in December 1967, Jenkins remarked:
    At present the outlook for detecting B^8 neutrinos from the sun...is very dim. The reason for this is the latest result of Davis...The preliminary result of his initial run, which he quoted to me on the telephone the other day, is that the flux is $2 \times 10^6/cm^2/sec$. Our equipment here has been improved steadily...However, since the apparatus was designed to detect a flux of the order of that predicted theoretically this latest information is such as to make the pursuit of the experiment

17. contd.   rather pointless. We are presently debating the
             future of the apparatus and I strongly suspect
             that we will terminate operating it within the
             next month or so.
    Letter, T. Jenkins to D. Allen, December 26, 1967.

18. For example, X-Ray Astronomy has grown rapidly in the last
    two decades.

19. Letter, A. Wolfendale to R. Davis, December 14, 1967.

20. Most of this criticism has been put forward in the context
    of seminars. As Bahcall has told me:
             The experiment has been challenged repeatedly
             over the eleven years since. At every seminar
             people raise some questions all of which have been
             subsequently answered. Ray is very good about that.
             I've raised some myself which he's done, and lots
             of other people have.

21. Letter, R. Davis to J. Bahcall, February 16, 1968.

22. Letter, J. Bahcall to R. Davis, February 26, 1968.

23. Bahcall argued that the background determination (made by
    Davis in the first two runs) should be treated as a different
    experiment since it was carried out before the sample was
    introduced. The introduction of the sample perhaps upset
    things in some unknown way. He argued that it would be more
    self-consistent to measure the background with as least
    disturbance as possible - that is after a sufficiently
    long time for all the Ar^{37} to have decayed (thus leaving
    background counts only).

24. In his letter to Bahcall of November 6, 1968, reporting
    the results of a new run, Davis wrote:
             The flux limit is the same as given earlier, but
             is free of the objection you raised....In this
             case the initial background, the final residual
             background and the sample count were essentially
             the same.

25. For instance, Fowler (1969: 368) wrote:
             It may well be that the "standard" background observed
             by Davis does not apply to the radioactive and
             carrier argon which is subject to considerable
             pre- and prior-exposure purification and processing.
             It is my impression that the upper limit...may well
             represent a real effect.
    And Cameron wrote to Bahcall that:
             Personally, I hope you are right and that the
             negative result obtained by Davis will turn into
             a positive one...
    Letter, A. Cameron to J. Bahcall, July 31, 1968.

26. Letter, J. Bahcall to R. Davis, January 26, 1968.

27. Letter, J. Bahcall to R. Davis, op. cit., note 22.

28. This statistical issue became important again in 1978 when the upper bound of the experimental result and the lower bound of the theory were fairly close together - see Pinch (1980 a) in Appendix II.

29. Bahcall and Davis (1976).

30. Letter, R. Sears to R. Davis, May 9, 1968.

31. Telegram, R. Dicke, D. Morton, J. Mark, R. Ruffini, and J. Wheeler to R. Davis, October 22, 1968.

32. Another reference to the Davis experiment at the time is that of Gribov and Pontecorvo in their article in which they put forward the suggestion of neutrino oscillation to explain the low result (see next chapter for details). They refer to the 'beautiful experiment of Davis et al.' (Gribov and Pontecorvo, 1969: 493). Of course, a beautiful experiment can always be wrong!

33. Letter, R. Davis to A. Wolfendale, May 3, 1968.

34. R. Davis Jr. 'A Search for Neutrinos From the Sun', B.N.L. 12981, 1968.

35. See, for example, G.E. Kocharov, 'The Proton-Proton Cycle and Solar Neutrinos', Soviet Physics Doklady, 9, 1964, 468-70; G.E. Kocharov, 'Nuclear Reactions in Stars and Solar Neutrinos', Bulletin of the Academy of Sciences of the USSR, 29, 1965, 1563-9; V.A. Kuzmin, 'Neutrino Radiation and Thermometry of the Interior of the Sun', Bulletin of the Academy of Sciences of the USSR, 29, 1965, 1573-5; V. A. Kuz~min, 'Neutrino Generation in the Solar Interior', Soviet Astronomy, 9, 1966, 953-6; and V.A. Dergachov and G.E. Kocharov, 'On the Investigation of the Internal Structure of the Sun by Means of Neutrino Radiation Studies', Canadian Journal of Physics, 46, 1968, S491-S493.

36. This account of the Moscow meeting is based upon that given in Bahcall and Davis (1980).

37. For an account of the Soviet work in 1978, see B. Belitsky, 'Soviet Neutrino Astronomy', Spaceflight, 311-312. I am grateful to David Edge for drawing my attention to this reference.

38. Dicke et al., op. cit., note 31.

39. Letter, R. Davis to W. Fowler, November 9, 1968.

40. Letter, R. Davis to F. Reines, November 14, 1968.

41. These measurements were made using detectors based upon the reaction $Ca^{40} + n \rightarrow Ar^{37} + He^4$.

42. Letter, R. Davis to J. Bahcall, February 3, 1972.

43. Reines and Trimble, 1972: 'Conference Description'.

44. For Goldhaber's comments see Reines and Trimble (1972:C25).

45. This offer was reiterated by Davis in his letter to Reines of March 30, 1972, where he thanks Reines for organising the conference.

46. Perhaps the attitude towards Davis at the Irvine conference is best summed up in the report of the AEC representative, R. Kandel. Kandel wrote:

> The meeting was convened, primarily, to sit in judgement of the BNL-Davis experiment...The astrophysicists and nuclear physicists present half-heartedly suggested that something might be wrong with the chemistry...another year or two of work remains on this experiment. Most people who have looked into it don't seriously doubt the results - but nobody understands them.

Letter, R. Kandel to R. Van Dyken, March 10, 1972.

47. For Bahcall's comment see Reines and Trimble (1972: C24).

48. For Cameron's comment see Reines and Trimble (1972: D7).

49. V. Trimble and F. Reines, 'The Solar Neutrino Problem - A Progress (?) Report', Reviews of Modern Physics, 45 1973, p. 1.

50. Letter, R. Davis to F. Reines, March 30, 1972.

51. The energy of the neutrinos produced by the decay of Zn^{65}_{30} was not sufficient to excite the analogue state of Cl^{37} but it would test the ground-state cross-section as well as the Ar^{37} chemistry. For more details see, L.W. Alvarez, 'A Signal Generator for Ray Davis' Neutrino Detector', Lawrence Radiation Laboratory, Physics Notes Memo 767, March 23, 1973.

52. Although it is not planned to use this calibration for the chlorine experiment, it is proposed to calibrate the gallium experiment using Zn^{65} (for details of the gallium experiment see later this chapter).

53. See, R. Davis, E.C. Fowler, S.L. Meyer, J.C. Evans, 'Study of the Neutrino Capture Cross Section in ^{37}Cl with μ^+ decay Neutrinos', Research Proposal to the Los Alamos Meson Physics Faculty, June 1973.

54. The results of this test are reported in Davis (1978).

55. See, for example, Evans, Davis and Bahcall (1974) and Pallisto and Wolfendale (1974).

56.   I asked all the scientists I interviewed in 1978 about both events. Most scientists felt that run 27, with hindsight was probably a statistical fluctuation. The interpretation of the Lande event is a matter of speculation as no similar event has since been recorded.

57.   Davis reported the introduction of a new shield (a layer of mercury 8 inches thick) on August 8, 1975. He said he hoped this would reduce the counter background by a factor of three.   Letter, R. Davis to H. Hecht, August 8, 1975.

58.   Davis reported to Fowler in late 1975 that all these changes had led to the first counter he had constructed with an essentially zero background (used for run 37). Letter, R. Davis to W. Fowler, December 9, 1975.

59.   No appreciable reduction in background was achieved by placing the counter in the mine.

60.   A few respondents have remarked that the trend in the data seems to be that of a sine wave (see Fig. 7.1), or a pattern which might be linked to the solar cycle. See, for instance, K. Sakurai, 'Quasi-biennial Variation of the Solar Neutrino Flux and Solar Activity', Nature, 278, 1979, 146-8; and A. Subramanian,'Neutrino Flux Correlation with Solar Activity', Current Science, 48, 1979, 705-7.

61.   All these developments are reported in Davis (1978).

62.   This result was reported in Davis (1978). The value has, on further analysis, subsequently been revised to 2.2 ± 0.4 SNU (Letter, R. Davis to T. Pinch, July 30, 1979). A Bayesian analysis of Davis's data is reported as giving a result of 2.2 ± 0.3 SNU, see Davis (1978).

63.   There is still some uncertainty over the exact contribution of the cosmic-ray background. Provisional background measurements by E. Fireman of the Smithsonian Astrophysical Laboratory are reported to indicate a background twice that estimated by Davis. See Davis (1978).

64.   Letter, R. Davis to D. Dodansky, January 15, 1975.

65.   'Solar-neutrino hunters still seek explanation', Physics Today, December 1978, 19.

66.   Many other proposals were discussed at the conference and there was an air of competition between the various proposals. For full details see Friedlander (1978a,b).I have much fascinating data on the funding battle for these experiments. However, as there is, thus far, only one pilot study funded (for the gallium experiment at a cost of 1.5 million dollars) it would be politically insensitive to present such data until the experiments are funded.

67.   See Belitsky, op. cit. note 37. Some of the correspondence I have seen suggests there has been a serious effort to get a joint US-USSR project off the ground. However, thus far the only real collaboration seems to have been short scientific exchange visits.

NOTES FOR CHAPTER SEVEN : PART II

68.   See J. Weber, 'Evidence for the Discovery of Gravitational
        Radiation', Physical Review Letters, 22, 1969, 1320-4.

69.   'Monitor', New Scientist, 12 February 1970, p. 294.

70.   See Collins (1981b).

70a.   J.N. Bahcall "Some Remarks on Solar Neutrinos", Talk
        presented at the Leningrad Conference on Particle
        Acceleration and Nuclear Reactions in Space, August
        19-21, 1974.

71.   See Collins (1981b).

72.   See, for instance, letter, R. Davis to W. Fowler, June
        23, 1972.

73.   Davis answered Clayton's letter in a similar way to that
        which he answered Rood's (see below). Letter, R. Davis
        to D. Clayton, March 13, 1975. Clayton told me in 1978
        that he did not now think the problem lay in the chemistry.

74.   K.C. Jacobs and R.T. Rood, 'A Speculation on the Deficiency
        of Solar Neutrinos', unpublished paper, 1974.

75.   Letter, R. Davis to R. Rood, June 25, 1974.

76.   Letter, W. Fowler to R. Rood, June 25, 1974.

77.   Letter, R. Davis to K. Jacobs, December 17, 1974.

78.   The type of arguments Jacobs mounted here can in principle
        be made against all calibration experiments. The basis of
        the argument is the difference between the calibration
        experiment and the main experiment. Jacobs's arguments
        all pointed to the significance of this difference. For
        the experimeter, however, these differences are not
        important. In order to get the calibration experiment
        accepted as valid the experimenter draws on a repertoire
        of cultural resources which point to the non-significance
        of the differences. The critic can only get around the
        calibration by challenging these cultural resources.
        Calibration of itself is clearly not a proof of the
        validity of the experiment since the determined critic,
        such as Jacobs, can mount such a challenge and circumvent
        the calibration (this point is elaborated in Chapter 10).
        However, the cultural resources drawn upon for the
        calibration experiment are usually more central and solid
        than the new finding which the main experiment is purported
        to demonstrate. By forcing the critic to challenge these
        more solid areas of knowledge the critics' task is made
        much harder. As the edge of criticism is shifted towards
        ever more solid parts of knowledge, the critic will appear

78   contd.
more and more deviant. Rather than appear to be this radical
the critic will often bow to the weight of contemporary
cultural wisdom and be forced to accept the calibration
as a legitimate test. In this way calibration can be
said to have a social function. See, Collins (1980).

79.   Harry Collins informs me that this ploy was used in the Weber
case.

80.   There is no reference to this reply in any of the correspondence
to which I was given access. One of the Indian scientists,
Chitre, had been at Caltech. Whether they were encouraged
by Fowler (say) to reply to Jacobs is an open question.

81.   Jacobs, as mentioned above, had nothing personally against
Davis and made no recourse to ad hominem arguments.

82.   This is not quite the full extent of the literature on trapping.
Now, with the Cl^{36} experiment completed, Jacobs's arguments
can be safely mentioned as an example of a meritorious wrong
idea! For instance, Bahcall and Davis in their recent
history of solar neutrinos take this approach; they write:

> There have been some worries expressed by physicists and astro-
> nomers that there could be something wrong with the
> radiochemical procedures...Some specific suggestions
> were advanced by Kenneth Jacobs....Although these
> suggestions were not based upon sound chemistry, we
> felt that an experiment should be performed to test
> these unlikely possibilities. To this end an
> experiment was performed with ^{36}Cl-labelled per-
> chloroethylene...This experiment and the other argon
> efficiency tests made with the 100,000 gallon tank
> show that ^{37}Ar is recovered with high efficiency.
> (Bahcall and Davis, 1980: 44-45).

83.   See Collins and Pinch (1979) and chapter 10.

84.   In other words a 'hypothetical Jacobs' could, in principle,
challenge this calibration test. See Harvey (1981) and
Collins (1981b) where similar hypothetical arguments are
used to show the non-determinate nature of experimental
outcomes.

85.   Both Jacobs and Rood have assured me that Jacobs's work on
solar neutrinos had nothing directly to do with his failure
to get tenure. This was not the 'establishment' way of
getting back at him for stepping out of line. However,
there is an indirect connection. Jacobs did not have many
'solid' publications in his mainstream area. He tended
towards dilettanteism. His work on solar neutrinos (being
chemistry) may not have counted for much professionally.
As Jacobs commented, with some bitterness:

> I think it may also be more profitable to write
> papers dealing with the possibilities of what might be going
> on the other way [i.e. theoretical possibilities]

than to worry about the experiment. Worrying about the experiment is not a profitable venture at all. There are few papers and essentially no profit in the sense of scientific profit. I'm sure you don't get tenure for trying to find something wrong with someone else's experiment, unless you find something wrong.

It is interesting to note that Jacobs's last paper, before losing his post, was the solar-neutrino paper.

86. This view is argued extensively in Pinch (1981a) in Appendix II, and Collins and Pinch (1982).

87. Again, the Weber case is the prime example - see Collins (1976, 1981b). But there are many others. For example, see the reception of the SRI remote-viewing experiments of Targ and Puthoff discussed in Collins and Pinch (1979).

88. Davis's confidence in this policy can be seen from his comment to me that he actively encourages people such as Jacobs to publish their ideas.

89. R.H. Dicke and M. Goldenberg, 'Solar Oblateness and General Relativity' Physical Review Letters, 18, 1967, 313-6.

90. W. Fowler, 'The Case of the Missing Solar Neutrinos', Engineering and Science, May-June 1978, p..4.

91. M. Schwarzschild, 'Conference Summary' (Friedlander, 1978b: 278-80).

92. It would be tedious to give further examples.

93. Some theorists complain that Davis is a little too modest. They think it would be easier to get funding for new experiments if he made more claims for the importance of his experiment. However, this would not necessarily be a good thing from their point of view because if Davis made exaggerated claims he might damage his credibility which, in the long-term, would not help with funding. (cf. the exaggerated claims of cancer researchers to be able to find a cure).

CHAPTER EIGHT

THE THEORETICAL PREDICTION OF THE SOLAR-NEUTRINO FLUX 1967-1978

The developments in the theory since 1967 (when the first experimental measure of the solar-neutrino flux was reported by Davis), form the subject matter of this chapter. The material, which is drawn from correspondence files, articles, and interviews, is again presented in two parts. Part I focusses on the year immediately following the announcement of Davis's result. In terms of Fig. 1.1, this takes us from the prediction of 19 SNU, of 1967, to the 7.5 SNU of 1978. This period, as we shall see, was characterised by different assessments as to what the theoretical consequences of Davis's result were. These differences culminated in a bitter controversy between two of the leading theoreticians as to whether or not there was a conflict between theory and experiment. Part II covers the period between 1968 and 1978. The main development up until 1976 was a growing consensus that the theory and experiment were in conflict coupled with an air of crisis in the theory. However, in 1976, the gap between the theoretical prediction and experimental result started to lessen and, by 1978, arguments were once more being broached as to whether or not there was a conflict.

As in previous chapters, this one has a chronological and descriptive bias. However, topics important for the overall argument of the thesis also get detailed treatment in what follows. In particular, in Part I, it is argued that once Davis's experimental result became known, the theoreticians, and especially Bahcall, set about attempting to accommodate the result within standard theory. By drawing attention to new input data and laying stress on

the uncertainties in other input data, Bahcall was able to argue

that the result were not in conflict with the theory. The

revisions in the theoretical prediction which followed the

appearance of Davis's result add weight to the argument of earlier

chapters that the standard theoretical prediction is to be seen

as a social construct which can, in certain circumstances, be

permeated by wider social factors. In this case, it is suggested

that Bahcall's previous commitment to the validity of the theory

and the need to maintain his credibility as a theorist led him

to seek ways of avoiding a conflict between theory and experiment.

As a result of the interpretative flexibility in the theoretical

prediction, it was possible, in the aftermath of Davis's first

result, to argue that the result was consistent with the theory

(as Bahcall maintained) or that there was a conflict (as another

theorist - Iben - maintained). These two opposed positions,

although both supported by seemingly legitimate scientific

arguments, indicate that 'conflict' and 'consistency' are both

terms which are subject to a degree of interpretative flexibility.

It would appear that conflict and consistency between theory and

experiment are themselves socially constructed relationships.

In Part II ,the interpretative flexibility of 'conflict' and

'consistency' is revealed further when it is shown that Bahcall

eventually changed his mind about the consistency of the theory

and experiment and became one of the leading exponents of the

'conflict' thesis. It is suggested that such a view was

rhetorically expedient in terms of arguing for new solar-neutrino

experiments. However, in 1976, with the lessening of the gap

between theory and experiment, arguments once more started to

appear as to whether or not there was a conflict. This renewed

debate again illustrates the interpretative flexibility possible

in the comparison of theory and experiment.

PART I. THE EVENTS OF 1967-1968

The First Reaction to the Davis Result

It will be recalled that, just before Davis's initial result

was known, Bahcall and Shaviv had finalised their theoretical

prediction in the light of new measurements of the cross-section

for the $He^3 + He^3$ reaction (S_{33}). The dramatic change (by a factor

of five) in the value of S_{33} had drawn attention to the possibility

that other parameters, upon which the prediction was based, might

be in error and part of the theoretical investigation which

Bahcall and Shaviv undertook was to look at a range of possible

uncertainties. Their conclusion was that the overall detection

rate, $\Sigma\phi\sigma$, was 19 ± 11 SNU, and they predicted a B^8 flux of

$\phi_B 8 = 1.4 ± 0.8 \times 10^7$ neutrinos $cm^{-2} sec^{-1}$. Although the paper

in which these results were given, was not published until nearly

a year later (Bahcall and Shaviv, 1968), these predictions were

widely quoted at the time the first experimental result became

known.[1]

The first discussion of the implication of Davis's result

for the theory came at a Japanese nuclear phsyics conference held

in September 1967. Maurice Goldhaber, the director of the Brookhaven

National Laboratory, had been following Davis's work closely and

mentioned the first result in his opening address to the

conference. Goldhaber showed a slide where the then experimental

upper limit of $\Sigma\phi\sigma$ < 12 SNU was compared with the Bahcall and

Shaviv prediction of $\Sigma\phi\sigma$ = 19 ± 11 SNU. Goldhaber remarked[2]

> ...you might say Davis is getting at most an effect of
> the kind expected by Bahcall, but it might still be
> considerablv smaller when the background counts are
> subtracted.

This last comment refers to the background effect in the tank which

had not yet been subtracted from the 'signal'. Although even this

upper limit seemed to be lower than Bahcall's best value for the

prediction, Goldhaber, as yet, saw no clear contradiction. As he

went on to comment:

> I do not believe that he [Davis] will succeed in
> contradicting Bahcall because of the large errors
> which incidentally, in my opinion, are optimistic
> errors because of all the cross-section errors which
> go in. (Ibid : 21).

It seems that Goldhaber felt that there still might be large

errors in the prediction, particularly cross-section errors,

and that these errors had probably been underestimated (optimistic

errors) as had proved to be the case for S_{33}. Thus it seemed

likely to him that the prediction could be lowered to bring it

in line with Davis's upper limit.

Goldhaber also noted that Davis's experiment did seem to

have ruled out the CNO-cycle as the predominant mode of energy

production in the Sun. It will be recalled that if the Sun

worked on the CNO-cycle then Davis was expected to detect 35 SNU

(Bahcall, 1966). Davis's upper limit of 12 SNU led Goldhaber to

remark:

> So you might say Davis has probably shown it is not all
> the CNO cycle (Ibid.: 21).

Although this confirmation of a small part of the theorists'

claims received some acclaim,[3] it can also be seen to be not that

significant a result since there was almost universal agreement

before Davis's experiment that the Sun was too cold for the CNO-

cycle to predominate anyway.

It seems that Davis's first result, which was at this stage still only provisional, although lower than expected, was not held to be completely out of the bounds of theoretical possibility. Goldhaber's emphasis on the lack of conflict and what he felt to be the remaining nuclear-physics uncertainties, was the first indication of the approach to be taken by the Caltech group in accommodating Davis's result. They too were to emphasise the lack of conflict.

Davis's Own Reaction

Before going on to discuss the reaction of Bahcall and other theorists, let us first see what Davis himself took to be the theoretical consequences of his result. Like Goldhaber, Davis was reluctant to draw any very firm conclusions from the first measurement, simply reporting to Fowler that the result seemed 'quite low'.[4] However, by the time he had made his second measurement and obtained an even lower limit, he became more confident over the interpretation. His view was quite straight-forward. A certain strength of signal had been predicted which he had failed to find. For instance, in a letter to the American Physical Society (written in November 1967 when his upper limit was 4 SNU) Davis commented:[5]

> We now have preliminary results, but unfortunately they are negative. The experiment was designed to observe the flux calculated by theorists from detailed models of the sun. Our present limits to the solar-neutrino flux appear to be a factor of five below the calculated flux.

On December 1st (by which time his limit had fallen to 3 SNU and he had completed the neutron-source calibration test - see Chapter 7), he wrote to Arnold Wolfendale that:[6]

> The experiment is a factor of seven below the calculations...
> John Bahcall and Al Cameron are aware of these results and
> I trust there will be some changes in the theory.

From this it is clear that Davis saw his result as being in dis-

agreement with the theory and, as a consequence, he expected some

changes to be made in the theory.

Davis's emphasis on the prediction he had gone out to test is

understandable since the whole project was designed around the

expectation that he would see a certain strength of signal. For

Davis, as an experimentalist, the theory was most important in

the context of arguing for and designing experiments. As we saw

in chapters 3 and 5, Davis, throughout the history of his research

programme, had turned to theory where,and when it could be used

to support his experimental ambitions. Although he needed the

theory (and the theoreticians) to argue for the merits of his

experiment (and to get funding for it), this did not mean that

he expected the theory to be exactly correct. Indeed he had had

the foresight to design his apparatus so as to be sensitive to

a smaller signal than that predicted in 1964.[7] Given this

attitude to the theory, Davis was not that surprised when he

failed to confirm the prediction. As he told me:

> I didn't feel strongly that I should see exactly what
> they calculated...So the fact that it didn't come out
> as expected didn't strike me as being so anomalous as
> it did someone like John Bahcall who was very certain
> that it would come out the way that he calculated it to.

Davis's own opinion was that the theory was far too simple to

take into account the full complexities of the Sun. As he remarked:

> I feel the Sun is very complicated, and that there are
> many things that aren't taken properly into account - and
> in essence a lot of this is a gross extrapolation on
> elementary physics.

Davis's sceptical attitude towards the theory was well
served by the publication format of separate papers published
'back to back' which he and Bahcall had used in 1964 and again now
in 1968 with the appearance of the first major experimental
report and theoretical discussion.In their paper,(Davis, Harmer
and Hoffman, 1968) Davis and his group stated their failure to find the
predicted signal and referred to the accompanying paper by Bahcall
and his associates (Bahcall, Bahcall and Shaviv, 1968) for
the theoretical interpretation. The publication format enabled
Davis to be cautious about the theory in the same way, as we saw
in the previous chapter, it enabled Bahcall to be cautious about
the experiment.

Bahcall's Reaction

As Davis hinted above, Bahcall's reaction was rather different.
After all he had spent the previous five years on the calculations
and he fully expected his prediction to be confirmed. Bahcall's
plan had been to produce the best prediction shortly before
Davis made the first measurement and, to this end, he and Shaviv
had reviewed all the uncertainties just before Davis obtained his
first result. As we saw in Chapter 6, Bahcall anxiously awaited
the first result not least because he felt that this was a personal
test of his scientific capabilities. Thus, for Bahcall, once it
became clear that the result and his prediction were in conflict,
this was no mere scientific anomaly - it was a personal disaster .
The extent to which this affected his life can be seen from the
following anecdote (related to me by Bahcall) which describes
a seminar which took place in 1970, by which time it was clear
to Bahcall that the experiment and his prediction were in sharp
conflict:

> Ray Davis...came to Caltech and gave an informal seminar,
> and Richard Feynman was there...And he is known for being
> a really tough guy at a seminar...Ray started off with
> what I told him, and what it was he had measured, and
> why he believed what he measured was right. And it was
> clear that there was just absolute conflict and afterwards
> I was enormously depressed...I think that was really the
> low point of my feeling about science...I remember after
> that seminar,Feynman...when he saw I was really pretty
> much destroyed by this...took me for a walk around Caltech...
> and then he took me out to dinner...He was really very
> nice and told me, 'Don't worry, you've done nothing wrong,
> nobody has found anything wrong in your calculations...
> It's not your fault if there's a discrepancy, it's all
> the more important'.
>
> For me that was the lowest point I ever reached
> scientifically and 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 to my ego 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 initial dismay at the conflict between theory and

experiment shows the amount of faith he had put in his prediction

being correct. Davis, when he first discussed his result personally

with Bahcall and Fowler (in early 1968), was also a witness to

this. As he told me:

> I remember going to Caltech and giving a seminar on the
> result [this is probably an earlier seminar than the one
> referred to by Bahcall above] ...John Bahcall was very
> defensive about the theory. He didn't see that there
> could be anything wrong with it.

Bahcall's competence as a theorist was threatened by the

Davis result. Although, in the long run, as Feynman had tried

to point out to him, the result could be more important because

it conflicted with the theory, Bahcall, at the time the result

first came out, was expecting his prediction to be confirmed.

His disappointment was probably enhanced by the active

part he had played in getting the experiment funded. It was not

just his career or credibility as a theorist which was at stake,

but also a $600,000 investment. There was a danger that he might

become seen to be responsible for a very large white elephant -

a one-hundred-thousand-gallon tank of cleaning fluid a mile

under the Earth, which was not detecting anything! As Bahcall

told me:

> And the thing we were most worried about this experiment,
> in funding it, was that it was just a pipe dream of
> astronomers and they'd measure nothing anyway. (This
> quote, is also to be found in Chapter 7, p.228).

In view of what Bahcall had staked in the project, it is

not surprising that when he first got an indication that Davis's

result was out of line with his prediction, he fought long and hard

to try and avoid the consequences. For some time Bahcall refused

to believe there was any serious contradiction between theory

and experiment and, as we shall see in Part II, it was not until

1970 that he was finally prepared to admit to there being a serious

discrepancy.

Part of Bahcall's initial response to the result was, as we

saw in Chapter 7, to double check everything Davis had done.

In addition, he hoped for a while that Davis's report of a

negative result might turn out to be a positive result because

of problems in the estimation of Davis's counter background.

Thus he did have reservations about the experimental side of the

apparent conflict. However, his main line of attack was to re-

examine the theory, and in particular the input parameters, in

order to try and find a way of accommodating the result.

Bahcall had time on his side in this endeavour. Because of

his close relationship with Davis, he knew of the results

immediately - he also knew of Davis's publication plans. As

the first major paper giving the results did not appear until

May 1968, Bahcall had approximately six months in which to try to lessen the impact. If Davis had published his results immediately, the clash between theory and experiment, of a factor of seven, would have seemed much more dramatic. As we shall see, by the time the results were actually published, the theoretical prediction had dropped to within the margins of the experimental limit and thus the conflict with theory was much less clear cut. Furthermore, because the first experimental report appeared alongside the new prediction, the impression could be obtained that this prediction was derived at about the same time as the result became known. In other words, it would not be so obvious that the theoreticians had had to revise their predictions in the light of the experimental result . For instance, Fowler, gave such an impression in one of the first major reviews of the result; he wrote:

> Thus at about the same time in 1968 that Davis completed
> the analysis of his preliminary experiments, Bahcall
> and collaborators found [the theoretical prediction
> given in May 1968]...(My emphasis, Fowler, 1969: 367).

Of course, in actual fact Davis, as we saw in the previous chapter, was certain of his result by December 1967[8] when the prediction was higher, and Bahcall had had indications of the low result as early as August 1967.[9]

It seems as if Davis had, at one stage, planned to publish his results earlier that winter in Physics Letters, a quick publication journal, rather than submit to the slower Physical Review Letters. However, Bahcall had advised him against this, writing:[10]

> I hope that you will yourself submit your results promptly
> to Phys. Rev. Letters (I would like a chance to see the

> paper first if possible in rough draft form) <u>as I think</u>
> <u>you don't do the experiment any real good by having it</u>
> <u>appear first in Physics Letters.</u> I'm sure you feel the
> same way about it. (My emphasis).

Whether, by this comment, Bahcall was pointing out the lower

prestige of <u>Physics Letters</u> vis-à-vis <u>Physical Review Letters</u>

or was hoping for more time before the result appeared, is an

open question. However, it is clear that the Caltech group

themselves had no such reservations about publishing in <u>Physics</u>

<u>Letters</u>. For whilst they advised Davis not to publish his result

there, they had, at about the same time, submitted their own

theoretical results to <u>Physics Letters</u>![11] Furthermore, their

paper gave the appearance of being written in ignorance of

Davis's result. The experiment was referred to as being

'currently' carried out[12] and the unwary reader could easily gain

the impression that the theoretical results reported had been

obtained independently from any knowledge of what the experimental

result was. As we have seen already, the Caltech group had,

however, been aware of Davis's result since the previous August.

The Caltech paper was written by John Bahcall, his wife

Neta (also a Caltech nuclear physicist), Fowler and Shaviv.

The paper was received by <u>Physics Letters</u> on January 23 and

published on Feburary 19 (Bahcall, Bahcall, Fowler and Shaviv,

1968). The main claim of the paper was that there was sufficient

uncertainty in key nuclear-physics cross-sections to mean that the

solar-neutrino flux could not be predicted very accurately at all.

To demonstrate this, the results of two extreme solar-model

predictions were given. One prediction had various nuclear parameters

pushed to the extreme of their ranges so as to produce the smallest

possible result (7 SNU). The other prediction gave the largest

possible result (49 SNU) and had the parameters set at the other

extreme of their error range.

The reason given for looking at the nuclear-physics uncertain-

ties in particular was the correction which had been needed in

S_{33} (although this had occurred over a year earlier). It was now

argued that other cross-sections and in particular, S_{34}, S_{17} and

S_{11} (the cross-section for the p + p reaction) could all be in

error. I will briefly look at each of these cross-sections in

turn.

S_{34} had been measured in 1963 by Parker and Kavanagh and

a value of S_{34} = 0.47 ± 0.05 keV-barns had been obtained. This

value had been used in Bahcall's previous predictions. Now it

was claimed that other extrapolations of the S-factor data for

this reaction could be obtained such that S_{34} could be as high as

0.6 keV-barns or as low as 0.3 keV-barns.

S_{17}, it will be recalled from Chapter 6, had been a matter

for concern in 1965 because up until that point there had only

been two low-energy measurements by Kavanagh. Parker (with the

aid of Davis) had, however, remeasured the cross-section and

obtained a value of S_{17} = 0.043 ± 0.004 keV-barns. At the time

there was great relief because Davis and Bahcall had feared

that this result might be considerably lower. Parker's new

value had been used by Bahcall in his prediction made in 1966

(Bahcall, 1966) and that made on the eve of Davis's experiment

(Bahcall and Shaviv, 1968). Now, however, S_{17} was quoted as

ranging from 0.02 keV-barns to 0.05 keV-barns. The first value

came from Kavanagh's old measurements and the second was the

upper limit on Parker's more recent measurement.

S_{11} cannot be directly measured in the laboratory since

the reaction proceeds too slowly. It can only be estimated

theoretically. Bahcall's latest calculations of S_{11} indicated

it could range from 3.70 x 10^{-25} MeV-barns to 3.03 x 10^{-25} MeV-barns.

The uncertainty in this cross-section had, however, been pointed

out by Bahcall and Shaviv in their paper written on the eve of

Davis's first experiment. The significance of this particular

cross-section will be discussed below.

With attention now focussed on the uncertainties in the cross-

sections, Bahcall et al.were able to conclude their article by

referring to the 'large uncertainty in the prediction for the

neutrino experiments' (Bahcall, Bahcall, Fowler and Shaviv, 1968:

361) and to call for new experimental measurements of S_{34} and S_{17}

to be made. This meant, of course, that it would be much harder

to say whether or not Davis's result was in conflict with the

theory.

It is interesting to note that Bahcall et al.'s estimate

of the spread in the uncertainties in the flux prediction which

arose from nuclear-physics errors was now larger than that given

earlier, before the experiment was funded. It will be recalled

that Bahcall had written to Davis then claiming that he could not

find uncertainties in all the parameters (both astrophysical

and nuclear) and the solar model that lowered the predictions

by more than 40% (Chapter 4, p. 136). Also, in Fowler's (1964)

letter of support to the AEC the impression was given that the

nuclear-physics data were better known than now appeared to be

the case. It will be recalled that Fowler had written then:[13]

> The position has been reached where little more can be
> done in the study of the nuclear reaction rates either
> theoretically or experimentally...With our present
> knowledge the neutrino flux at the earth can..be
> precisely predicted.

The increased spread in the range of the prediction, which came from the nuclear-physics data, seems to have come as a surprise to Davis. He had been led to understand that the errors in the nuclear physics were fairly small by this stage. As he had written to Parker earlier (in December 1967):[14]

> As you know there are errors in the calculations that
> have been carefully assessed by John [Bahcall] and the
> result is outside of the expected errors. The errors
> in the cross sections should be small now...it is hard
> to believe that the error in the flux calculations is
> in the nuclear physics. (My emphasis).

In response to now receiving a preprint of the Bahcall et al. paper, Davis felt compelled to question some of the findings. In particular, he disagreed with the large error range which Bahcall placed on S_{17} and S_{34}. Concerning S_{17}, he wrote (to Bahcall):[15]

> It does not seem reasonable to use the old Kavanagh
> measurement as an extreme. Peter [Parker] used a
> technique that gives a far cleaner result than
> Kavanagh's, and I would think his result would be
> accepted with the quoted error for S_{17}.

On S_{34} he noted:

> The range of values of S_{34} of 0.3 to 0.6 keV-barn seems a
> little wide.

He went on to remark rather pointedly:

> If you stick to the quoted errors from the experiments,
> I would estimate the range of values of the $\phi(B^8)$ would
> be 1.3 to 2.2 x 10 $cm^{-2}sec^{-1}$ [this compares with a range
> of 0.38 to 3.31 x 10^7 cm-2sec^{-1} given in Bahcall et al.
> (1968)]. This range is more in agreement with the spread
> you have quoted before arising from the errors in the nuclear
> cross sections. (My emphasis).

Davis's comments indicate the change in emphasis which had now occurred. It seemed that the nuclear-physics data would bear a range of interpretations. Just as in 1964 Bahcall had interpreted the data on the He^3 + He^3 reaction as indicative of a lower value of S_{33} and hence a larger flux, he now interpreted the data on

the Be^7 + p and He^3 + He^4 reactions as indicative of a larger range

in the values of S_{17} and S_{34} than had previously been quoted.

The reason for this change in emphasis was partly the impact of

the revision that had been needed in S_{33}. However, the change

in S_{33} had occurred nearly a year earlier and the uncertainty in

S_{34} and S_{17} had not been emphasised in Bahcall's and Shaviv's

(1968) paper where all the uncertainties were reviewed in prep-

aration for Davis's first measurement. In view of this, I would

argue that a more important reason for the change in emphasis

now was Bahcall's knowledge of Davis's first result. The new

emphasis on the uncertainties in the cross-sections would mean

that the conflict between prediction and experiment would be

less sharp. Thus Bahcall's embarrassment would be lessened

and, furthermore the blame for the change in the prediction

could be seen to lie in the uncontrollable input data rather than

the theoretical calculation itself. And, of course, the

publication of the cross-section uncertainties before Davis's

result was itself published gave the appearance that the

discovery of the uncertainties arose <u>independently from the</u>

<u>knowledge of the first experimental result.</u>

As it turned out, by May 1968 Bahcall's argument that the

experimental result fell within the range of his prediction

became even more compelling when further input data, upon which

the prediction was based, were re-examined. New values for two

parameters came to the fore which, when incorporated into the

theoretical prediction, had the effect of lessening the gap between

theory and experiment.

The existence of new data for one of these parameters, the

solar heavy-element abundance, Z, seems to have been pointed out

first by Davis. The new measurements, made by D.L. Lambert (1967)

had been chanced upon by Davis when scanning through Nature. He

had written to Bahcall in December 1967, asking him:[16]

> How does the abundance of Lambert affect the calculation?
> From my crude approximations from your graphs it could
> make $\Sigma\phi\sigma = 1.0 \times 10^{-35}$ sec^{-1} [10 SNU], a drop by a
> factor of 2.4...

Davis did not know who Lambert was - neither did Bahcall. It turned

out that Lambert was spending time at Caltech, but Bahcall's

interest in his work was only aroused after Davis's letter, a

subsequent telephone call, and a chance meeting; as Davis

informed me:

> I called John Bahcall about this, 'I see this paper in
> Nature by this person Lambert says the abundance of
> carbon, nitrogen and oxygen is entirely different
> from what you used, isn't it?' John Bahcall said
> 'I don't know I haven't seen the paper.' So I told him
> where it was...So after John talked to me, either
> the same day or the day afterwards, they went out to
> lunch and a lot of people came along, so he was
> introduced to Lambert... And so they were talking and
> John Bahcall says 'What are you working on, what's your
> field?'. And Lambert said 'Well I've been doing
> the spectrum of the Sun...carbon, nitrogen and oxygen.'
> And John Bahcall says 'What's your name again?' And he
> says 'Lambert'. 'Oh!', he says, 'you're Lambert'.
> From then on Lambert convinced John Bahcall that he had
> it correct....

Although Lambert's results had been published in July 1967, it was

only now that their significance for the neutrino-flux predictions

was realised. Lambert's measurements gave a lower value for

the heavy-element content of the Sun (Z = 0.013). If this value

of Z was used in the solar model, the predicted flux was lowered

and the discrepancy with Davis's result was lessened. Bahcall's

decision to use Lambert's value for the flux predictions was not

entirely inconsistent with his previous work, as he and Shaviv

had already pointed out the large uncertainty in the solar

composition. In view of this uncertainty, it was not unexpected that a lower value of Z had been obtained. However, the choice of the first new measure of the solar composition to come along was not acceptable to at least one solar-model specialist, as we shall see below.

The other new piece of input data to be incorporated into the prediction is relevant to the theoretically-determined cross-section mentioned above, S_{11}. One key parameter used in the theory from which S_{11} is calculated is the half-life of the neutron.[17] In December 1967, a new measurement of the neutron half-life was reported. This result appeared in time for Bahcall, Bahcall, Fowler and Shaviv to add a note in proof to their paper. They pointed out that if the new value of the neutron half-life was used then the extreme value of S_{11} given (S_{11} = 3.70 x 10^{-25} MeV-barns) for the model with the low neutrino-flux prediction was approximately correct. In a further paper, in which the detailed calculation of S_{11} was outlined, Bahcall and May (1968) showed that, with the new measurement of the neutron half-life, S_{11} was even larger (S_{11} = 3.78 ± 0.15 x 10^{-25} MeV-barns). This led to correspondingly lower neutrino flux ($\phi_B 8$ = 0.9 x 10^7 neutrinos $cm^{-2} sec^{-1}$). The new result for the neutron half-life was thought to be preferable to the earlier value because 'it has a smaller quoted probable error and is more recent' (Bahcall and May, 1968: L18).[18]

The combination of using the new solar-composition measurement and the new neutron half-life measurement had the overall effect of drastically lowering the neutrino flux. Bahcall's provisional calculations indicated the flux could be as low as 4.5 SNU (and this was before the uncertainties in S_{17} and S_{34}

were taken into account). Bahcall estimated that with the use of

this new input data there was as yet no contradiction between

Davis's result and his theoretical prediction. Bahcall immediately

contacted Davis with the new results. He was anxious to stem

speculation at the time that there was a contradiction. As we

saw above, Davis himself felt this to be the case and, furthermore,

the scientific media had heard Davis give a presentation at

Yeshiva University where he claimed his result was in conflict

with theory.[19] Both Physics Today[20] and New Scientist[21] ran

articles which suggested that Davis's result contradicted the

theorists' prediction. Bahcall wrote to Davis concerning the

Physics Today journalist:[22]

> She wrote something to the effect that the results
> indicated something may be wrong with the theory of
> nuclear-energy generation...I suggest not, at least
> the results of the just mentioned calculations [those
> which gave a result of 4.5 SNU] suggest it may only be
> a combination of smaller uncertainties.

Bahcall's and his associates' new calculations were published

alongside Davis's paper in the May issue of Physical Review Letters

(Bahcall, Bahcall and Shaviv, 1968). Bahcall et al. claimed that,

with the latest input data, and allowing for the uncertainties

in the cross-section data, and particularly S_{17}, there was no

obvious conflict between theory and experiment. They wrote:

> ...the present results of Davis ... are not in obvious
> conflict with the theory of stellar structure. (Bahcall,
> Bahcall and Shaviv, 1968: 1209).

The result of their latest prediction was that $\Sigma\phi\sigma = (0.75 \pm 0.3)$

$\times 10^{-35}$ sec^{-1} $S_{17}/0.043$ keV b. In other words, the expected

detection rate was 7.5 ± 3 SNU, if Parker's value for S_{17} of

0.043 keV-barns was correct. The reason the result was expressed

in this unusual way (directly in terms of the uncertainty of S_{17})

was because of the increased misgivings that this cross-section

was in error.   As Bahcall et al.wrote:

> If we use...the value of 0.043 keV barns obtained for S_{17}
> by Parker, the most probable predicted counting rate
> [7.5 SNU] is about a factor of 2 larger than the probable
> upper limit set by Davis [et al.]. However, the provisional
> results of Vaughn et al. [Vaughn et al, 1967] suggest that
> Parker's value may require revising downward.(Ibid.,:1209).

The new prediction was almost down to Davis's upper limit and

Bahcall hoped that an error in S_{17} might account for the remaining

discrepancy.   A possible error in S_{17} had been given a renewed

emphasis at this time because of some provisional measurements

made by a group at Lockheed, Palo Alto (Vaughn, Chalmers, Kohler

and Chase, 1967). Their results indicated that Parker's previous

value may have been too large.

As well as reporting their latest model calculations, Bahcall

et al. also specified an absolute lower limit on the theoretical

prediction. They claimed that if Davis failed to confirm this

limit it would mean there was something fundamentally wrong with

the theory.   This lower limit, 0.3 SNU, was the expected signal

from the low-energy pep neutrinos (see Fig. 2.2), the flux of

which was expected to be independent of the detailed solar model

and hence the uncertainties over the solar composition and the

value of S_{17}. In addition, Bahcall et al.reiterated the point made

earlier by Goldhaber, that Davis's result showed that less than

9% of the Sun's energy was derived from the CNO-cycle.

Bahcall's view, that there was no conflict between theory

and experiment, was, as indicated above, different from that of

Davis. This difference was a matter of emphasis.   For Davis the

important theoretical prediction was the one made before his

result was known. It was this flux of neutrinos which the apparatus

had been designed to detect and which he had searched for and

failed to detect. Bahcall, on the other hand, chose to focus

attention on his most recent theoretical calculation. This

difference in emphasis is nicely illustrated by letters which

Davis and Bahcall wrote independently, and at about the same time,

to Hans Bethe. Bahcall and Davis were to present papers in

Washington at a meeting of the American Physical Society, and

Bethe also planned to be in Washington to give a repeat of his

Nobel lecture. It was expected that he might make some comment

about the solar-neutrino results. Davis informed Bethe:[23]

> Unfortunately we have not observed the estimated flux of
> boron-8 neutrinos predicted by current solar model calculations...
>   As you probably know, Dr. John Bahcall has a theoretical
> interpretation of these results in terms of his solar models...

Bahcall, expressed a somewhat different viewpoint in his letter

(written a day earlier):[24]

> As you will readily see, there is no obvious conflict
> between theory and observation (despite numerous and popular
> statements to the contrary) when the uncertainties in both
> are taken into account.

The outcome of the test of stellar-evolution theory as conveyed

in the Physical Review Letters papers was thus rather unclear. Davis

had failed to find the flux originally predicted, but, meanwhile, the

theorists had changed their predictions. Furthermore it looked as

if Davis's apparatus would not be sensitive enough to measure the new

absolute theoretical limit of 0.3 SNU.The situation was summed up in

a review article written by E. Salpeter, a theoretical nuclear astro-

physicist who had close connections with the Caltech group of theorists,

but who had not actually worked on this problem:

> The present state of affairs is most frustrating for all
> concerned. The original theoretical estimate of about 12
> counts per day would have been easily and accurately

> measurable and the theoretical revisions could as easily
> have been up as down. They were down, however, and we have
> seen that a further factor of two down in the theoretical
> estimate is quite possible. Thus, at the present time we
> neither have a positive identification of solar neutrinos
> nor the morbid satisfaction of predicting a scandal in
> stellar-evolution theory! (Salpeter, 1968:101).

The critical test of stellar-evolution theory which Bahcall had

promised in 1964 seemed to have vanished along with the neutrino

flux.

In summary then, it can be said that the publication lag in

Davis's results gave time for Bahcall to re-examine his prediction.

By the time Davis's result was published, the conflict had been

minimised by the production of a new lower prediction (made with

the choice of new input data) and by attention being drawn to

parameter uncertainties, and, in particular, uncertainties in

the value of S_{17}. It has been argued that it may have been

important to Bahcall at this stage for the conflict with the

prediction to be minimised   since he had staked his reputation,

and the AEC had staked $600,000 on the prediction being correct.

Other Reactions to Davis's Result

Bahcall was, of course, only one of a number of scientists

to have made predictions of Davis's expected signal. Fowler,

Sears, Reeves[25], Cameron and Iben had all also been involved

to a greater or lesser extent, as we saw in Chapters 4 and 6.

It turns out that Fowler, Sears and Cameron, although not always

in complete agreement, were in general sympathy with the approach

Bahcall had taken.   Iben, on the other hand, had a very different

interpretation of events - he felt that Davis's results were in

clear conflict with the theory and further he was prepared to

challenge Bahcall on this point.   We will look at the attitude

of each of these scientists in turn.

Fowler

Fowler's reaction was particularly important given his institutional role at Caltech and his previous involvement with the experiment. It is no surprise that he was in almost complete agreement with the theoretician he had hired to work on the problem - John Bahcall. As a co-author with Bahcall of the Physics Letters paper in which the uncertainties in the cross-sections were discussed, he shared Bahcall's pronouncement that the uncertainties in the nuclear-physics cross-sections made it difficult to give a definitive theoretical prediction anyway. By the time Bahcall et al. had published their Physical Review Letters paper in which the prediction had been lowered by the inclusion of new input data, Fowler, in his first major review of the consequences of Davis's result, was able to write about the 'precarious agreement'[26] between theory and experiment. Also, as mentioned above, Fowler gave the impression that this latest prediction had been arrived at at about the same time as Davis's result became known. He further bolstered the impression of agreement by pointing to the exclusion of the CNO-cycle which he felt to be a 'significant and satisfying result'.[27]

The general impression conveyed by Fowler was that, although Davis's result was not quite what had been expected, nevertheless there was no need for serious concern.

Sears

Since computing the solar models upon which Bahcall had based his 1964 prediction, Sears had not had much involvement with solar neutrinos. He had computed some more solar models in 1965 with Weymann (Weymann and Sears, 1965), but his active participation had waned after he left Caltech for the University

of Michigan.   Davis had, however, sent Sears preprints of the

Physical Review Letters papers and Sears had written back comm-

enting on the theory. He certainly felt that Davis's results

had not confirmed the original prediction. As he wrote:[28]

> I suppose we would have sat back with ill concealed smirks
> if your experiment had agreed with our predictions...In any
> case your experiment has certainly got us with our backs
> to the wall.

However, Sears seemed to be in agreement with what Bahcall had

subsequently done. As he went on to comment:

> I have been following the effects on the predicted B^8 flux
> of the revisions that have come along, and my crude estimates
> agree well with the detailed work of John Bahcall. One
> revision that may lower the solar opacity and thus the B^8
> flux, which I don't think he knows about, is a collective
> interaction amongst electrons...But the effect is probably
> negligible.

Although Sears does not say whether or not he agreed with

Bahcall's conclusion that there was no obvious conflict between

theory and experiment, it is evident that he too was thinking

of ways of reducing the theoretical prediction in order to minimise

any discrepancy.   In this regard, his position seems to have been

similar to Bahcall's.

Cameron

Despite Bahcall's and Fowler's claims that the standard

theory was not yet in trouble, other theorists found some cause

for alarm. For instance, Cameron and his student Ezer, on learning

of Davis's results, produced the first proposed modification to

the standard model.   They suggested that there might be circulation

currents in the solar interior which meant that hydrogen from the

outer layers was continually mixed with the core.[29]   This would

increase the energy production, expand the core and lower the

central temperature. The net effect would be a reduction of the

highly temperature sensitive B^8-neutrino flux. They estimated

that this mixing process could produce a B^8 flux as low as 2.5×10^6

neutrinos $cm^{-2} sec^{-1}$, which was in the region of Davis's upper

limit. Also, further small adjustments in the input data, such

as the solar composition and nuclear parameters, could produce a

value for the predicted flux that was below Davis's upper limit.

Thus, this model could be made consistent with the experimental

result.

Although he had proposed this model, Cameron himself hoped

that such a radical way out would not be needed. This view was

strengthened after the appearance of the Bahcall et al. paper

in May 1968. As he wrote to Bahcall at the time:[30]

> Personally I hope you are right and that the negative
> results obtained by Davis will turn into a positive
> one consistent with the new values of the nuclear reaction
> rates and abundances which you have discussed. The
> interior circulation may be the remaining 'ace in the hole'
> which might have to be invoked if the neutrino flux turns
> out to be lower than can be accounted for from your recent
> modifications to the various solar quantities.

Thus, Cameron was hopeful that a contradiction could be avoided,

but felt that in the circumstances an investigation of other ways

around the problem was warranted.

Iben and the Controversy over Whether or Not there was a Discrepancy

One theorist disagreed enough with Bahcall's interpretation,

that the theory was not yet in trouble, to take issue publicly

with him. This theorist was Icko Iben who, it will be recalled,

had been a member of the original Caltech team which had made the

first attempt to produce an accurate prediction of the boron-

eight neutrino flux. Iben had subsequently left Caltech for

MIT. As a stellar-model specialist who had also worked on the

nuclear-physics part of the neutrino-flux prediction (the calculation

of the Be^7 screening effect mentioned in Chapter 6), Iben was

particularly well qualified to evaluate the theoretical implications

of Davis's result.

In a paper submitted to Physical Review Letters, in July 1968,

Iben reported the results of an extensive investigation of the

relationship between the neutrino flux and a variety of input

parameters. He found results which he felt were, at some points,

at variance with those found by Bahcall et al. The most

important difference was that Iben, unlike Bahcall, felt that the

standard solar-input composition parameters were not consistent

with the Davis result. Although he thought a contradiction could

still be avoided if the cross-section data were pushed to the

limits of their error range, such a possibility seemed rather

unlikely. His main disagreement with Bahcall centred over the

value of the primordial helium abundance of the Sun, Y, that was needed

in a solar model consistent with Davis's result. This value

should agree with observational determinations of Y (such as

made from other stars). Bahcall et al. had concluded that $Y \simeq$

0.22 ± 0.03, a value just consistent with observation. Iben's

conclusion was, however, different; as he wrote:

> With the standard choice of solar input parameters, the
> Davis, Harmer, and Hoffman limit implies an upper bound
> on the sun's initial helium abundance that is small
> compared with the helium abundance estimated for other
> galactic objects. The upper limit on Y...required for
> consistency with the Davis, Harmer and Hoffman limit
> is $Y_o \simeq 0.16 - 0.17$. On the other hand, almost every
> attempt to estimate Y for galactic objects other than
> the sun has led to values in the range $0.2 - 0.4$...
> (Iben, 1968:1208)

In order to understand the difference between Bahcall and

Iben on this point it is necessary to delve a little into the

procedure of solar-model construction. To produce a solar model

the primordial chemical composition of the Sun is required. The chemical

composition is given in terms of the fractional abundances, X (the

hydrogen abundance), Y (the helium abundance) and Z (the heavy-element

abundance). As X + Y + Z = 1, only two free parameters are needed.

For computational convenience the two parameters which are usually

used are Z/X, and Y. Once the initial composition has been set the

solar model can be evolved over the age of the Sun (.4.7 billion years).

The correct solar model must, after this period, be able to produce

the observed luminosity. If it fails to do so it is conventional

to vary slightly one of the composition parameters until a match with

the luminosity is achieved. The approach followed by Sears (1964),

Bahcall and Shaviv (1968), and Bahcall, Bahcall and Shaviv (1968) was

to adopt an initial value of Z/X, given by current spectroscopic

observations of the solar photosphere, and to vary Y until a match

with the luminosity was achieved. In this approach it was assumed

that the value of Z/X obtained in the photosphere was the same as

the primordial value, and that this value was better known than Y

(which could not be obtained from the photosphere). Of course, the

value of Y chosen to fit the model must be consistent with observations

of Y made from galactic objects, and values of Y derived from cosmology.

Bahcall, Bahcall and Shaviv claimed that, with Lambert's value of

Z/X = 0.019, they needed a value of Y=0.22 to produce the present lumin-

osity. This value of Y was just consistent with observations.

Iben, on the other hand, followed a different approach. He felt that

Z/X was not that well known as emphasised by the recent revision in the

value made by Lambert. He preferred to use Y as his composition parameter

and vary Z. [31] He found that, for a reasonable value of Z, consistency with

Davis's upper limit for the neutrino flux could only be obtained with

Y=0.16-0.17, which was much lower than observation permitted. In his paper,

Iben pointed out that the only reason Bahcall, Bahcall and Shaviv

had managed to obtain a higher value of Y was because they had

chosen Lambert's value of Z/X. He felt that this spectroscopic

measurement might not be the correct choice of Z. As he wrote:

> A Z estimated from spectroscopic data is therefore not
> necessarily the appropriate choice for the opacity
> parameter Z.
> The value of Y = 0.22 quoted by Bahcall, Bahcall
> and Shaviv is the result of a specific choice of Z...
> Y ≃ 0.22 is $\underline{not}$ consistent with the Davis, Harmer and
> Hoffman limit. An insistence on consistency with this
> limit, rather than on insistence on a particular choice
> for Z, leads instead to an upper limit of Y_o ≃ 0.16 - 0.17.
> (Iben, 1968: 1209, his emphasis).

The implication of Iben's argument was that Bahcall $\underline{et}$ $\underline{al}$.

were shirking from the inevitable conclusion of their

calculations which was that a model consistent with reasonable

values for the solar composition and with observations of the

neutrino flux could not be obtained. Thus, Iben felt that there

$\underline{was}$ a contradiction between the standard model and Davis's result.

This contradiction was most clear if the helium abundance, rather

than what he considered to be the more uncertain heavy element

to hydrogen ratio, was taken as the composition parameter.

Some of Iben's motivation for writing his paper can be seen

from the following comment which he made to me:

> Well anyway, along came the first experiment and a rather
> dramatic discrecpancy...And immediately people started
> counting, finding effects that would bring it down...
> Bahcall wrote something saying there was no discrepancy,
> everything is still OK...So here we are with the experiment
> out and the theoretician on the team arguing that nothing
> was wrong. So I tooled up and constructed a whole bunch
> of solar models. I wrote a $\underline{Physical\ Review\ Letter}$...and
> that got bombed by John who said it had all been done.

Iben's paper had initially been sent to Bahcall for refereeing

and Bahcall had recommended against publication in $\underline{Physical}$

$\underline{Review\ Letters}$. Iben had been very annoyed by this. He had

immediately rung Bahcall on his home number to complain and thus

woke Bahcall in the early hours of the morning-in his annoyance

it seems   Iben had forgotten the time difference between the

East and West coasts!   Iben's strong feelings on the matter can

be seen from the letter he wrote to the Editor of Physical Review

Letters requesting that his paper be sent to another referee.

He wrote:[32]

> I can only conclude   from the tone of his report
> that the referee considers the subject of solar neutrinos
> to be his exclusive preserve and feels that I am poaching
> on his territory.
> Much of the theoretical work that was instrumental
> in persuading governmental agencies to finance the Davis
> et al.experiment has been presented to the scientific
> community in a deceitful manner, with high claims made
> for a $\Sigma\phi\sigma$ that is an order of magnitude larger than the
> upper limit resulting from the experiment.
> The article published by Bahcall et al. ... is a continuation
> of this deceit. All the previous bold predictions are
> forgotten and it is proclaimed that the Davis et al. limit
> is consistent with theoretical models. In order to achieve
> this proclaimed consistency, one of the parameters in the
> model calculations (the choice of Z) is changed from what
> had formerly been considered most likely.   The work of the
> one spectroscopist [Lambert] , whose estimate of Z happens
> to be appropriately low, is cited to make the work appear
> respectable and unforced.
> A careful look [reference to his own manuscript]
> shows that the B^2S [Bahcall, Bahcall and Shaviv, 1968]
> results are not consistent (as advertised) with the Davis
> et al. limit. I submit that this is quite unscientific
> reporting. I feel that the Physical Review Letters is, in
> good conscience, obligated to give equal time to an
> astrophysicist who can correct the misconception.

Accompanying this letter were detailed comments (many acrimonious

ones) on the referee's report.

Iben's comments to the Editor state forcefully his view

that Bahcall had chosen input data which led to a lower prediction.

Iben felt the real consequences of Davis's results for the theory

could be seen from his own paper which used the helium abundance

rather than a value of Z/X based on the work of Lambert.   It is

clear also from his letter that Iben felt that Bahcall was

reneging on the theoretical prediction made before the experiment
was undertaken.

Unfortunately Bahcall's original comments on Iben's paper
are not available. However, some of what Bahcall's riposte might
have been can be seen from his comment on the matter made to me
in 1978. In referring to the difference between his approach and
Iben's, he told me:

> It's a difference in philosophy but there is nothing
> scientifically different...The traditional thing, or at
> least the thing I have always done, and people working
> with me have therefore done, is to take the ratio of heavy
> elements to hydrogen from observations of the surface of
> the Sun, supplemented by guesses as to what the neon abundance
> is from other stars. Icko has assumed instead that he knew
> the helium abundance. We calculate the helium abundance...
> I think it is more plausible to take for the Sun the heavy
> element to hydrogen ratio, because we observe that pretty
> much in the surface of the Sun...The helium abundance is
> the cosmological one, I don't like cosmology so I didn't
> do it that way...

From this comment it would appear that Bahcall's preference
for the Z/X parameter was quite reasonable. As far as he was
concerned, this was the standard approach (and it is true that
Sears had used this parameter for his models) and furthermore
he regarded the Y parameter, which was not directly observable in
the Sun, to be even less reliable since its value was derived, in
part, from cosmological arguments.

In order to try and resolve Bahcall's and Iben's disagreement,
Iben's paper was sent to another (anonymous) referee for adjudication.
This referee recommended the paper be published and commented.[33]

> I believe that the author, after setting aside the polemics,
> is entirely correct in the scientific conclusion of his
> paper and his reply to the first referee...
> The author is also correct in stating that some of the
> results are in contradiction to the conclusions offered by
> Bahcall et al. in their paper...Since this whole matter is
> of great current interest in physics and in astrophysics,
> I believe that a rapid publication of a note emphasizing
> these differences could be justified...

Iben's paper was finally published in Physical Review Letters

in October 1968. However, his battle with Bahcall was not yet

over. As a sequel to the letter, he prepared a longer paper giving

his detailed results. This he submitted to the main journal in

the field - The Astrophysical Journal. However, Bahcall was again

the referee and again he turned the paper down. By this time Iben

had become so disenchanted, that he took up the offer of a MIT

colleague who was editor of Annals of Physics, to submit his paper

there instead. The paper was duly accepted for publication in

that journal (Iben, 1969). However, as that journal was not read

by astrophysicists Iben felt that, in effect, his work had been

ignored, He felt some bitterness about this, as he told me:

> Here is a field in which one person has essentially wiped
> out the opposition. In the minds of the whole community of
> astronomers, the only guy who has ever done anything worth
> while is John Bahcall. And I don't deny that he should
> receive full credit for the whole thing. On the other hand,
> that's crappy science, to wipe out the opposition, in
> effect, just out of pique.

Iben's feelings at the time are also reflected in a letter which

he sent to Davis; he wrote:[34]

> I really don't know why I'm having this much trouble with
> getting my stuff on solar neutrinos published, but I've
> become pretty disgusted with the whole affair.

Although Iben and a student did calculate some further solar

models (Abraham and Iben, 1971), his active participation in the

field ceased from this point. Since within a year, Bahcall was,

himself, to admit to there being a contradiction the differences

between his view and Iben's, in hindsight, appear to be rather less

vital than they were in 1968.[35] However, the vociferousness of

the controversy for its short duration indicates that an issue of

some substance was at stake.

This particular episode is of considerable interest because

it is one of the few cases of acrimonious controversy in the

solar-neutrino field. Its implications will be discussed more fully in Chapter 10. The main conclusion to be drawn here is that it shows that Bahcall's interpretation of Davis's result was not the only one which could be legitimately held at the time. Iben's position seems to have been equally viable, as indicated by the endorsement given it by the anonymous referee.

Thus, in the year following the announcement of Davis's result, the theorists were in some disarray. Bahcall, the theorist most closely associated with the project, maintained that there was no conflict between theory and experiment. Others, however, thought that the situation was more serious than Bahcall was willing to concede and at least one theorist, Iben, was prepared to argue that there was a contradiction between theory and experiment. It seems that whether or not there was a contradiction between theory and experiment depended on which theorist's view was believed. And, as we have seen, there were reasonable scientific arguments in support of each side.

Contradiction and consistency in this case seem to have been (to use current sociological parlance) matters for negotiation.[36] Scientific argument alone could not seem to settle the issue. The outcome of the experimental test of the theoretical prediction can be said to have been socially constructed. Bahcall, with his strong commitment to the 'prediction' being correct, was able to claim the experimental result was not in conflict, whilst Iben, who was less personally involved with the theoretical prediction, was able to claim that a contradiction existed.

PART II. THE EVENTS OF 1968-1978

Bahcall Acknowledges the Existence of a Discrepancy

Throughout 1968, Bahcall still maintained that there was no

fundamental contradiction between theory and experiment. As before,

he continued to base this view largely on the expectation that

S_{17} would have to be revised downwards.

In October 1968 , S_{17} was indeed revised downwards, but only

very slightly. Parker, under the stimulus of the new measurements

of S_{17} by Vaughn et al. (1967) had looked again at his 1966 data.

He now found (Parker, 1968) that the best value for S_{17} was

0.035 keV-barns, as opposed to the old value of 0.043 keV-barns.

This small downward revision was caused, partly by changes in

the standard value of an experimental parameter which had been

used to normalise the low-energy extrapolation[37], and partly by

a reanalysis of the effect of a low-lying resonance on the extra-

polation.[38] Parker now took a slightly different extrapolation

and obtained the new lower value.

The resulting effect on the solar-neutrino flux prediction

was reported in a paper, submitted to The Astrophysical Journal

in October 1968, by Bahcall, his wife Neta, and a solar-model

specialist Roger Ulrich[39] (Bahcall, Bahcall and Ulrich, 1969).

They estimated that, with all the most likely values for the input

parameters, Davis should expect a rate of 6 SNU. The reduction

of 1.5 SNU since the May 1968 prediction was not produced by the

new value of S_{17} alone. A new value for the rate of electron

capture by Be^{7} was also used. As was mentioned before (in Chapter

6) Iben had recalculated this rate taking into account bound-

electron capture (Iben, Kalata, Schwartz, 1967). Bahcall and

a colleague at Caltech, Charles Moeller, (Bahcall and Moeller, 1969),

had now refined Iben's calculation and it was their new value for this reaction rate which was used in the latest solar-model calculations. As the new value was very slightly larger than the old one, it led to a correspondingly small reduction in the B^8-neutrino flux.[40]

The conclusion which Bahcall, Bahcall and Ulrich reached in comparing their latest model prediction with Davis's results, was similar to that drawn earlier by Bahcall, Bahcall and Shaviv. They stated:

> There is as yet no obvious conflict between the theory of stellar structure and the experimental results of Davis et al. (1968) (Bahcall, Bahcall and Ulrich, 1969:567)

Again, this conclusion seemed to be based on the uncertainty in S_{17}. As they wrote:

> ...the cross-section factor S_{17}...should be subject to further careful experimental and theoretical analysis. We note that the quoted experimental values for this cross-section factor have varied by a factor of 2...in recent years. It is probably the most difficult cross-section measurement...and it is certainly the most important for solar-neutrino experiments. (Bahcall, Bahcall and Ulrich, 1969: 566-7).

Bahcall et al. went on to list further uncertainties in the low-energy extrapolation of this cross-section and pointed out that if Tombrello's 1965 theoretical calculation of S_{17} was used (this gave S_{17} = 0.012 keV-barns - see Chapter 6) then the theoretical prediction could be as low as 3 SNU, which would not be in disagreement with Davis's upper limit.

Although Bachall's main hope for removing the remaining discrepancy lay with the S_{17} cross-section, he continued to explore other uncertainties in the solar model. For instance, Bahcall, Bahcall and Ulrich (1969) attempted to see how arbitrary modifications to the equation of state (the Sun was assumed to be

described by an ideal-gas law equation of state - see Chapter 9

for more details) and the opacity would affect the neutrino flux.

They could not find any definite plausible effect that lowered

the neutrino fluxes by the requisite amount.

The extent to which Bahcall, in the summer of 1969, hoped

that the remaining discrepancy between theory and experiment would

be explained by an error in S_{17} can be seen from an article of

his published in August 1969, in <u>Physical Review Letters</u> (Bahcall,

1969b). The purpose of this article was largely to review detection

rates for a variety of new experimental targets that had been

proposed. Bahcall's calculations of these detection rates were

all based on the lower value of S_{17} (S_{17} =0.012 keV-barns)

estimated by Tombrello from his work on Li^7. As Bahcall wrote:

> In order to obtain consistency with the experimental results
> of Davis, Harmer and Hoffman we have adopted the value of
> S_{17} determined from experiments involving ^7Li (which is a
> factor of 3 smaller than the 'standard' value of S_{17}). We use
> this indirectly determined S_{17} because it enables us to
> make somewhat more plausible estimates for the rates of
> future solar neutrino experiments not because of any
> criticism of the 'standard' measurement. (Bahcall, 1969b:252)

Despite the disclaimer concerning criticism of the 'standard' S_{17}

measurement, some criticism of it must have been implied by the

fact that it was this parameter which had been changed. It

seemed that Bahcall had such confidence in the standard value of

S_{17} being in error that he was starting to make computations

based upon a lower value.[41]

Although Bahcall could maintain his view that there was no

fundamental discrepancy well into the summer of 1969, events

which occurred in the autumn of 1969 forced him to change his mind.

Firstly, theoretical work on the solar opacity indicated that the

previous value was in error and that the new opacity would produce

a _larger_ neutrino flux. Secondly, and more importantly, Kavanagh

reported on his new measurements of S_{17} and found a result which

supported Parker's value.

The new work on the opacity was carried out by a student of Iben's

at MIT, W.D. Watson.[42]   His thesis had been on radiative transfer

processes in stars and, as such processes governed the central

temperature, his work was of great importance for solar-neutrino flux

calculations. Watson looked at the effects on the neutrino flux of

several refinements which he had made to the opacity calculations.

In particular, he drew attention to three effects.

Firstly (Watson, 1969a), he found that the inclusion of auto-

ionisation lines increased the opacity. Autoionisations are parti-

cular lines observed in optical spectra that have a peculiar shape

caused by the mixing of discrete and continuous states of an atom

such that the atom , spontaneously ionises. Their effect on the

solar opacity had previously been ignored. The increase in the opacity

which stems from autoionisation lines enhances the boron-eight neutrino

flux. Watson (1969b) also pointed out that the controversy occurring

at the time over the correct value of the iron abundance in the Sun

would have an appreciable effect on the boron-eight flux prediction.[43]

It seemed likely that the previously used value for the iron abundance

would have to be revised upwards by a factor of ten. In which case,

this would again lead to an increase in the expected neutrino flux.

Watson (1969c) also investigated another atomic-physics effect. This

was the so-called plasma effect, and concerns the influence of

collections of electrons on the opacity. This tends to decrease

the opacity and thus to some extent off-sets the increases in the

flux produced by the other two effects.

The detailed influence of Watson's findings on the solar-neutrino flux predictions was considered by Bahcall and Ulrich (1970). Using all the latest input parameters, they found a flux of 7.8 SNU, but this could easily be higher if the iron abundance needed to be revised further, as seemed likely. It was also in this paper that the first announcement of Kavanagh's measurements of S_{17} was made. The upwards trend in the theoretical prediction produced by the work on the opacity and the closing off of the S_{17} loophole led Bahcall and Ulrich to write:

> A comparison...with the experimental results of Davis et al.
> shows that a sizeable discrepancy exists between theory and
> experiment. This discrepancy has been strengthened greatly
> by the results of the beautiful experiment of Kavanagh et al.
> (Bahcall and Ulrich, 1970: L58).

Thus, two years after the results of the experiment were first known, Bahcall was, at last, prepared to accept that a 'sizeable discrepancy' existed.

1970-1976 The Years of Crisis

The discrepancy between theory and experiment reported by Bahcall and Ulrich (1970) became more dramatic in 1971. It emerged that there had been a coding error in Ulrich's computer programme which seemed to have been caused by the move from Caltech to UCLA (where Ulrich had just taken up a post). This meant that the previous prediction had been about 30% too low. Bahcall and Ulrich (1971) now predicted a signal of 9 ± 5 SNU.[44] In addition, in 1971 Davis reported results which were consistent with a signal of 1.5 SNU. The combined effect of the larger prediction and smaller result led Bahcall and Ulrich to write:

> This observation [of Davis] is in conflict with the theories
> of stellar evolution and nuclear fusion in stars as applied
> to the Sun. (Bahcall and Ulrich, 1971: 593).

The emphasis which Bahcall now placed on the contradiction

between theory and experiment can be seen from a talk given

to the American Astronomical Society in 1970. This talk, entitled,

'Some Unsolved Problems in Astrophysics', was the Helen B. Warner

Prize lecture for 1970. This prize was awarded for outstanding

work done by an astronomer under 35 while resident in the U.S.

It was the first time the prize had been awarded to a theoretical

physicist and Bahcall's work on solar neutrinos no doubt contri-

buted to the award.[45]   Bahcall, in his talk, referred to the

'.discrepancy between theory and observation   which   cannot be

explained by known uncertainties in either'.   (Bahcall, 1971:283).

Bahcall stated that he felt that the theory of stellar interiors

was 'probably wrong' and he cautioned his fellow astronomers

against the use of this theory. Bahcall's argument was, that since

the Sun was the best star on which to test the theory (as there

was more accurate information about the Sun than any other star),

the failure of the theory in this case pointed to the problems

of applying the theory to stars where the uncertainties were all

the greater.

Bahcall's new emphasis on the discrepancy seemed, however,

to take some time to percolate through into the scientific media.

For instance, reports in the scientific press in 1971 of Davis's

latest results (1.5 ± 1 SNU) played down any discrepancy. In a

report in Science it was claimed:[46]

> Although their [Davis et al.'s] recent rate is a trifle low com-
> pared with that expected from the best models of the Sun,
> it should cause no serious theoretical problems.

In New Scientist it was reported:[47]

> They [Davis et al.] calculate a corrected solar neutrino
> capture rate per chlorine-37 atom of $(1.5 \pm 1.0) \times 10^{-36}$
> per second. This is the first positive signal to emerge

above the background. It can be compared with the current theoretical calculation of 10^{-35} per second. Although slightly low, the experimental result probably will not cause any serious problem.

These reports seem to have based their conclusions on the fact that Davis (1971), with the introduction of the pulse-rise discrimination facility reported for the first time a positive signal and an error. However, as we saw in Chapter 7, this positive signal was short-lived and Davis (1972), with several very low runs, once more expressed his results as an upper limit.

The view that there was a discrepancy between theory and experiment was, however, becoming prevalent in the technical literature on the problem. For instance, Abrahams and Iben (1971) presented solar-neutrino flux predictions which gave similar results to those obtained by Bahcall and Ulrich, and they referred to:

> ...what is now generally recognised as a rather clear discrepancy between theory and observation (Abraham and Iben, 1971: 157).

In 1971 Bahcall and Sears were asked to review the subject of solar neutrinos for the <u>Annual Review of Astronomy and Astrophysics</u>. They commenced their review with the statement:

> The most important fact about the subject we are reviewing is that there is a large discrepancy (at the present writing) between calculation and observation. (Bahcall and Sears, 1972: 25).

The conflict between theory and experiment worsened in 1972 as Davis started to report even lower results. Bahcall's assessment of the situation in early 1972 was, as he reported to the Irvine Conference:[48]

> It seems to me that the situation is so grossly discrepant with the theory that we are clearly faced with the question of something basically being wrong either in the physics or the astrophysics.

He expressed a similar opinion in an article written at about this time:

> The most recent experimental results...have established what

appears to be a scientific crisis...(Bahcall, 1973: 381)

He went on to write:

> The present crisis can be appreciated by comparing Davis's
> limit [1 SNU] with the expected counting rates...It is
> clear that something very serious is wrong with the theory⌐
> (Bahcall, 1973: 383).

The term 'crisis' appeared in correspondence at the time[49]

and there was even talk of a revolution occurring in stellar-

evolution theory.[50] It thus appears that, by 1972, it was widely

believed that there was a serious problem with Davis's solar-

neutrino results which might lead to important theoretical revisions.

Now that Bahcall was convinced that there was a contradition

between theory and experiment, he began to reiterate the arguments

he had made in 1965 concerning the crucial nature of the experiment.

For instance, he wrote:

> The Brookhaven Solar-Neutrino experiment of Davis and his
> colleagues is therefore a crucial one for the theories of
> stellar evolution and of nuclear energy generation in stars.
> One might even hope that the solar neutrino problem will
> play somewhat the same role in the theory of stellar evolution
> as the hydrogen atom did in quantum mechanics. (Bahcall,
> 1973: 381, his emphasis).

The extent of the transformation in Bahcall's viewpoint,

from his earlier attempt to play down the discrepancy by stressing

the leeway in the prediction to his new view of a serious contra-

diction, can be seen from a comment which Bahcall made to Davis in

June 1973. In a letter, Bahcall criticised a recent conference

presentation by Davis. Bahcall wrote:[51]

> The remark you made at the beginning is misleading: 'though
> with variations in the standard models...the flux can be
> reduced below 1 SNU.' No model with a consistent physics
> gives 1 SNU. As you know, one has to introduce special
> assumptions about the physics that are revolutionary from
> the standpoint of physics and stellar evolution. I think
> you would be doing a disservice to your experiment, which
> tests astrophysics where the predictions are most inflexible,
> if you give the opposite impression. I am sure Schwarzschild
> feels the same way. (My emphasis).

Bahcall's mild rebuke to Davis here, for failing to stress the

contradiction between theory and experiment, is ironic when we

recall that Davis earlier (in January 1968) had chastised Bahcall

for his attempts to argue there was no such contradiction!

The view that there was a serious discrepancy was by now

reflected in the scientific media as well. For instance, in Science

it was reported:[52]

> The implications of Davis's result, if sustained by further
> experiment, is that either existing solar models or some
> fundamental nuclear theory as applied to the Sun are seriously
> in error.

With the standard theory so obviously in conflict with the

experiment, numerous suggestions appeared as to where the problem

might be. Apart from the questioning of the radiochemistry at the

Irvine conference, as discussed in the preceding chapter, renewed

attention was focussed on the nuclear-physics and astrophysical

components of the calculation. For instance, the suggestion was

made at the Irvine conference that there might be a low-energy

resonance in the $He^3 + He^3$ reaction. This might distort the low-

energy extrapolation and hence affect the value of S_{33}. On the

astrophysical front, it was suggested at Irvine that the Sun might

be 'mixed-up'. That is sudden convection currents in the Sun

might periodically mix material from the outer layers in with the

core. This would lead to an expansion of the core and lower the

central temperature enough to deplete the boron-eight flux. This

suggestion is similar to the earlier one by Ezer and Cameron (1968)

only they had proposed mixing over the whole life of the Sun,

rather than the sudden mixing now being discussed.

Another important class of explanations lay in the direction

of neutrino physics. It was possible that there was some property

of neutrinos which took on importance in their long journey from

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the Sun to the Earth.   One idea , which had some currency, was

that of neutrino oscillation. For instance, it was known that there

were two types of neutrino - electron neutrinos and muon neutrinos.

If electron neutrinos could change state and spend some time as

muon electrons and vice versa this could explain the low result.

Half of the electron neutrinos produced by the Sun which were

expected to be detected in Davis's experiment, would have changed

into muon neutrinos by the time they reached the Earth and hence

would escape detection. The various explanations which have emerged

over the years to explain Davis's result are discussed further in

Chapter 9.

The different disciplinary components of solar-neutrino astronomy,

as exemplified by the different domains called upon to explain the

outcome of Davis's experiment, were widely thought to be a barrier

to the solution of the problem.   It was felt that scientists

working in one specialty. tended to disregard the problem, believing

it to be the responsibility of some other specialty.   As Cameron .

commented, in his summing up of the Irvine conference.[53]

> Thus a possible solution of the puzzle may lie in nuclear.
> physics, in astro-physics or in neutrino physics. There is
> a tendency for scientists working in any one of these fields
> to feel that the chance that his field is the culprit is
> so remote that one of the other fields must be involved.
> It is always easy to blame the other fellow , particularly
> if you do not know much about his specialty.   As long
> as this general attitude holds, work on the solar-neutrino
> problem is bound to go forward somewhat sluggishly.

The view that astronomers blame physicists and vice versa was

also shared by Bahcall. As he wrote to a colleague at the time:[54]

> Almost universally, astronomers believe that the difficulty
> must lie in the physics and physicists think the difficulty
> must be in the astrophysics.

It can be seen that the suspicions of the theoreticians

concerning the correctness of the experiment and the suspicions

of the experimenter concerning the correctness of the theory had

by now spread amongst different groups of theorists, with each

group tending to blame others for the problem. The interdisciplinary

tensions which the solar-neutrino problem had aroused are discussed,

in their own right, in the paper presented in Appendix II (Pinch,

1981a).

In 1973 two further corrections were made to the standard

theory which both had the effect of lowering the predicted fluxes .

somewhat.   Gari and Huffman (1972) recalculated the p-p rate

(S_{11}) taking into account meson-exchange effects which had been

ignored in the previous calculations. They found a lower value

of S_{11} and this reduced the predicted detection rate by 1.65 SNU.

Also, it was discovered that the effect of one of the corrections

to the opacity made earlier by Watson (the autoionisation effect)

had been overestimated. New opacity calculations by the Los

Alamos group revealed a reduction of about 2 SNU,   The

incorporation of these new calculations into the solar-neutrino

flux computation   was reported in a paper   written by Bahcall and

Ulrich in association with the Los Alamos opacity group (Bahcall,

Heubner, Magee, Merts, and Ulrich, 1973).They estimated an overall

detection rate of 5.6 SNU.   A prediction which they still claimed

to be 'in strong disagreement with the experimental results'.

(Bahcall et al., 1973:2).

Over the period 1973-6, the main theoretical effort was

directed towards the attempt to explain the discrepancy. During

this time, the explanatory candidates to emerge from the Irvine

conference were investigated and, by and large, rejected. In

addition, a whole range of non-standard solar models had by now

been proposed. None of these gained widespread acceptance (see

Chapter 9). Also, by now, most people were willing to accept that

Davis's experiment was, itself, not the cause of the problem

(see previous chapter).

Thus, by 1976, the solar-neutrino problem seemed more

imponderable than ever. Bahcall and Davis summarised the situation

with a joint publication in Science.[55] They wrote in the introduction

to their paper:

> For the past fifteen years we have tried, in collaboration
> with many colleagues in astronomy, chemistry and physics,
> to understand and test the theory of how the sun produces
> its radiant energy...All of us have been surprised by the
> results; there is a large unexplained disagreement between
> observation and supposedly well established theory. This
> discrepancy has led to a crisis in the theory of stellar
> evolution; many authors are openly questioning some of the
> basic principles and approximations in this supposedly
> dry (and solved) subject. (Bahcall and Davis, 1976: 264).

Again , in this article the, by now,familar theme

that the experiment was of a crucial nature was reiterated. Also,

the phenomenon of physicists blaming astronomers and vice versa

was stressed. In order to settle the issue of whether the solar-

neutrino problem was a result of faulty astronomy or faulty physics,

Bahcall and Davis called for a new experiment to be carried out

(either the gallium or lithium experiment). Because such experiments

were sensitive to the lower-energy neutrinos (pep and pp neutrinos)

the fluxes of which were independent of detailed astrophysical

models, they should be able to distinguish between whether the

problem was in the physics or astrophysics. For instance, if the

lower-energy neutrinos were found in the correct quantities, then

this would indicate that the detailed solar model was incorrect and

that the problem was in the astrophysics rather than the physics,

On the other hand, a failure to detect the lower-energy neutrinos

would indicate that something fundamentally was wrong in the physics

(perhaps neutrino oscillation).[56]

The emphasis which Bahcall now placed on the contradication

can be seen to be a useful rhetorical ploy in terms of arguing

for new experiments. Bahcall's (and Davis's) argument was, that

because the chlorine experiment was in conflict with the theory

new experiments were required to solve the problem. Any

embarrassment caused by the previous failure of the theory was in

the past. Bahcall had not lost his credibility as a theorist.

He now appeared to have taken Feynman's advice and emphasised the

discrepancy as an indication of the importance of the Davis result.

Thus, by 1976, Bahcall seemed to be once more furthering

his theoretical ambitions by starting to push for new experimental

approaches .xperiments which would give him, as a theorist, further

opportunities to make; predictions and produce the associated

theoretical work. In essence, Bahcall's arguments for new exper-

iments now were similar to those which he had put forward in 1964.

Whilst, in 1964, it seemed thatisizeable prediction was needed

in order to show the feasibility of the experiment, now the existence

of a sizeable discrepancy would help in the battle for the funding

of ˛ second—generation experiments.

1976-1978 The Argument over Whether or Not There is a Discrepancy

Reappears

Although Davis quoted an upper limit of 1.5 SNU in 1976, his

latest data which were shown in the Science article (see Fig. 8.1)

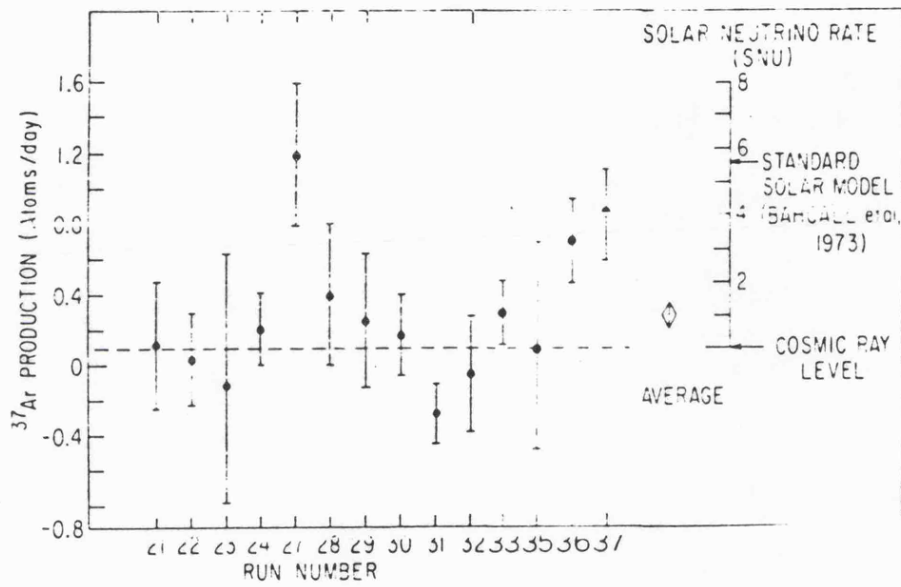seemed to indicate an upward trend. This led some people to wonder

Fig. 8.1 Davis's Data in 1976 (taken from Bahcall and Davis,1976).

whether he was, at last, detecting something. For instance, in the

commentary in New Scientist on the Science article, it was stated:[57]

> ...for most of the last decade he saw nothing...But in the last year his neutrino count has risen; the last three counts each lasting 100 days, have returned numbers within striking distance of the neutrino brightness that has always been expected...So has the solar neutrino problem gone away?

The author of the article concluded that the answer was probably

'no' and quoted Davis as saying that the last few higher counts

were to be intepreted as statistical fluctuations. However, it

seems that the first hint that the problem might be lessening

was appearing.   Others seemed to be thinking along similar lines.

For instance, Steven Weinberg, in a letter sent to Bahcall in

February 1976, noted the upward trend in the data and asked:[58]

> Anyway isn't Davis now seeing almost enough SNU's?

And the British astrophysicist, Douglas Gough, wrote in 1976:[59]

> Interestingly, Davis's most recent measurements are more than a factor of three above his previous mean and only marginally below the theoretical values. This has led some people to believe that the theory is more or less correct and the problem is disappearing, others still believe the solar models are seriously in error.

Over the period 1976 - 1978 the discrepancy appeared to

narrow further. Bahcall (1977) reported some revised calculations

of the Cl^{37}-capture cross-sections using more up-to-date data

and techniques.   He found that the cross-sections were about 15%

lower than he had previously estimated and this reduced the

prediction to 4.7 SNU. Also (as was mentioned in Chapter 7),

Davis with the use of more sophisticated statistical techniques,

reported a positive signal of 1.6 ± 0.4 SNU (Davis, 1978) later

to be revised to 2.2 ± 0.4 SNU.

With the theoretical prediction and experimental result

drawing closer together, arguments once more arose as to whether

or not there was a discrepancy and how serious it was. Bahcall,

for instance, firmly adhered to the view that there was a serious

discrepancy. When discussing the theoretical interpretation of

Davis's result at the Brookhaven conference, he remarked. [60]

> ...what we conclude is that we are in trouble; the standard
> theory is wrong; it's wrong because it predicts too large
> a boron-8 flux.

However, other scientists thought otherwise. For instance, one

respondent, whom I interviewed in 1978, told me:

> The accumulating data...is no longer entirely out of
> line with the theoretical predictions.

Thus, a decade after the first comparisons between theory

and experiment were made, we seem to have come full circle with

arguments once more appearing as to the significance of the dis-

crepancy. These new arguments would seem to be linked with the

attempts to get new solar-neutrino experiments underway (and to

get funding for them). As was mentioned above, the argument made

by Bahcall in favour of such experiments depended on the crisis

over the interpretation of the Davis experiment. If the crisis

disappeared, then much of the rationale for carrying out new

experiments might also disappear. Clearly, with the huge

investment which these new experiments require, a lot hangs on

the present theoretical interpretation of the Davis experiment.

The current (as of 1978) arguments over the existence or

not of a discrepancy and, in particular, certain statistical

arguments used to assess the degree of discrepancy, are discussed

in their own right in a paper presented in the Appendix (Pinch,

1980a). The material presented there adds weight to the

argument of this chapter that 'contradiction' and 'consistency'

are to be seen as being socially constructed.

Interestingly enough, since 1978, the theoretical prediction

has again been revised. In their latest publication (Bahcall, Lubow et al.

1980) Bahcall and his associates have indicated that further modi-

fications to the opacity calculations and to some of the nuclear-

physics data have raised the predicted detection rate to 7.5 ± 1.5 SNU.

This latest widening of the discrepancy, can be seen to have made

the arguments for new experiments (and their funding) even more

compelling. These latest developments are discussed briefly in

the paper in the Appendix.But it is clear that negotiations are

still in progress!

NOTES FOR CHAPTER EIGHT

1. For instance, in a letter to Fowler (August 11, 1967), in
    which his initial measurement is reported, Davis wrote:
    'This limit is quite low, but according to the latest
    opus from Bahcall and Shaviv the B^8 flux is 1.4 (1 ± 0.6)
    x 10^7 $cm^{-2}sec^{-1}$.' Also, in the first public discussion
    of the result Goldhaber (see below) cited Bahcall and
    Shaviv (1967) as giving predictions of $\phi_B 8$ = 1.4 (1 ± 0.6)
    x 10^7 $cm^{-2}sec^{-1}$ and $\Sigma\phi\sigma$ = 1.9 (1 ± 0.6) x 10^{-35} sec^{-1}.

    The original Bahcall and Shaviv paper was not received by
    The Astrophysical Journal until August 10, 1967. Since
    Bahcall had been in touch with Davis by telephone throughout
    August and probably had the first indications of a low
    result at the beginning of August, it could even be argued
    that the Bahcall and Shaviv 1967 prediction was made in
    the knowledge of Davis's low result. However, although
    logistically possible, this seems to be unlikely as the
    Bahcall and Shaviv paper was probably in its final form
    some time before it was submitted for publication (allowing
    for typing delays etc.).

    The paper was not finally published until July 1968 having
    had a minor revision in January 1968 which led to the
    fractionally smaller prediction of $\phi_B 8$ = 1.3 (1 ± 0.6)
    x $10^7 cm^{-2}sec^{-1}$. (It is not clear what the cause of this
    revision was).

    Bahcall was anxious for an earlier publication date. For
    instance he wrote to Chandrasekhar (the Editor of The
    Astrophysical Journal) on November 8, 1967 and urged him
    to publish the paper earlier. He wrote:

    > As you may have heard on the grapevine, the experiment
    > which motivated these calculations is now essentially
    > completed and comparisons with our theoretical cal-
    > culations have already been used in summarizing the
    > implications of the experimental results [mentions..
    > Goldhaber's talk in Japan]...Under the circumstances
    > I would very much appreciate it if you could check
    > to see if an earlier publication date for our paper
    > is possible.

    It seems an earlier publication date was not possible and
    thus Bahcall was faced with the prospect that his prediction
    made in August 1967 would not appear in the scientific
    literature until a year after the experimental result was
    known. By the time the Bahcall and Shaviv paper appeared,
    the theoretical prediction had been drastically revised
    (see below).

2. M. Goldhaber, 'Introductory Talk, II: Nuclear Physics - Where
    do we Stand?', p. 21. Undated paper, given sometime in
    September 1967 at an international nuclear physics conference
    in Japan. Copies of this paper are available from M. Goldhaber,
    Brookhaven National Laboratory.

3. For instance, it is stressed in Davis, Harmer and Hoffman (1968), Bahcall, Bahcall and Shaviv (1968) and Fowler (1969). Also, in a letter drawing the attention of the AEC to the Davis result, it was claimed:

> The full significance of their measurements is being evaluated but they can state definitely that less than 9% of the sun's energy is produced by the carbon nitrogen cycle.

Letter, C. Goodman to J. Conway, and E. Bauser, AEC, May 31, 1968.

It is interesting to note that the failure of the experiment to detect the CNO neutrinos is often put forward as a straight forward success of the experiment. However, it does seem to beg the question to claim any successes for an experiment which has failed to find the expected pp-chain neutrinos. After all if the explanation of this failure lies in some exotic property of neutrinos or something being fundamentally wrong in the fusion reactions, then the failure to detect neutrinos may not have any great significance for whether the CNO-cycle is in operation or not. However, as we shall see, Bahcall and Fowler at this time were still not convinced that the experiment was in conflict with any aspect of their prediction.

4. Letter, R. Davis to W. Fowler, August 11, 1967.

5. Letter, R. Davis to H. Barschall, The American Physical Society, November 6, 1967.

6. Letter, R. Davis to A. Wolfendale, December 1, 1967.

7. Davis had claimed in 1964 (see Chapter 3) that he would be able to look a factor of ten below what the theorists predicted at the time (40 SNU). In fact, this turned out to be rather an optimistic claim because in 1967 he was only able to set an upper limit of 3 SNU. He did not achieve increased sensitivity until the introduction of the pulse-rise facility in 1970.

8. Bahcall had written to Chandrasekhar in November 'the experiment...is now essentially completed', op. cit., note 1.

9. As Davis reported to Fowler in his letter of August 1, 'I have, of course had many telephone conversations with John', op. cit., note 1.

10. Handwritten letter, J. Bahcall to R. Davis. Unfortunately this letter is undated but from other information in the letter it is possible to impute that it was written sometime in December or January.

11. This paper was definitely not written earlier because Davis, who received all of Bahcall's papers as soon as they were written, did not receive a copy until January 1968. For instance, Davis's letter to Bahcall of January 29, 1968, commences, 'I have looked over your latest reprint...'

12. Of course, as Davis had not yet officially published his result the Caltech group may not have wanted to pre-empt him. But Davis's preliminary result was already in the literature (C & EN, September 25, 1967, 13 - 14); and often theorists quote private communications of experimental results. It seems unlikely that any deliberate deception was implied here as many people must have known already that Davis's results were available and such a deceit would have been obvious to them. It was more a question of 'understatement' than deception.

13. Letter, W. Fowler to R. Dodson, July 31, 1964.

14. Letter, R. Davis to P. Parker, December 7, 1967.

15. Letter, R. Davis to J. Bahcall, January 29, 1968.

16. Letter, R. Davis to J. Bahcall, December 8, 1967.

17. This is used in the V.A. theory of weak interactions.

18. Although Bahcall's preference for the new measurement has not to my knowledge been questioned, the neutron half-life seems to be far from being settled.   For instance, V.L. Telegdi has recently referred to the 'controversy concerning the different values of the neutron life time.' (see V.L. Telegdi, 'Summary', in Till Van Egidy (ed.), Fundamental Physics with Reactor Neutrons and Neutrinos, Bristol: Institute of Physics, 1978, 169-174). This controversy seems to have stemmed from new measurements made in 1978. For a discussion of these measurements and the most recent results see, J. Byrne, J. Morse, I.F. Smith, F. Shaikh, K. Green, and G.L. Greene, 'A New Measurement of the Neutron Lifetime'. Physics Letters, 92B, 1980, 274-278. This latest measurement, which leads to a slightly increased neutrino flux, is the value used in Bahcall's latest prediction (Bahcall et al., 1980).

19. This seminar was held sometime in November and is referred to in: 'Neutrino Flux from Sun is Lower than Expected', Physics Today, March 1968, 73.

20. Ibid.

21. E. Edelson, 'The Puzzle of the Missing Neutrinos', New Scientist, February 29, 1968, 472.

22. Op. cit., note 10.

23. Letter, R. Davis to H. Bethe, April 5, 1968.

24. Letter, J. Bahcall to H. Bethe, April 4, 1968. Bethe wrote back to Bahcall on April 9, 1968, and commented: 'It is a great comfort that there is no irreconcilable contradiction between theory and experiment as yet'.

25. Reeves seems to have played no further active part since
    there are no comments of his to be found in the correspondence
    files to which I was given access and he did not publish
    again on solar neutrinos.

26. Fowler (1969: 359). Fowler's attitude seems, however, to have
    been slightly contradictory because in a section of his
    review entitled 'Afterthoughts' he wrote that 'The observational
    results thus seem definitely lower than the lowest predicted
    ones' (p. 369). However, it seems that Fowler, like Bahcall,
    was not greatly concerned about any discrepancy because he
    shared Bahcall's misgivings about the uncertainty in S_{17}.

27. Ibid, p. 369.

28. Letter, R. Sears to R. Davis, May 9, 1968. We should perhaps
    be wary in interpreting Sears explicit agreement with
    Bahcall here since he sent a copy of his letter to Bahcall
    as well.

29. The suggested mixing mechanism was 'solar spin-down'. This
    is the slowing down of the rotation of the Sun caused by the
    loss of angular momentum from the exterior layers via the
    solar wind. The turbulence set up between the differential
    rotation of the solar interior and exterior would initiate-
    the mixing.

30. Letter, A. Cameron to J. Bahcall, July 31, 1968.

31. Iben used Z rather than Z/X.

32. Letter, I. Iben to G. Trigg, August 19, 1968.

33. Anonomous referee's report, undated.

34. Letter, I. Iben to R. Davis, September 20, 1968.

35. The conflict between Iben and Bahcall seems to have been
    a puzzle to other people at the time. For instance, in
    1970, Bahcall and Sears were commissioned by the Annual
    Review of Astronomy and Astrophysics to write a review
    of the solar-neutrino field. In a letter to Sears the
    Editor noted:
          I have recently been reading the papers by John
          Bahcall and Icko Iben on the solar neutrino problem,
          and I am puzzled by the discrepancies between them.
          I hope these will be resolved in your review.
    Letter, D. Layzer to R.L. Sears, November 18, 1970.

    However, in the review (Bahcall and Sears, 1972: 32 and 34-35)
    the different approaches of Sears and Bahcall, and Iben
    are described with no attempt to evaluate the differences.
    When I talked to Bahcall in 1978 he very much played down
    the controversy and talked about it as a matter of different
    philosophies.

36. For similar ideas on contradictions being socially constructed see Travis (1980b) and Collins and Pinch (1982).

37. This is the cross-section for the reaction $Li^7 + d \rightarrow Li^8 + \nu$.

38. The importance of such resonances was emphasised by the data of Vaughn et al. (1967).

39. Ulrich had worked on solar models at Berkeley and had carried out some computations of neutrino fluxes (Torres-Peimbert, Simpson and Ulrich, 1969). Ulrich had been in contact with Bahcall since 1966 (Letter, R. Ulrich to J. Bahcall, October 3, 1966) and had offered to run computations for him. After Shaviv left Caltech in 1967, Bahcall managed to get a temporary post for Ulrich at Caltech in order that they might collaborate. This was the first paper stemming from their collaboration.

40. The $Be^7 + p$ and $Be^7 + e$ reactions compete. An increase in one reaction rate means a decrease in the significance of the other branch (see Fig. 2.2).

41. Bahcall's view that there was no fundamental contradiction because of the uncertainty in S_{17} was reiterated in a semi-popular article in Scientific American (Bahcall, 1969a). It should be noted that around about this time the uncertainty mentioned by Bahcall et al. (1968) over the value of S_{34} was resolved by new low-energy measurements made at Kellogg (Nagatani et al., 1969). A value slightly larger than the 'standard' value was found. The effect of this on the predicted neutrino flux (a slight increase $\sim 10\%$) was counterbalanced by a modification due to theoretical work on non-equilibrium nuclear reactions (a slight decrease $\sim 10\%$). These effects are described in Bahcall (1969b).

42. Whilst at MIT, Watson shared an office with Rood (whom we encountered in Chapter 7).

43. This controversy concerned the interpretation of the iron spectrum in the Sun. Measurements made on the solar corona gave abundances a factor of ten larger than measurements made on the photosphere. Previously the corona measurements were regarded as unreliable, as they were considered to be difficult to interpret. However, new laboratory measurements of the transition probabilities of iron atoms at high temperatures indicated that the standard value was a factor of ten too small. (The transition probabilities are used for relating the intensity of the solar iron spectra to the abundance). Thus the photosphere measurements turned out to be the unreliable ones!

44. This error is acknowledged in Bahcall and Ulrich (1971).

45. As Bahcall wrote to Davis on May 27, 1970:
    The citation hasn't come out yet but our collaboration
    on solar neutrinos probably played a major role in the award.

46. G.L. Wick, 'Neutrino Astronomy: Probing the Sun's Interior', Science, 173, 1971, 1011-2.

47. Monitor, 'The First Observation of Neutrinos from the Sun', New Scientist, June 10, 1971, 610.

48. Bahcall in Reines and Trimble (1972: A1-A2).

49. For instance in a letter to A. Cameron, S. Colgate and G. Field (March 27, 1972), Bahcall wrote:
    The neutrino meeting organizers want to have a seminar by Davis and myself on Solar Neutrinos and I think we are indeed obligated to participate because of the crisis in this subject.

50. Letter, J. Bahcall to G. Ragosa, May 29, 1973. (this letter is a review of an experimental proposal to detect solar neutrinos). Bahcall wrote:
    The current revolution in stellar (sic) is a direct result of the only analogous experiment, that of Davis et al. from Brookhaven.

51. Letter, J. Bahcall to R. Davis, June 18, 1973.

52. A. Hammond, 'Solar Neutrinos: Where Are They?' Science, February 4, 1972, 505.

53. A.G.W. Cameron 'Summary' in Reines and Trimble (1972:D3).

54. Letter, J. Bahcall to N. Cabibbo, March 15, 1972.

55. This paper was originally written for New Scientist but was rejected because it was thought to be too technical.

56. The distinction is, of course, not absolute, since a failure to detect low-energy neutrinos could still result from an astrophysical problem which affected all neutrino fluxes.

57. Monitor, 'Will the Solar Neutrino Problem Go Away?' New Scientist, March 15, 1976.

58. Letter, S. Weinberg to J. Bahcall, February 7, 1976.

59. D. Gough, 'The Shivering Sun Opens Its Heart', New Scientist 70, 1976, p. 591.

60. J. Bahcall, 'Theoretical Introduction to the ^{37}Cl Solar Neutrino Experiment', in Friedlander (1978a: 63).

CHAPTER NINE

OTHER THEORETICAL APPROACHES TO THE SOLAR-NEUTRINO PROBLEM

Thus far in this thesis, the theoretical predictions discussed

have all come from the so-called 'standard' solar model. However,

as mentioned in Chapter 8, a number of modifications to the physics

and astrophysics which go into the standard model have been proposed.

Such suggestions have also often included modifications to the

standard physics of how neutrinos propagate through space and how

they interact with the experimental target.   In this chapter these

radical theoretical ideas are considered. Firstly, the main ideas

are briefly reviewed. Then, one particular radical theoretical

approach is considered in some detail. It is argued that this

approach (and, by extension, all the other non-standard theoretical

approaches) is as scientifically plausible as the standard approach.

Thereby, it is claimed that the   standard

approach can be deconstructed. The rejection of deviant theories,

it is argued, is to be understood as a social process.

The Main Classes of Solution

The different explanations of Davis's result which have been

put forward tend to fall within the domains of either nuclear

physics, astrophysics or neutrino physics.   The explanations come

across during the course of the present research are listed under

these headings in Table 9.1.   (The literature cited in Table 9.1

refers, not only to the source of the original explanation,

but also to further elaborations, discussions and attempted

refutations).[1]   Occasionally other areas have been drawn upon.

For instance, Ruderfer (1975), has suggested that solar neutrinos

are detected by living matter and hence are of relevance to

parapsychology!

TABLE 9.1. EXPLANATIONS OF THE SOLAR-NEUTRINO DISCREPANCY

(References are given after the notes to Chapter 9).

NUCLEAR PHYSICS

Low energy resonance in He^3+He^3	Fowler (1972); Fetisov and Kopysov (1972); Barker (1972); Halbert et al. (1973); Fagg et al. (1973); Parker et al. (1973); Dwarakanath (1974); Fetisov and Kopysov (1975).
S_{11} in error	Slobodrian et al. (1975); Newman and Fowler (1976a); Davies et al. (1977); Andreev et al. (1977).
Catalysis of fusion by quarks	Libby and Thomas (1969); Salpeter (1970).
Photodisintegration of B^8	Mitalas (1973).
Maxwell-Boltmann distribution of particle velocities in error	Clayton (1974); Clayton, Newman and Talbot (1975a); Krook and Wu (1976).
Anomalous production of N^{13}	Starbunov (1974) quoted in Kuchowicz (1976).
S_{17} in error	Barker et al. (1980).
S_{34} in error	Rolfs (1980).

ASTROPHYSICS

Opacities in error	Bahcall, Bahcall and Ulrich (1969); Iben (1969a); Abraham and Iben (1971); Stothers and Ezer (1973); Carson et al. (1974).
Low-Z interior	Torres-Peimbert, et al. (1969); Iben (1969a); Bahcall and Ulrich (1971).
Heavy element abundance at surface differs from interior	Kuchowicz (1973); Joss (1974); Auman and McCrea (1976); Newman and Talbot (1976); Talbot and Newman (1977); Christensen-Dalsgaard et al. (1979).
Chemical Inhomogeneity	Kuchowicz (1973); Prentice (1973, 1976); Faulkner et al. (1975); Wheeler and Cameron (1975).

ASTROPHYSICS contd.

Equation of State in error	Bahcall, Bahcall and Ulrich (1969); Rouse (1969).
Sun switches off	Sheldon (1969); Aurela (1970).
Temperature Variation with time	Shchepkin (1973).
Mixing over whole of solar life-time	Ezer and Cameron (1968); Shaviv and Beaudet (1968); Bahcall, Bahcall and Ulrich (1968); Iben (1969b); Shaviv and Salpeter (1968,1971).
Sudden Mixing	Fowler (1972); Dilke and Gough (1972); Rood (1972); Ezer and Cameron (1972); Defouw (1973); Bochsler and Geiss (1973); Ulrich and Rood (1973); Schwarzschild and Härm (1973); Rosenbluth and Bahcall (1973); Ulrich (1975); Gabriel et al. (1976).
Turbulent diffusion of He^3.	Schatzman (1969); Bahcall and Ulrich (1971); Shaviv and Salpeter (1971).
Magnetic Field in Sun	Iben (1968, 1969a); Bahcall and Ulrich (1971); Abraham and Iben (1971); Chitre et al. (1973); Bartenweifer (1973); Parker (1974); Snell et al. (1976).
Rotation of Sun	Demarque et al. (1973a,b , 1974); Bartenweifer (1973); Roxburgh (1974, 1975); Monaghan (1974); Rood and Ulrich (1974); Snell et al. (1976).
Non-Orthodox Energy Transport Mechanisms	Littleton (1972); Hill et al. (1975); Newman and Fowler (1976b); Beaudet et al. (1977).
Black Hole in Sun	Stothers and Ezer (1973); Clayton, Newman and Talbot (1975b).
Unrealistic Solar Model	Rouse (1969, 1975).
Brans-Dicke Varying-G Cosmology	Shaviv and Bahcall (1969).
Dirac Varying-G Cosmology	Maeder (1977); Chin and Stothers (1975, 1976).

ASTROPHYSICS contd.

Dilaton Theory of Non-Newtonian Gravity	Fujimoto and Sugunato (1972).
Anisotropic Cosmology leading to high production of He^3	Kocharov and Starbunov (1970); Abraham and Iben (1970); Mitalas (1972).
Sun formed from Super Nova	Manuel and Sabu (1977).
Sun has half its mass added later	Hoyle (1975).

NEUTRINO PHYSICS

Neutrino Oscillation	Gribov and Pontecorvo (1969); Bahcall and Frautschi (1969); De Graaf (1971); Parkinson and Vasholz (1973); Mann and Primakoff (1977); Bilensky and Pontecorvo (1976).
Neutrino Decay	Bahcall et al. (1972); Hamza and Beck (1972); Moles and Vigier (1974); Davis and Ray (1975).
Magnetic Moment of Neutrino	Cisneros (1971); Landovitz and Schreiber (1973); Radomski (1975); Kim (1976).
Different sorts of neutrino exist	Pakvasa and Tennakone (1972, 1973); Perkins (1972).
Neutrinos lose energy in Sun	Mikaelyan (1972); Clark and Pedigo (1973); Ruderfer (1974); Barnothy (1974); Leiter and Glass (1977).
Neutrinos lose energy in space	Tennakone (1973); Aurela (1970).
Variation of Weak Interaction Constant	Finzi (1974); Shaviv (1974).
New Particle – Reggieum – A massive uncharged boson	Bahcall and Pegge (1972) as discussed in Trimble and Reines (1973).
Reverse Causality	Csonka (1970).
New Version of Weak Interaction Theory	Ray Chaudhuri (1971a, 1971b); Bandyopadhyay (1972); Stothers and Ezer (1973).

Given the complexity of the problem and the large number of input parameters involved, it is no surprise that so many solutions have been offered. There are too many solutions to discuss in detail here; however, some of the broad characteristics of the solutions can be identified.

The largest class of solution falls within the domain of astrophysics and involves attempts to modify the solar model. Because the boron-eight neutrino flux is highly temperature dependent $(\phi_B 8 \propto T^{14})$, any slight perturbation to the standard solar model that will produce a small drop in the central temperature of the Sun, will also solve the solar-neutrino problem. The other major class of solutions falls within neutrino physics and involves the suggestion of some property of neutrinos which prevents them reaching Davis's experiment in the expected number. The reason there are so many solutions of this type seems to stem from the etherial nature of the neutrino (it is thought to be massless and chargeless) and the consequent paucity of experimental data which might restrict such speculations.

Many of the solutions are simply suggestions that the current value of some input parameter is incorrect. For example, in the nuclear-physics domain, the explanations which postulate errors in S_{17} and S_{34} are of this type. If either of these cross-sections has been overestimated, then the predicted neutrino flux will be too large. Similarly, in the domain of astrophysics, if the opacity is actually lower than presently assumed, then this will lower the temperature gradient, the central temperature, and hence the B^8 flux. Often explicit physical mechanisms have been postulated to account for errors in input parameters. For instance, in the nuclear

physics, the existence of a low-energy resonance in the He^3+He^3

reaction would mean that the value of S_{33} had been underestimated,

and hence that the branch of the pp-chain through boron eight

was less important. Similarly, the error in the opacity could

have been caused by the accretion of matter on to the solar surface

which would mean that the surface abundance (from which the opacity

is estimated) was different from the interior abundance.

Another general type of explanation involves the postulation

of physical processes which are not normally expected to occur

under these conditions, or which are expected to play only a minor

role. For instance, in the domain of nuclear physics, it has been

suggested that boron eight is photodisintegrated in the Sun by the

large fluxes of photons and gamma rays present. Suggestions have also

been made that the Sun has a rapidly rotating core; large magnetic

fields; an inhomogeneous composition; a mixed-up core; energy

transport by acoustical waves; and a central black hole - all

of which, alone or in combination, could explain the lack of

neutrinos. Similarly, in the area of neutrino physics, if neutrinos

oscillate, decay, or have magnetic moments, then Davis could

expect a reduced signal. Novel physical mechanisms have also

been postulated, such as neutrino interactions with photons or

other particles in the Sun. Even the universality of standard

physical constants and approximations has been questioned. For

instance, it has been suggested that the Maxwell-Boltzmann

distribution of particle velocities, upon which the calculation

of the nuclear reaction rates is dependent, does not apply in the

interior of the Sun. Variations in the universal gravitation

constant, G, and the universal weak-interaction constant, g, have

also been suggested. Finally, some authors have postulated novel

cosmological assumptions which have the effect of solving the solar-neutrino problem. For instance, Fred Hoyle has suggested from a cosmological basis, that half the solar mass with a different chemical composition has been added to the Sun at a later stage in its evolution.

This brief account of the different types of explanation does not do justice to the full complexities of the physics. Much more informative discussions of the physics involved in such explanations can be found in Kuchowicz (1976) and Rood (1978).

Despite the large number of suggestions which have emerged none has gained widespread acceptance as the solution to the solar-neutrino problem (assuming that there is such a problem). For instance, Bahcall, has recently written:

> ...there is no general agreement as to what aspect of the theory is most likely to be incorrect (Bahcall, 1979:227).

And Rood, in summarising his review of non-standard solar models at the Brookhaven conference, said:[2]

> So to summarize the status: Many things have been tried and for various reasons none of them are very satisfying.

In such circumstances it seems that most scientists are content to continue to use the standard model and standard assumptions despite Davis's solar-neutrino results.

Although none of the proposed solutions has been widely accepted there is agreement that some have been rejected. For instance, the $He^3 + He^3$ resonance solution has been tested in experiments and the consensus is now that it is no longer a viable solution (but see Fetisov and Kopysov (1975) who still maintain that the experimental evidence is less than definitive). The status of many of the astrophysical and neutrino-physics suggestions is, however, far less clear cut since such ideas can

rarely be tested by experiment. More abstruse arguments, such

as the degree of <u>ad hocness</u> of the idea, tend to be used to rule

out such explanations.[3]

The Social Deconstruction of the Standard Theory

We must be careful in concluding that any suggestion has been

rejected because it is found to be scientifically wanting. If

we are to view scientific knowledge as being socially.constructed,

the falseness of certain solutions to the solar-neutrino problem

is to be found in the social rather than the natural world. In

other words, we cannot say, as many solar-neutrino scientists do,

that certain of the solutions to the problem are manifestly

scientifically false. To claim that an idea is false is to make

a knowledge claim every bit as strong as to say that a certain

idea is true. In the same way that in Chapter 7 we deconstructed

the solidity of Davis's experimental claims we must also try to

deconstruct the claim that certain theoretical ideas are patently

false. That is to say, the sociologist must try and show the

validity of such rejected ideas and point to the social processes

whereby they have become discredited.

Ideally this should be done for every idea put forward to

explain the solar-neutrino problem that has been rejected. To

do this for every case, however, is clearly impossible in.the

context of this thesis. Instead, what will be attempted is a

much more limited task. It will be considered how just one of

these explanations has been rejected. Such an analysis will

serve to deconstruct the claims of the standard theory for, if

it can be shown that one alternative theory is equally scienti-

fically plausible., then the scientific standing , of the 'standard'

theory can be seen to rest in the social rather than the natural world.

In the selection of a suitable illustrative case for such an analysis, a problem immediately arises. In previously researched cases of 'rejected knowledge' (e.g.,Pinch,1977; Collins, 1975; Collins and Pinch, 1979), the proponents of deviant ideas usually have been found to press home these ideas.[4] That is, when challenged, they produce arguments and evidence as to why their approach is correct and the orthodox approach incorrect. This makes it relatively easy to see the scientific rationale for such ideas. Also, if they push hard enough the proponents frequently engender a controversy which makes the social processes involved more transparent. However, in the case of radical solutions to the solar-neutrino problem very few proponents are willing to press their ideas very far at all.

Many of the solutions to the solar-neutrino problem have been put forward in almost apologetic tones. It is as if the proponents do not themselves really believe such ideas. The ideas are often discussed in the context of 'seeing what is possible', and very few proponents are willing to defend their ideas to the hilt. As Rood commented, in his review of non-standard models at the Brookhaven conference:[5]

> I don't think I believe any of them, and many of the other
> people who perpetrated these models don't have any great
> fondness for them either.

This became apparent when I talked to some of the originators of such ideas. For instance, one scientist told me:

> You're presented with this result that you can't explain
> easily. So the first reaction is, and it's a perfectly
> normal one, is exploring all possible suggestions as to how
> you can get out of it, kinda disregarding how ludicrous

they are.   I mean, in another context most of us wouldn't
have touched these things.   So I say they are kinda trash
in the sense of normal astronomy, where you try to be
conservative.   And the idea was to try some outlandish
things and to see just how far you can push the various
aspects of the problem...

Pinch: Are you saying the models you propose as solutions
to this, you don't really believe them?

Yes that is true.   I do not really believe in them. It is
a matter of if I push this hypothesis to the limit, how far
can I go?

Another respondent said:

None of us would have published these things or would have
thought about these suggestions if there hadn't been a
crisis in the theory of stellar evolution...If there is
a serious problem it means that serious people, normally
reasonably conservative in their scientific work, allow
themselves to publish things that otherwise they would have
probably not discussed at a cocktail party...I would describe
the suggestion that Q and I published as 'crackpot'.

Although the ideas of what might be described as 'unen-

thusiastic proponents', could, in principle, be subject to

sociological analysis,[6] it is easier to deal with radical ideas

towards which there is a degree of commitment. This is because,

in effect, the committed proponent does the sociologist's work

by showing how it is that the orthodox approach falls down and

why the radical ideas are so attractive.

The attitude of the above scientists who, although being

prepared to discuss radical ideas, were not prepared to believe

them, is understandable when we consider that most of the

scientists putting forward such ideas regularly use the standard

theory and indeed have built their scientific careers 'puzzle

solving' within the confines of the standard theory. Clearly

they will be ambivalent to ideas which serve to undermine their

'bread and butter' scientific work.   Most are simply content to

obtain the publication to be gained from discussing such ideas

without really pushing for them. One of the main phenomena

documented in Table 9.1 is the vast outcropping of theoretical

publications which has been produced in response to Davis's

result. As one theoretician (Sears) perceptively remarked early

on in response to Davis's result:[7]

> The results...are certainly exciting in the sense that the
> negative result will produce some very positive activity in
> many people. As the old saw has it, 'No v's is good news',
> for theoreticians looking for challenges.

The phenomenon of scientists not been willing to push for their

ideas has been discussed by Bourdieu (1975), and Collins and Pinch

(1982). Since most scientists are already immersed in, and

dependent for their rewards upon, a particular knowledge culture,

they will be reluctant to mount challenges to the predominant

culture. In such a situation, scientists will often dabble in

radical ideas giving the appearance of a lack of 'authentic'

commitment to the radical ideas (see Collins and Pinch, 1982,

and Chapter 10). Thus it can be expected that if there are any

committed revolutionaries, who are prepared to believe in and

push their radical ideas, these are more likely to be found on

the margins of orthodoxy.[8]

This seems to be true for the solar-neutrino field. I only

encountered two respondents who were prepared to stand by their

suggested solutions to the solar-neutrino problem and both were

clearly marginal figures. One scientist was a retired cosmic-ray

experimentalist who operated from his own scientific institution

(of biomagnetism) located at a private address.[9] The other

scientist worked for a commercial company (on research not directly

connected with the solar-neutrino field).It is the work of this

second scientist - Carl Rouse - which will be considered here.

Rouse's impact (or rather the lack of it) is particularly interesting

since he was a contemporary of the leading theoreticians and had

discussions with the Caltech theorists as far back as 1964. There

are also other interesting aspects to his case. He is the only black

scientist to be interviewed. His case has also attracted the

attention of at least one other sociologist[10] and has even been of

interest to US Congressmen.[11]

Rouse's Claims in 1964

Rouse's interest in stellar-evolution theory began in the

early 1960's. He had obtained his doctorate at Caltech in 1956

in cosmic rays[12] and, after a short spell as an engineer,[13] he took

a job as a physicist at the Lawrence Livermore Laboratory where

he worked on hydrodynamical computer programmes for use in weapons

calculations. As part of this work, Rouse developed a very accurate

equation of state describing the behaviour of real gases in highly

ionised plasmas. An astronomer colleague at Livermore persuaded

him to use his equation of state to study pulsating stars. Rouse

constructed a stellar model and found that he could not get self-

consistent answers for this problem. He thus thought he would

check his programme by computing models for the best understood

star - the Sun. He expected this to be a simple matter. As he

informed me:

> I thought I was wasting my time. I said everyone understands
> the Sun...In fact my programme did not even take time to
> print things out in a logical order, all we did was dump
> memory and looked in memory. That's how little time we
> thought was going to be involved.

He found, however, that he could not produce a model that agreed

with the temperature-density profiles of other standard models,

such as produced by Sears and Iben at Caltech. Rouse commented:

> I'll never forget. It was the September of 1963. I came
> back to my office at the Lawrence Livermore Laboratory after
> one of my trials and I said to myself 'I don't think we
> understand the Sun'...And I've been working on it ever
> since.

Rouse reached this conclusion at approximately the same time

that Sears was producing the range of solar-neutrino flux predictions

from the standard solar model which Bahcall used in arguing for

the feasibility of Davis's experiment (see Chapter 4). Indeed,

Rouse was in correspondence with Sears at this time.[14] Rouse

informed Sears that he was unable to construct a satisfactory

model even when he attempted to use parameters given him by Sears.

He concluded that in order to produce a self-consistent model

of the Sun he needed a lower central density, a higher central

temperature and a greater abundance of helium in the core, than

given by the standard model.[15] This meant that, according to

Rouse, the Sun was hot enough to produce its energy on the CNO-

cycle. His model was described by Sears, in his letter to Rouse,[16]

as 'of course totally incompatible with solar models over the past

several years'.

Whilst Rouse was concluding that the Sun was poorly under-

stood, Sears commenced his (1964) article in The Astrophysical

Journal by writing:

> Theoretical models of the internal structure of the Sun are
> no longer at the frontier of the theory of stellar structure
> and evolution. Since the recognition of the proton-proton
> chain as the major energy source, the general features of
> solar structure have become quite well established...
> (Sears, 1964:477).

At the time this was being written Rouse was attempting to

get his own results published in the same journal. As he had

had a series of papers on the equation of state published in

The Astrophysical Journal, he expected no problem in publishing

an article on his solar model there too. Rouse's paper was, however,

rejected. He was given a very brief referee's comment which

read as follows:[17]

> Great advances have been made in the last few years in
> computational technique, as well as results, for stellar
> structure calculations in general and for solar models in
> particular...Unfortunately the philosophy and execution
> of the stellar model calculations in the present paper
> are highly unorthodox, somewhat arbitrary (e.g., the type of
> abundance variation assumed), and on several points less
> accurate than the standard modern methods (e.g., the use of
> mixing length theory for the outer convection zone). I
> would suggest Dr. Rouse get together with some of the
> exponents of the more orthodox modern methods (e.g.,
> Prof. Henyé at U.C. Berkeley or Drs. Sears and Iben at
> Kellogg Rad. Lab., Cal. Inst. Tech.) to combine his
> accurate equation of state with the best techniques for
> model calculations.
> I strongly recommend against publication of this paper.

Rouse took great exception to this rebuttal as he felt that

he was being turned down because he did not follow the standard

approach. He,of course, knew what this approach was since he

had been in correspondence with Sears.[18] As we shall see below, as

far as Rouse was concerned there was no point in using the

standard approach since he considered the standard approach

to be both arbitrary and incorrect.

He resubmitted his paper to Physical Review, but it was

again turned down. This time the Editor thought its subject

matter was more suitable for The Astrophysical Journal (from

where, of course, it had already been rejected).[19]

Although Rouse has managed to find other outlets for his

work since (such as European journals),[20] his failure in 1964

and subsequently to get into the mainstream literature has lessened

his impact. It would have been very dramatic (particularly with

hindsight) if Sears and Rouse with their very different conclusions,

had both had their articles published in the same issue of The

Astrophysical Journal.

Rouse has not only found it difficult to get his claims published, but also he has found it virtually impossible to get NSF funding[21] (on which he is peculiarly dependent as he is not at a University). Ironically, one of the criticisms of his funding applications has been his failure to publish in the mainstream literature.[22]

In order to understand the difference between Rouse's approach to solar-model calculations and the standard approach, it will be necessary to go into the construction of solar models at greater depths than has thus far been needed (descriptions of building a solar model can be found also in Chapters 4 and 8).

The Standard Approach to Solar Models and Rouse's Approach

The solar model is based upon observational parameters of the Sun which are coupled with theories of the Sun's physical structure, energy mechanisms and evolution. Certain of the observational parameters are known with a high degree of accuracy. These include the solar mass (which can be determined from the motion of the planets), and the solar luminosity and radius (both obtained from direct observation of the solar photosphere). The age of the Sun is also well known (it is assumed to be the same age as the Earth). In addition, the heavy element to hydrogen ratio of the Sun, Z/X, can be determined from spectral observations of the photosphere. However, as we have seen in Chapter 8, there is some uncertainty as to the exact value of this ratio and it has, on occasions, been revised. The helium fraction of the Sun, Y, cannot be determined from direct spectral observations of the photosphere (despite helium having first been discovered in the Sun (Helios)). However, the abundance can

be estimated from a variety of other sources, such as galactic

objects and cosmology.   But this abundance is not known accurately

either. As we have seen (in Chapter 8), there is some disagreement

as to whether Y is better known than Z/X.   It is usually assumed

that whichever composition parameter is chosen for the solar model

that the present abundance is the same as the primordial abundance.

The fundamental physical processes of the Sun (hydrostatic

equilibrium between gravitational force and pressure, energy

production by hydrogen burning and energy transport by radiation

and convection) can be described by a set of partial-differential

equations.   It is the solutions of these equations with the

relevant input data which constitute the solar model.   A computer

programme is necessary to solve the equations.   Ideally, if an

initial composition and distribution is assumed, and a model[23]

representing a star with the Sun's mass is evolved for 4.7 billion

years (the solar age) then the model ought to be able to produce

the observed luminosity and the radius of the present Sun. However,

the outer convective layer of the solar envelope is considered to

be poorly understood and the standard practice is to assume that

such a model cannot be expected to produce the correct radius.

The correct radius is obtained only after one of either two

adjustment procedures are followed. Either it is assumed that the

Sun has an ideal-gas massless envelope surrounding it and the

depth of this envelope is arbitrarily adjusted until a fit with

the radius is achieved.   Alternatively, the convective layer is

described by a phenomenological theory, known as 'mixing-length

theory'.   This theory contains an adjustable parameter (the so-

called 'mixing-length ratio', α) which can be arbitrarily adjusted

to produce a match with the radius. Since the convective layer is

thought to play a small part in energy-generation processes, it is

considered to be acceptable practice in the standard approach to make

an adjustment to get the correct radius.

The crucial test for the standard solar model is considered to

be the match with the luminosity of the present Sun as the luminosity

is more of a direct measure of the energy-generation processes. As we

saw in Chapter 8, even the match with the luminosity cannot be achieved

in a straightforward manner. The procedure followed is to choose

ab initio a value of either Z/X (Sears and Bahcall) or Y (Iben),

and to choose an arbitrary value for the other composition parameter.

A model is then computed and the luminosity produced after 4.7 billion

years is inspected. Successive values of the arbitrary composition

parameter are chosen and a succession of models computed until a

fit with the luminosity is achieved. Provided the final value of

the parameter that is varied lies within the acceptable observational

range, it is assumed that the solar model has been found. It is

this model which gives the neutrino-flux predictions by a sub-routine

of the main programme.

It can be seen that the standard approach is not fully rigorous

in the sense that a unique model can be derived from a given set

of inputs. However, the composition parameters needed to make

the model consistent with the present luminosity seem to lie in the

range permitted by observation.

The approach Rouse adopted was to try and eliminate the free

parameters needed to produce a self-consistent model, and, in

particular, the parameter associated with the radius. His aim

was to try and produce both the observed luminosity and the

radius. By using his own equation of state, which he claimed

represented the Sun more accurately than the ideal-gas equation

of state used in the standard approach, he considered that he should be able to produce a model which, when evolved, gave the observed radius. In other words, he refused to adopt what he considered to be the 'fiddle' of arbitrarily adjusting parameters to achieve a match with the radius. However, when he based his model on composition parameters used by Sears and others he found that he could not match both luminosity and radius. He found the only way to achieve such a match was with a model which gave a central temperature, density, and helium abundance different from those commonly assumed.

To those following the conventional approach, Rouse's procedure was considered to be hopeless because it was felt that there was no adequate theory of convection. It was no surprise that he failed to match radius and luminosity because this was assumed to be impossible without assuming an adjustable massless envelope or mixing-length theory. As one referee commented, on a later research proposal of Rouse's:[24]

> My conclusion is that Rouse's statement (in effect) 'that attempts to reproduce models found in the literature failed' is no surprise because if he did what he said he did there was no possibility of reproducing them.

Rouse, on the other hand, considered that the correct approach was to try and make more sophisticated physical assumptions, such as a real equation of state, and try and match the radius as well as the luminosity. As far as he was concerned, until a model could be found which did not require free-parameter adjustments, the Sun had to be considered to be poorly understood. As Rouse told me:

> In the standard approach they wind up short of the radius and then they say this difference is made up of a perfect gas massless envelope. And what they do is essentially go

> back and fudge...Now the popular thing is so-called mixing-
> length theory, where the temperature gradient is a function
> of some constant they call the mixing length. And that's
> fudged, that's a free parameter...So, I, said, I just don't
> believe these calculations should be done with so many free
> parameters...As a physicist coming into it I was appalled
> at a lot of this.

It can be seen that both sides of this argument have a certain

plausibility. The standard procedures enabled model builders to

produce what seemed to be workable models and hence investigate

all the myriad problems of stellar-evolution theory. As far as

they were concerned, there was little point in worrying about

their failure to do something which they considered to be im-

possible anyway. On the other hand, we can easily imagine scenarios

in which Rouse's attempt to bring more rigour and more sophisticated

physical assumptions into solar-model calculations was seen as

laudable. It seems very difficult to find anything mistaken

about his approach, rather it is just that it is un-conventional.

The difficulty which the followers of the orthodox procedure

have had in finding faults or errors in Rouse's work can be seen

from the referees' comments which he has tended to receive. These,

such as the one from The Astrophysical Journal, all tend to point

to the 'unorthodox', 'arbitrary', 'naive', or 'outdated' nature

of his approach rather than to substantive errors.

Given Rouse's disenchantment with the standard solar model,

the failure of Davis to detect solar neutrinos was an event of

great significance to him. The Davis experiment had been billed

as the definitive test of the standard model and Rouse naturally

took its failure as a vindication of his own approach. Rouse

had learnt of the solar-neutrino experiment from Sears in 1963

and he had sent profiles of his own model to Reines (whom he knew

from his own undergraduate studies at Case) in the hope that he might be interested in searching for neutrino fluxes consistent with the model. When Davis's result became known, Rouse (1969a) published a short letter in Nature where he reiterated his arguments that the standard solar models did not correspond to the real Sun. According to Rouse, any agreement between Bahcall's prediction and Davis's result would have been by chance anyway.

After the publication of Davis's initial results, Rouse keenly followed the progress of his work. He took issue with the reports in Science and New Scientist (discussed in Chapter 8, Part II) which suggested that there was no major discrepancy between theory and experiment. When Bahcall and Fowler publicly acknowledged,[25] in 1972, that the Sun was not understood (see Chapter 8, Part II), Rouse felt this was no more than he had been saying for the previous ten years.

The impact of the solar-neutrino result on Rouse's work has, however, been somewhat diluted by his previous claims that the Sun operated on the CNO-cycle. If the CNO-cycle did predominate, then Davis was expected to detect an even larger flux than the conventional model predicted.[26] Thus, Rouse's own 1963 model was no better than the standard model at predicting the outcome of Davis's experiment. However, there is no doubt that Rouse's general message of 1963, that the Sun was not well understood, was borne out by Davis's result.

The failure of the standard model did not, however, help Rouse's approach to gain acceptance. It is one thing to point to something being wrong; it is quite another to convince others that your approach is any more likely to be successful. Rouse

continued to fare badly, especially with his funding proposals

to the NSF.[27]   His proposals were turned down for similar reasons

to those mentioned already. Although Rouse was invited to both

the Irvine and the Brookhaven conferences, his presentations

did not draw much critical comment. There has never been any

public confrontation in the literature over his views and it

seems that his work has met with 'implicit rejection'.[28]   As

Rouse told me himself:

> I said to myself [in 1963] 'I don't think we understand
> the Sun'. And I've been working on it ever since. The
> establishment knows this but no one wants to admit it in
> public.

I asked several of the solar-model specialists I talked with

in 1978 about his work.   It was clear that as far as they were

concerned his approach was too unorthodox to take seriously.

Typical comments were:

> I haven't looked at his work for 7 or 8 years, so I am not
> sure of that...His argument was just wrong from a stellar-
> structure point of view.

> Yes, Rouse is a kinda funny guy...He has a way of computing
> models that I don't think anyone else agrees with.   It's
> just very strange you can't take him seriously at all.

> He has been saying things like that for a long time and it's
> also true that people have not been taking very much notice
> of him. I think part of the reason for that is the content
> of some of his ideas have been a bit quacky.I mean he's
> a good scientist, but some of his ideas have pushed into the
> borders of quackery....It was more his way of describing it
> and the timing seemed to go against the flow of contemporary
> science. I haven't looked at his stuff recently, it's old
> 1960's stuff...It might be worth spending some time relooking
> at all of his papers and seeing how they look now. If I was
> more interested in the history of ideas I would do that.

> I do not know his work well.   I know he had, long before
> all this, differed from the majority of people in the field...
> I cannot judge the difference at all except that I have not
> seen anyone else agreeing with Rouse.   I have not followed
> his more recent work in any detail.   It's one of these
> very worrisome cases of possibly being unfair to somebody
> who, in a sense, is a lone wolf and deviates persistently
> from the establishment, although he has a good scientific
> background.

When more technical objections were put to me concerning Rouse's

work, they tended to reiterate the standard view that it is hopeless

to try and produce a model which will give the solar radius. For

instance, I was told:

> I've never been able to understand his approach because to
> get the radius you have to get the convective zone and the
> convective zone has a variable value and you have a mixing-
> length parameter. And, for some reason, he has not
> recognised the existence of the mixing-length parameter
> which gives the flexibility to get the radius no matter what.

Another respondent recollected that:

> Rouse would put all this crazy stuff out, thinking you can
> use the Sun's surface as a boundary condition. Which you
> can't because there are uncertainties in convection theory
> that don't allow you to do that.

However, a more sympathetic view came from one fluid hydrodynamicist

with whom I spoke. He seemed to share some of Rouse's misgivings

concerning the way standard solar models were constructed, and hence

appeared less worried than most about the negative solar-neutrino

result:

> You get less depressed about the failure of a theory that's
> not completely deductive...There are parameters and certain
> aspects of the theory when applied to stars like the Sun
> that are just empirically determined....The convective
> process...well I won't call it a theory...we have a kind of
> made up formula...a recipe...There are two arbitrary
> constants, but the Sun is sensitive to one of them and
> then you go and adjust the whole stellar-structure calculation
> by varying this constant [mixing-length ratio] till you
> predicted, in inverted commas, the radius of the Sun...Well
> that's a bit silly, it's not deductive.

A British scientist also had some sympathy and felt Rouse's earlier

contribution had not met with the credit it deserved:

> If you look back at some of the things he has done, it's very
> interesting actually...Some of the so-called corrections
> he has made to so-called early models, in terms of physics,
> are almost ahead of his time...It's only in the last few
> years that people like Ulrich and Bahcall bothered to do
> that sort of thing. And I think he should get some credit
> for this which he doesn't get.

It should be said that most respondents (perhaps, being

conscious of the racial issue) were at pains to stress to me that

they thought Rouse was a 'nice guy'. There was also general agreement

as to the quality of Rouse's scientific pedigree (PhD from Caltech)

and his early work on the equation of state. However , despite

these positive attributes, his work on solar models has almost

universally been ignored.

Rouse's work seems to have been rejected, not so much because

it gave different results to the orthodox models, but rather because

it challenged the whole basis on which stellar-evolution computations

were carried out. Because he made such radically different assump-

tions concerning the constraints on a viable solar model, most

scientists could either not understand his approach or else rejected

it out of hand. Furthermore, his approach tended to re-open problems

that were considered to have been solved years before. By 1964,

solar-model construction was a backwater of stellar-model con-

struction and most theorists were working on the latter stages of

stellar evolution.(Recall the reluctance of Iben and Sears to even

work on solar models - See Chapter 4). Rouse, by calling for more

rigorous solutions, was, in essence, challenging the whole industry

of stellar-model construction. Since Rouse's call for more rigor

was considered hopeless anyway, most model specialists were happy

to get on with an activity which did seem viable, rather than chase

'pie in the sky'.

Rouse's own marginal position (working for much of the time

in industry without funding for this type of research), and his

earlier failure to get into the mainstream literature meant that,

by the time the solar-neutrino problem drew increased prominence to

alternative theoretical approaches, he could make little headway.

Despite his failure to win acceptability, there do not seem to be any compelling reasons as to why Rouse's approach should be any less scientifically plausible than the orthodox approach. Indeed, in terms of the standard rhetoric of scientific argument, Rouse seems to have much going for him. He has been able to argue (Rouse, 1975) that his approach is more mathematically rigorous (because he needs fewer free parameters) and that it is more in tune with observations (since he attempts to match the observable radius of the Sun). And, as indicated above, no-one has been able to point to factual errors in his work. Furthermore, what was billed as the crucial test of the standard approach - the solar-neutrino experiment - showed that, if anything, the standard approach was incorrect. Rouse had argued this before the results of the solar-neutrino experiment were known.

Summary

The argument of the above section has been that Rouse's proposed solution to the solar-neutrino problem - that the standard solar model is unrealistic - is an equally viable theoretical approach in terms of standard scientific arguments, such as the match with experimental data and logical consistency. Indeed, in some respects his approach was more scientifically rigorous than the orthodox approach. By showing the plausibility of Rouse's model, we have deconstructed the   standard model. This points to the social construction of the standard model itself. The rejection of Rouse's approach (and by extension all other non-standard approaches , to which, in principle, the same argument could be applied), is to be found in the social world rather than

the natural world. The continuing success of the standard model
in the face of its many rivals will be discussed again in
Chapter 10.

Although the argument has been made in detail for Rouse's
approach only, to be consistent it must be applied equally across
the board. That is, the rejection of all the deviant theoretical
approaches must be considered to be a social process. If the
social-construction-of-scientific-knowledge thesis is to be pushed
to its limits, then it must be maintained that, not only are the
standard solar model and all the concomitant standard assumptions
concerning neutrino physics and nuclear physics social constructs,
but so also are all the theories and explanations referred to in
Table 9.1. Furthermore, we are not able to say that any of these
theories are scientifically more plausible than any other. Some of the
consequences of this view for our picture of scientific knowledge
will be pursued in Chapter 10.

NOTES FOR CHAPTER NINE

1. Since these solutions are taken from a broad sweep of time, and since the magnitude of the discrepancy between theory and experiment has varied over time, it is difficult to judge quantitatively how successful these proposals have been. For instance, an idea which might have lowered the neutrino flux by a sufficient amount in 1978, might not be drastic enough for the 1980 discrepancy. Also, it should be noted that, since there is nothing to stop different solutions being combined together, virtually any magnitude of neutrino flux desired can be predicted by taking the appropriate combination.

2. R.T. Rood, 'Review of Non-Standard Models', in Friedlander (1978a; 194).

3. The degree of ad hocness seems to be a singularly unconvincing way of ruling out such solutions. I found that there was no agreement amongst respondents as to what counted as ad hocness in this context. Also, explanations which one respondent would consider to be ad hoc would not be called ad hoc by other respondents.

4. But see Harvey's study of the local hidden-variables controversy (Harvey, 1980, 1981). It seems that one experimenter, having found a result at variance with orthodoxy, was not prepared to even publish his result, never mind press for it.

5. Rood, op. cit., 1978, note 2, 177-8.

6. This can be carried out by using 'hypothetical' arguments which a determined proponent might put forward. This formula was used by Harvey (1981), Collins (1981b) and in Chapter 7 where Jacobs's hypothetical response to the Cl^{36} test of argon trapping was discussed.

7. Letter, R. Sears to R. Davis, May 9, 1968.

8. It is inevitably difficult to define marginality. In Chapter 10, it will be argued that the cohesiveness of the solar-neutrino field was in part due to its dominance by the Caltech nuclear astrophysicists. The scientists being considered here were both marginal in the sense that they were not part of the Caltech hegemony. Neither were they in any other way in a strong institutional position - such as having a post at an elite N. American establishment.

9. This scientist had his own theory of how neutrino radiations affected living matter. He told me, during the course of the interview, how he had smuggled a radioactive source out of a laboratory to keep in his house. He believed this source would help prolong his life! The recovery of the plausibility of this scientist's beliefs was considered to be beyond the scope of the research (this respondent did, however, seem to be unusually sprightly for someone of his age!)

10. Michael J. Moravcsik, of the Institute of Theoretical Science,
    University of Oregon, who was a physicist colleague of Rouse's
    and who is now a science-policy analyst, has written to me
    concerning Rouse's work.   In commenting on a paper of the
    author's (Pinch, 1981a,in Appendix II), he suggested that 'this
    part of the story [Rouse's work] is a truly "sociological"
    element in the controversy, and should eventually be investi-
    gated in detail' (June 11, 1981). My decision to look at
    Rouse's work was taken in 1978 when I interviewed Rouse
    and obtained complete copies of his solar-neutrino corres-
    pondence files. Moravcsik, in his letter, went on to write:
    > ...Carl being black and hence in some ways not the
    > urban, smooth, cosmopolitan type of theorist, his
    > model has been given very little attention, though
    > nobody was ever able to point out anything in it that
    > could be agreed on as being wrong.
    As we will see in the text, I agree with Moravcik that Rouse
    has received little attention and that nobody has been able
    to find an 'error' in his work.   However, rather than attempting
    to explain his rejection in terms of his background and his
    behaviour as a theorist, I argue in the text and in Chapter
    10 that it is the fundamentally unorthodox procedures which
    he followed, procedures which did not promise solutions to
    the problems of stellar-evolution theory which other theorists
    could work on, which accounts for his lack of impact.

11. In 1973, Rouse wrote to three US senators in order to try
    and elicit their support in the face of the continual rejection
    of his research proposals by the NSF.

12. Rouse chose an experimental project for his PhD because it
    was a safer bet than a theoretical PhD that he could complete
    it in time.   However, he had a large mathematical component
    in his training and most of his subsequent scientific work
    has been on theoretical problems.

13. Rouse has told me that at the time he graduated it was almost
    impossible for Blacks to get academic jobs at good universities.
    He was offered several teaching posts in Southern (Black)
    State Universities.

14. This correspondence arose via Fowler whom Rouse had contacted
    in 1963 (Rouse knew Fowler from his days at Caltech).

15. I have not been able to see a copy of Rouse's original paper
    but a paper entitled 'A New Solar Model', presented to the
    American Association of Physics Teachers, New York, January
    27-30, 1965 (UCRL-12120), summarises the conclusions of the
    earlier paper.   Rouse wrote in this paper (p. 1):
    > No one really knows what the inside of the sun
    > is like...

16. Letter, R. Sears to C. Rouse, February 28, 1964.

17. Anonymous referee's report, July 6, 1964.

18. Rouse, ironically, was also in contact with Henyé at Berkeley.

19. Letter, S. Pasternack to C.Rouse, January 13, 1965.

20. Rouse has had a number of articles on his views of stellar-
evolution theory published in the European Journal, Astronomy
and Astrophysics, and in a series of books which he edited
(see Rouse, 1966, 1969b,1975).

21. Rouse had five proposals to the NSF turned down between 1968
and 1978. Rouse did, however, work on a NSF-funded project
in 1964. This research was an attempt to calculate the helium
abundance in the photosphere, and was carried out
at the Hulbert Centre for Space Research, Washington. This
work, although part of his research programme of attempting
to eliminate free parameters (such as the helium abundance)
from solar models - see below for details - could also be
considered a useful avenue to pursue from the point of view
of orthodox models since it might lead to a more accurate
determination of the notoriously unreliable helium abundance.

22. Rouse received this particular criticism from an NSF reviewer
on June 6, 1972.

23. In actual fact, usually a series of half-a-dozen models are
constructed to cover the full period of evolution.

24. NSF Review, sent to C. Rouse, June 6, 1972.

25. As might be expected, Rouse maintained that there was a
conflict between theory and experiment. See, C.A. Rouse,
Letter to the Editor, New Scientist, August 19, 1971, 437.
Rouse's letter, criticising the reporting in Science, was
refused publication by the Editor.

26. Two respondents told me that they rejected Rouse's work because
his model was even more inconsistent with the Davis result
than the standard model. However, as the standard model had
also failed and they still believed in the standard model it
seems questionable to reject Rouse's approach on such grounds.
Rouse probably did not help the chances of his approach being
accepted by, at the time, enthusiastically embracing Bandyopadhyay's
(1972) unorthodox theory of weak interactions. This theory
predicted lower absorption-neutrino cross-sections than the
standard theory and hence gave Rouse some grounds for hope that
the Sun might still work on the CNO-cycle. In advocating
this radical theory, Rouse appeared to be advocating unorthodoxy
to the power two!

27. After losing his NSF funding in 1968 (this was due to budget
cut backs in NSF), Rouse tried again without success to obtain
an academic post. Since 1968 he has worked as a staff scientist
and consultant for Gulf General Atomic, San Diego.

28. This term comes from Collins and Pinch (1979). It is elaborated
upon in Chapter 10.

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CHAPTER TEN

CONCLUSIONS: THE SOCIAL DECONSTRUCTION AND CONSTRUCTION OF EXPERIMENTAL

AND THEORETICAL SCIENTIFIC KNOWLEDGE

In this chapter the findings of greatest import to the thesis

as a whole are summarised.   These findings are further developed

in terms of both the social deconstruction and construction of

scientific knowledge.   The consequences of the 'deconstructivist'

viewpoint for the relativistic sociology of science is discussed.

An attempt is made to show the fruitfulness of interest models

and credibility models for understanding the social construction of

science.

Summary of Findings

In view of the breadth of material presented and the range of

sociological topics covered, it is helpful to start by summarising

the main points raised in each chapter.   These points will be

presented in the context of the overall goals of the social decon-

struction and construction of scientific knowledge.

In Chapter One, it was argued that research carried out within

the relativist programme in the sociology of scientific knowledge

needed to meet two goals.   The first goal is the social deconstruction

of scientific knowledge. That is, it has to be shown how a seemingly

hard-and-fast fact (or theory) of the natural world can be decon-

structed such that the interpretative flexibility which resides at

the heart of all human knowledge (i.e., socially produced knowledge)

can be recovered.   The second goal which the research must meet -

and it is upon this goal which much of the present work centres -

is to show processes whereby scientific knowledge is socially

constructed.   In other words, we must try and show how interpretative

flexibility vanishes from scientific knowledge such that scientists

reach a consensus about facts (and theories) of the natural world.

It was argued that the development of solar-neutrino astronomy

provided a suitable location for research orientated towards these

two goals.   Also, this research location provided an opportunity

to investigate problems raised by other studies of the construction

of scientific knowledge and of the roles played by theory and experi-

ment.

The two branches of science from which solar-neutrino astronomy

emerged - neutrino-detection physics and nuclear astrophysics - were

described in Chapter Two.   Davis's long-term experimental aims - to

detect neutrinos - and the nuclear astrophysicists' long-term

theoretical aims - to test the theory that stars have nuclear

energy sources - were spelt out. It is the meshing together of

these separate experimental and theoretical aims which sets the

scene for subsequent developments.   It was described how these

two aims came together in 1958 when the first realistic opportunity

arose to test nuclear-astrophysical theory by means of a solar-

neutrino detection experiment.

In Chapter Three , events associated with the experimental

project between 1958 and 1964 were described.   This chapter is

an important chapter in terms of the social construction of scienti-

fic knowledge because the emergence of social processes and relation-

ships vital for the future of the project are outlined.   The eventual

social construction of the facticty of Davis's result can only be

understood in terms of social processes dating back to this epoch

in the history of solar-neutrino astronomy.   In particular, attention

was focussed on the crucial need to get funding for the experimental

project.   The commitment of the Caltech nuclear astrophysicists

to the project was outlined and the processes whereby funding was

successfully obtained, in 1964, were documented. Attention was

drawn to the importance of publicising scientific activity as

part of the process of getting funding. Davis's relationship

with the theoreticians was described as an 'instrumental relationship'.

That is, Davis used the theory to further his own experimental

ambitions.

Chapter Four, which focussed on theoretical developments between

1958 and 1964, is important with respect to both the social con-

struction of the facticity of Davis's result and the social decon-

struction of the theoretical prediction. The instrumental attitude

of the Caltech nuclear astrophysicists, whereby they used the

experiment as an opportunity to further their theoretical aims, was

outlined. As has been stressed already, it was the involvement

and support of the theoreticians over this period which was to

prove to be so vital in understanding Davis's later successes in

getting his claims accepted. It was shown that, as a consequence

of their interests, the theorists were concerned to get the

experiment funded – and that, in turn, required them to predict

a large signal. The prediction of this expected signal was de-

constructed by showing the interpretative flexibility in certain

constitutive elements of the prediction. In particular, the flexi-

bility in the determination of the value of the parameter, S_{33}, and

the flexibility in the averaging of solar-model predictions were

shown. The need to predict a sufficiently large signal in order

that the experiment might be funded was, it was argued, a key factor

in the social construction of Bahcall's prediction of 1964.

Chapter Five is, in the main, a descriptive chapter. The period

covered took us from the funding of the experiment in 1964 until the

successful completion of the apparatus in July 1976. The financial

and technical problems which Davis met were described. The con-
struction over this period of three other, less-ambitious, solar-
neutrino detectors was also outlined.

In Chapter Six, the activities of the theorists, and in
particular Bahcall, were described as preparations were made for
Davis's first result. It was shown that by this stage Bahcall
had entered into a close partnership with Davis. He was for a while
Davis's 'house theorist'. This relationship between Bahcall and
Davis was to prove to be important for the reception of Davis's
result. Thus, processes vital to the social construction of knowledge
are described in this chapter. The deconstruction of the theoretical
prediction was also furthered in this chapter by the demonstration
of the interpretative flexibility over the certainty adduced to the
prediction. Bahcall's increased emphasis on the uncertainty in the
prediction, as opposed to the pre-funding emphasis on its certainty,
can be understood as part of the processes of social construction
of the theoretical prediction.

Chapter Seven is in two parts. In Part I, the social construction
of Davis's result as a fact of the natural world was outlined. It
was shown that Davis and the other neutrino experimentalists were
convinced of the veracity of Davis's claims almost immediately,
but that the theorists were not finally convinced until 1978. It
was argued that the pre-existing relationship between Davis and
the theoreticians helped him to maintain the public credibility
of his experiment. By an elaborate programme of experimental
tests, Davis was eventually able to convince the theorists of the
correctness of his results. In Part II, an attempt was made to
deconstruct some of the facticity of Davis's result. Attention
was drawn to the role of replication (and, in particular, the lack

of replications) in the acceptance of Davis's result. The arguments

of Jacobs that the result is an artefact of the chemistry of the

detector were described and it was shown that these arguments

could be viewed as having some force. The rejection of Jacobs's

arguments drew attention again to some of the processes of social

construction (in this case, destruction) of knowledge.

Chapter Eight is also in two parts. The first part dealt

with the immediate theoretical aftermath of Davis's result. Bahcall's

reaction was outlined and it was shown that he attempted to accommodate

the result within the range of the 'prediction' by modifying the

error range and the 'best value' of the prediction. Again, the

social deconstruction of the prediction was illustrated by a

comparison of Bahcall's prediction in 1967 with his earlier predictions.

It was argued that, to maintain his credibility as a theorist,

Bahcall constructed a prediction that did not conflict with the

experiment. However, other theorists, and in particular Iben,

questioned the degree of conflict between theory and experiment.

The interpretative flexibility in the notions of 'contradiction'

and 'consistency' was illustrated by considering the scientific

merits of both Bahcall's and Iben's views. Iben's failure to make

much impact again illustrated important social processes of know-

ledge construction (destruction in this case, again). The social

deconstruction and construction of the notions of consistency

and conflict were further illustrated in Part II. It was argued

that Bahcall's switch to the 'conflict thesis' by 1970 was rhetori-

cally expedient in terms of his overall scientific goals - to get

second-generation experiments funded. The reappearance of the

consistency/conflict debate in 1978 added weight to the attempt to

deconstruct these terms (the current debate has been pursued in

Pinch, 1980a, in Appendix II).

Finally, in Chapter Nine, the deconstruction of the standard

theory (rather than the standard prediction) was attempted by

describing the wider theoretical response to Davis's result. It

was argued that the interpretative flexibility of the standard

theory could be revealed by showing the scientific plausibility

of non-standard theoretical approaches. This argument was made,

in particular, for Rouse's non-standard theory. The rejection of

Rouse's approach again illustrated aspects of the social construction

(destruction) of theoretical knowledge .

Having outlined which pieces of the previous chapters are

relevant to the social deconstruction and social construction themes,

I will now develop further these two basic themes.

The Social Deconstruction of Experimental Knowledge

At the heart of any piece of relativistic sociology of science,

is the need to show that the production of experimental knowledge

can be understood as a process in which the natural world plays

a small or non-existent part. In this case, the piece of experimental

knowledge on which attention has been focussed has been Davis's

solar-neutrino results. The detailed argument has been made in

Part II of Chapter 7.

The standard technique for the deconstruction of an experimental

claim is, as outlined in Chapter 1, to locate a scientist or group

of scientists who dispute the result that is to be deconstructed

and then to show the plausibility of their arguments. In the event

of failing to find such scientists, or them failing to want to press

home their arguments, the researcher must exercise his/her own

ingenuity and invent hypothetical arguments which serve the same

effect.

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The deconstruction of Davis's result has not been easy to

achieve with standard methods since he is the only experimentalist

involved and no attempt has been made to repeat his experiment.

This lack of replication, however, can be used, in its own right,

as a means to deconstruct the result. By a comparison with the Weber

case, it was argued that the lack of replication in the Davis case

pointed to the possible defeasibility of his result. That is, the

Davis result could, in principle, have been rejected in a similar

manner to that in which Weber's has been rejected, if replications

had been attempted. The justifications scientists gave for their

belief in Davis's result, despite the lack of replication, re-

vealed some of the politics of the process of replication. There

was felt to be little benefit in attempting to repeat Davis's

experiment. However, some scientists, rather than acknowledging

the lack of replication, argued that the experiment was to be or

had already been repeated. For instance, second-generation detectors

(such as the gallium experiment) were regarded as potential repli-

cations, and for one respondent, Davis's own tests of his procedures

were even regarded as a replication. It seemed that what counted

as replication was itself a matter for negotiation. This finding

is in broad agreement with the work of Collins (1975, 1976) and

Travis (1981).

Another front, on which the attempt to deconstruct Davis's

result was pursued, was the more traditional means of deconstruction.

That is, a scientist was located who, for a period anyway, con-

tested Davis's experimental claims. Although this scientist,

Jacobs, had presented the main force of his argument a few years

earlier (1974-5), it proved possible to recapture some of his doubts

with the use of interview material and historical documents

(correspondence files and scientific papers). The present-day

facticity of Davis's results was further deconstructed by

considering how a 'hypothetical-Jacobs' could have responded to

the Cl^{36} test - the test which was found to be so convincing to

most scientists.

The criticisms Jacobs mounted are slightly different to those

encountered in most cases where an experimental result is disputed,

in that they centre on secondary experiments performed by the main

experimenter. In some senses these secondary experiments can be

regarded as 'calibration experiments'. That is, they follow the

same basic experimental procedures as are used in the primary

experiment but use different sources to produce the effect. In this

case the effect is the production of Ar^{37}, and, in one such

experiment, a neutron source was used, and, in another, Ar^{37}s were

physically placed in the tank. The conclusiveness of such experi-

ments lies in the claim that they mimic the main experiment in all

essential details. The way Jacobs circumvented this claim was to

make the counter claim that in some essential detail these experi-

ments were new and different. Hence their results did not serve

necessarily to bolster those of the primary experiment. Jacobs

argued that such experiments were not the same because he did not

accept all the assumptions made in the secondary experiment. For

instance, he did not accept the assumption that argon atoms behaved

for all intents and purposes in the same way as argon ions. For

Davis and for others who accepted the conclusions drawn from

secondary experiments involving argon atoms, it was ceteris paribus

that argon atoms behaved in the same way as argon ions. For

Jacobs such a claim was questionable, especially as he had his

own theory, which pointed to such an assumption being in doubt.

In essence, then, the argument over these secondary experiments

is similar to the arguments over replication documented in Chapter

1. It is just the Duhem-Quine thesis in another guise.

The above means of social deconstruction is relevant to Collins's

(1980) argument concerning calibration. It shows that scientists

need not necessarily be constrained by calibration experiments

(it appears that Weber did accept certain calibrations of his

apparatus). In principle, such experiments can be challenged in

the same way that any experiment can be challenged. Of course,

there comes a point, as Collins argues, at which a scientist can

no longer plausibly refuse to accept a calibration experiment

as reproducing the experimental technique of the primary experiment.

It is possible that Jacobs had reached such a point with the Cl^{36}

experiment. However, there is always the option that, having

accepted such an experiment <u>as a calibration experiment</u>, the

scientist can claim it was incompetently performed. It thus seems

that the general argument concerning the deconstruction of experimental

knowledge can be extended to calibration experiments as well.

There can be little doubt, as argued in Chapter 1, that the

task of social deconstruction would have been easier to carry out

if contemporaneous material had been gathered. For instance, inter-

views with Bahcall in the autumn of 1967, when his scepticism of

the experiment was at a height, and with Rood and Jacobs in 1974

would probably have made the social deconstruction task easier.

However, the claim made here is that more traditional historical

methods have sufficed in this case.

Of course, Davis's experimental result is not the only one encountered in this work. For instance, the earlier results at nuclear reactors, the results of other neutrino experimentalists and cosmic-ray experimentalists, and the whole range of experimental parameters upon which the theoretical prediction of neutrino fluxes are based (e.g., the nuclear-physics cross-section measurements) are all important experimental results which bear on the development of solar-neutrino astronomy. The deconstruction of every one of these pieces of experimental evidence is possible in principle, but is clearly beyond the resources of the present study. Each piece of evidence would require as assiduous an investigation as the deconstruction of Davis's results has required. Upon closer investigation, perhaps scientists who contested some of the above experimental claims could be found. Even without going into any detail, in the course of the present research I often came across intriguing references to experimental disputes. For instance, the 'wrong' experiments (mentioned in Chapter 2) in favour of neutrino-antineutrino similarity and in favour of the four-component neutrino theory are, I would suggest, just the tip of an ice berg.

In general, once the analyst is alerted to the possibilities of the deconstruction of knowledge it is easier to see hints that the basis of experimental knowledge is much less robust and straight= forward than is often portrayed in realist accounts. Even if the analyst does not get the whiff of an experimental controversy, he can always make the hypothetical argument - 'If someone wanted or cared sufficiently to contest any experimental result, then, in principle, they could'. This argument is not as weak as it first appears when we bear in mind that realists must habitually make a

similar hypothetical argument. No-one can possibly look at every experimental result in detail and often scientists (and presumably other realists) take such results on trust. The realist reasons that if every case was probed in detail then no doubt the real world would be found to be behind every result. But this is only a hypothetical argument. Of course, it would be unreasonable to ask a realist to show the influence of the real world in every case. Similarly, it is unreasonable to ask the relativist to deconstruct every experimental result that is encountered.

The Social Deconstruction of the Theoretical Prediction

This aspect of the research is quite novel since (to my knowledge, anyway), the deconstruction of a theoretical prediction (as opposed to the theory itself) has never before been attempted. The type of argument mounted has also been slightly different to that encountered before in the deconstruction of experimental knowledge. Because we are dealing here not with just one prediction, but a number presented over a period of time (see Fig. 1.1), it is possible to use the different predictions to deconstruct themselves. That is, a prediction produced at one time, which differs from the same prediction for a different time, can be used as a means to question the veracity of both predictions. The assumption is that both predictions cannot be correct and thus they can be treated as competing claims. The counterposing of such competing claims is, of course, the essence of attempts to deconstruct experimental knowledge. This approach is used throughout the sections on the theoretical prediction (Chapters 4, 6 and 8). For instance, Bahcall's and Shaviv's prediction on the eve of Davis's experiment was compared with Bahcall et al.'s lower prediction made after Davis's results

became available. The implication is that, as the two predictions

differed, in some senses this points to the interpretative flexi-

bility possible in the generation of such predictions.

The underlying assumption of the above argument is that both

predictions are based on the same knowledge. Of course, such an

assumption is, in the main, unrealistic, particularly for such a

horrendously complicated calculation as a solar-neutrino flux pre-

diction. As we have seen, such a calculation is dependent itself

on a whole variety of other experimental and theoretical inputs.

It is quite possible that these experimental inputs and sub-

theories themselves change over time. If this is the case then

perhaps it is less remarkable that the overall prediction should

change with time.   Such a change might simply reflect better and

better experimental and theoretical knowledge.

It is arguable that something similar to this has happened

in the solar-neutrino case - but the connection between the overall

theoretical prediction, sub-theory and experimental inputs is not

as straightforward as portrayed above. There is no standard way

in which revisions and updates in sub-theory and experimental

inputs are assessed.   For instance, the experimental values them-

selves may be in dispute, in which case, it is not easy to say which

value should be used.   For example, was Bahcall correct to take

Lambert's new value of Z, a value which Iben thought was too new

and controversial to use in solar-model calculations (see Chapter

8)? It seems that in general there is a built in inertia in the

production of the predictions such that 'standard' values tend to

be used unless there is a good reason for a change (after all, a

major change often requires a modification to the computer programme

used for such calculations). Certainly, it appears there is no

systematic way in which new experimental inputs and sub-theories

are incorporated into the predictions. The accidental way in

which Bahcall's attention was drawn to Lambert's new value of Z

will be recalled (see Chapter 8). In view of this, there seems

to be considerable room for choice and selection by the theorist

as to which input values and sub-theories are to be used. This

element of choice makes it very difficult to say for the pre-

dictions documented in the thesis, how much the changes in the

prediction were 'forced' upon the theorists by experimental and

theoretical events beyond their immediate control, and how much

the changes were a product of particular choices of input parameters.

In view of this difficulty, no strong claim can be made for

the deconstruction of the theoretical prediction by the comparison

of differing predictions made at different times. Such evidence

is a best suggestive,and at times it is very suggestive, especially

when the prediction changes over a very short time span (as in 1967-8).

The above reservations do not apply to a second method of

deconstruction which has been followed in this work. This second,

more conventional, method is to use disagreements between different

theorists over the correct value of the prediction at any one time.

For instance, the disagreement between Bahcall and Sears over

whether the best prediction, in 1964, was Sears's model J or

Bahcall's weighted average (see Chapter 4), is a good example of

such a disagreement. Another example is Bahcall's and Parker's

non-standard interpretation of the S_{33} data which led them to

favour a smaller value than normal for this parameter (again, see

Chapter 4). As in the experimental deconstruction case, the argument

proceeds by recovering the scientific rationality for each contending viewpoint. This method of social deconstruction of the theoretical prediction is more clear cut and is free of the problems raised by taking predictions at different periods of time.

The same comments, in regard to the limited number of predictions considered, as were made for the case of experimental deconstruction, apply here. The aspects of the theoretical predictions dealt with have only been those upon which it has been possible to marshal evidence. The comment made at the start of Chapter 4 on the paucity of evidence associated with the production of theoretical work is especially pertinent. The details of the derivations of all the solar-neutrino predictions just are not known and hence it is difficult to attempt their deconstruction. It is quite possible that the sort of analysis attempted for some aspects of the theoretical prediction (such as the disagreement over averaging procedures and the choice of S_{33}) could be extended to other parts.[1] Even if documentary evidence could not be found the analyst could always make a hypothetical argument. However, it must be said that to produce hypothetical theoretical arguments (and, by necessity, these must be quantitative arguments) of a convincing nature requires a far greater degree of competence in modern physical theory than the present author would presume to possess. That is why no such hypothetical arguments in the domain of theoretical predictions have been made in this work.

The Social Deconstruction of 'Consistency' and 'Contradiction'

In a way, much of the argument of the above section applies here as well, because it has been disputes over the details of the theoretical prediction, rather than the experimental result, which

have often been at the core of the debate over whether or not the

theory is consistent with or in contradiction to the experiment.

This was certainly the key issue in the disagreement between Bahcall

and Iben (Chapter 8, Part I). Their dispute provided a good

opportunity to deconstruct the notions of consistency and contra-

diction because their arguments were mounted at the same time, and

hence avoid the problems of comparing pronouncements of consistency

and contradiction made over a period of time. Consistency and

contradiction were deconstructed by showing that Bahcall's .

argument for consistency and Iben's argument for contradiction

both possessed a degree of scientific plausibility. The current

(as of 1978) controversy over whether or not there is a serious

discrepancy between theory and experiment (Chapter 8, Part II;

Pinch, 1980a in Appendix II) is also highly relevant to the de-

construction task because the arguments again occur within the

same time frame. This last controversy over consistency and

conflict has a further advantage. Because the arguments have

occurred contemporaneously with the fieldwork they provide a

rich source of data with which to demonstrate interpretative

flexibility.[2] It was argued in Chapter 1 that contemporaneous

interview data are the best for deconstructing scientific

knowledge.

A less powerful means of deconstruction is the attempt made

to counterpose Bahcall's views on consistency and contradiction

at different times. For instance, in 1968 Bahcall argued for

consistency but in 1970 he claimed a contradiction (Chapter 8,

Part II). However, the temporal element introduced by this kind of

comparison raises problems discussed in the previous section.

Perhaps the theory and the experiment themselves changed over time

and this 'forced' Bahcall to change his mind?   Indeed it would seem

that Watson's work on the opacity, the new measurement of S_{17} and

Davis reporting lower results, did all combine to have this effect.

The view that contradictions can be socially negotiated has

been suggested by Travis (1980b). Travis used examples from his

study of the 'memory transfer' controversy to show that experimental

evidence could be presented to be either consistent with or in

contradiction with certain theoretical hypotheses (e.g., that

memory transfer is mediated by RNA or proteins). The view that

consistency and contradiction are both socially negotiated was

also taken by Pinch and Collins (1979) and Collins and Pinch (1982).

By the study of arguments over the compatibility of parapsychology

and orthodox science, we were able to show that both the conclusions

that the two areas were compatible and that they were incompatible

could be legitimately defended.

The thrust of all these arguments which attempt to deconstruct

consistency and contradiction is that logic itself cannot resolve

scientific disputes. It seems that it cannot be decided whether

we have a case of p or not-p because the p-ness or not-p-ness is

itself subject to interpretative flexibility.   The interpretative

flexibility at the heart of demonstration of logical contradiction

has been illustrated by Roger Brown in his criticism of Festinger's

cognitive dissonance theory.[3]   Brown points out that the two

statements:

> Man will in the near future reach the moon
> Man will not be able to leave the earth's atmosphere

are not (as Festinger claimed) contradictory. This is simply because

'someone might figure out a way of moving the earth close to the moon

so that we could step across without leaving our familiar atmosphere'
(Brown, 1975: 595).   Although this is implausible, it is not
impossible. Thus, Brown showed that a paradigm case of contradiction
is defeasible by the application of sufficient imagination.   In
principle, the willingness to be sufficiently imaginative is no
different from the preparedness of scientists to challenge ceteris
paribus clauses.   Such challenges may seem implausible but there
seems to be nothing in logic to prevent them from being made. Logic
is no more a guarantee of veracity in scientific argument than
experiment and theory have been shown to be.

The extension of the argument concerning the socially negotiated
character of consistency and contradiction which is taken up in
Pinch (1980a) (in Appendix II) takes us into some new territory.
This is because much of the argument centres on a technical stati-
stical criterion (the so-called 'three-sigma' level of significance)
with which consistency and contradiction can be assessed.   The
deconstruction of this statistical criterion illustrates that
statistical rules of thumb are in themselves no guarantee of
veracity.   Such criteria are shown to exhibit the interpretative
flexibility with which we are by now familiar. The appeal to statistics
is no more an appeal divorced of social processes than the appeal
to experiment, theory and logic.

The conclusion to the paper presented in the Appendix (Pinch,
1980a) is commensurate with the view argued by MacKenzie that the
sociology of knowledge can be extended into mathematical areas,
such as statistics.   MacKenzie (1978), in his study of the con-
troversy between Yule and Pearson over the correct way to
associate nominal variables, was able to deconstruct this area
of statistics by showing the plausibility of each side's argument.[4]

The focus of MacKenzie's argument and that in Pinch (1980 a)is, however, different. MacKenzie deconstructed statistical theory as it was being developed. That is, statistics were treated as an object of investigation in the sociology of knowledge in the same way that the development of solar-neutrino physics is the object of attention of the present study. However, my focus on statistics has been on their application in physics. Thus the arguments documented may bear very little resemblance to the concerns of statistical theory.[5] In that this is one of the first attempts to deconstruct statistics as used in a modern physical science it is perhaps of some interest.

The Social Deconstruction of the Standard Solar Theory

The deconstruction of the theoretical prediction (discussed earlier) is not the same as the deconstruction of the standard theory itself. For instance, in the debate between Bahcall and Sears over averaging procedures, it was not the standard theory which was at issue. They both agreed that the standard theory (and the concomitant standard physics) was to be used - they disagreed rather over the best prediction which this theory generated. However, the standard theory itself must be deconstructed if we are to carry through the full aims of the relativist programme.

The deconstruction of the standard theory was attempted in Chapter 9. In order to deconstruct the standard theory, the now familiar technique of finding someone who does not support the standard theory and showing the plausibility of their arguments, was followed. The case was made specifically with reference to Rouse's non-standard approach to solar models. It was shown that scientifically it was very difficult to fault his arguments. Apart

from the consideration of Rouse's work, some of the more general

criticisms which can be levelled at the standard theory and the

standard physical assumptions of nuclear physics and neutrino

physics have been documented in Pinch (1981a) in Appendix II.[6]

This paper, thus, carries the deconstruction task further.

The deconstruction of scientific theory has been shown in many

other studies. For instance, in my earlier study (Pinch, 1977)

of Bohm's work in quantum mechanics I attempted to deconstruct

the standard interpretation of quantum mechanics by showing the

plausibility of Bohm's hidden-variable interpretation. Similarly,

Harvey's (1980, 1981) efforts to show the plausibility of local

hidden-variable theories is a means by which the standard inter-

pretation of quantum mechanics can be deconstructed. Pickering's

(1980) recovery of the plausibility of the colour hypothesis is

another exemplary study in the deconstruction of scientific theory.

Deconstructivism

If we are to take our concerns as 'deconstructivists' seriously,

then at some point the picture of scientific knowledge as a whole

which is emerging must be considered. In this final section on

the deconstruction of scientific knowledge, I would like to use the

theoretical response to Davis's result to illustrate some of the

broader consequences of deconstructivism.

As pointed out in Chapter 9, Rouse's theoretical approach was

only one of many alternatives to the standard theory. If we are

prepared to believe that Rouse's theory is scientifically plausible,

then we seem to be obliged to believe in every alternative to the

standard theory and, of course, the standard theory itself. This

might seem to leave us with a bizarre picture. In its most dramatic

form, it means we must entertain a Sun with a central black hole

and a Sun with no fusion reactions at all. They are both equally viable possibilities.

This picture does not, however, have unpalatable consequences as long as the <u>distinction</u> between the scientist's and the analyst's world views is drawn. A scientist must necessarily impute reality to only one theory in order to proceed. Hence, most scientists will reject all the theoretical possibilities (such as are listed in Table 9.1), except perhaps for the standard theory or their own cherished theory.[7] There is thus no difficulty for scientists because they only grant ontological status to, at most, one theory. There is also no difficulty for the sociologist. The above situation, where several alternative scientific realities must be seriously entertained, <u>is no more than an extreme case of what we as deconstructivists must always expect.</u> The myriad of physical possibilities revealed by the theoretical reaction to Davis's result simply indicates the vast number of physical realities and ontologies which are permitted by the <u>social construction</u> of reality. As long as the sociologist treats reality as being socially constructed, there is no great ontological problem in different (and often seemingly incompatible) realities being considered possible.

In a sense, the solar-neutrino result of Davis has merely acted as a stimulus for scientists to exercise their collective imaginations and their abilities to construct reality in different ways. It is <u>merely</u> a stimulus because all these possibilities (and many more which we do not have the imagination to perceive) must have resided in run-of-the mill nuclear physics, astrophysics and neutrino physics <u>before</u> Davis's result was known. In other words, if anyone had been imaginative enough (or perhaps perverse enough) all these scientific possibilities could have, in principle,

been produced before Davis's result was known.

Similarly, it can be argued that in any area of what looks like solid knowledge (as stellar-evolution theory appeared to be before the Davis result) such potentialities exist. It is just that most of the while such possibilities are not realised even to the limited extent which we have seen in this area. The theoretical response to Davis's result is thus very illuminating to the sociologist. It brings us a glimpse of the 'Pandora's Box' which resides behind what seems to be for most of the time solid reality. For instance, who would have thought before Davis's experiment that there was a black hole in the middle of the Sun? If we are willing to grant the plasticity of the Sun perhaps other areas of seemingly hard-and-fast reality will become less immutable to sociological explanation.[8]

Thus, I would argue that the deconstructivist sociology of science (as proposed in Chapter 1) provides us with a self-consistent world view. The world view is analogous to that possessed by physicists (such as Heisenberg), who base their conclusions on the foundations of quantum mechanics.[9] According to their view of quantum mechanics, reality consists of a world of potentiality which is only actualised by the famous 'collapse of the wave function'. For the deconstructivist, the natural world is treated as a world of potentiality which may be constructed in an infinite number of ways. In practice, only a few of the myriad potentialities are actualised. Similarly, in quantum mechanics, only one state, of the infinite number of possible states of a system is ever observed. The deconstructivist is in some ways like the physicist who writes wave functions for the possible states of the system. The deconstructivist

must show some of the possible states of reality before they are

actualised away.

In order to understand why only some realities get constructed,

we must now move on to the second task of relativistic sociology

of science - the social construction of scientific knowledge. In

other words, we need to show some of the social processes which have

led to reality being ascribed to, for instance, Davis's results,

but reality being denied to, for instance, Rouse's theory.

The Social Construction of Scientific Knowledge

Before considering the construction of specific pieces of

scientific knowledge encountered in this thesis, I will first

consider the social construction of scientific knowledge more generally.

The difficulties of producing a general account of why some knowledge

claims in science succeed and others fail should not be underestimated.

It is clear from the case studies that have been carried out, even

within the narrow confines of modern physics, that the social

processes of science are extremely complicated. Simple answers,

such as that the successful ideas are aligned with the dominant

ideology, dominant class interests or powerful institutions, are

simply not adequate. There can be little doubt that the delineation

of the social processes of knowledge construction within today's

scientific establishment calls for subtlety in the analysis. It

also calls for ideas which are capable of capturing the complexity

of the processes we wish to explain. Within the new sociology

of scientific knowledge such ideas (as, for instance, discussed in

Chapter 1), have tended to evolve along with the case studies.

Unfortunately, because the theoretical categories used have tended

to be embedded in the empirical work, they are often not easily

generalisable.

As a means of attempting to make the present study less narrow,
I will express the findings in terms of a general schema for the
analysis of the social construction of scientific knowledge. This
general schema, which I shall refer to as the 'interest-credibility
model', is a fairly complicated model of scientific activity.   I
make no apologies for this. It seems, as emphasised above, that if
we are to do justice to the complexity of modern science, then only
ideas which are capable of expressing a variety of social processes
will be adequate.   The 'interest-credibility model' is drawn from
two closely aligned ways of explaining social processes in science
which have emerged from recent work.   The two areas of explanations
are respectively, 'interest models' and 'credibility models'. I
will discuss each in turn.

Interests and the Social Construction of Scientific Knowledge

Much of the interest in 'interest models' seems to have stemmed
from Barry Barnes's (1977) book Interests and the Growth of Knowledge,
and, in particular, the use to which Barnes puts certain ideas
drawn from Habermas. The general role of interests is illustrated
by the following passage, where Barnes writes:

> ...knowledge has the character of a resource, communally
> exploited in the achievement of whatever interests the
> actors decide.   And precisely because of this, knowledge
> is always primarily linked, in its generation and initial
> evaluation, to an interest in prediction and control.
> (Barnes, 1977:16).

It is clear from Barnes's writings that the primary interest of
scientists is the instrumental interest of prediction and control.
Further illumination on the issue is to be found in Barnes's
and MacKenzie's (1979) article, 'On the Role of Interests in
Scientific Change'. They write, concerning scientists'methods of
evaluating paradigms:

>They [scientists] assess their potential as resources in
>the pursuit of instrumental interests... (Barnes and
>MacKenzie, 1979:52).

Barnes and MacKenzie also identify <u>social interests</u> which are closely

related to instrumental interests. They write:

>The situational patterns of instrumental interests...are
>generally in turn related to a set of social interests.
>Often...this set of social interests is simply part of
>the esoteric organisation of science itself...This may
>generate certain shared esoteric or social interests:
>interests in the resolution of a certain particular set
>of puzzles and problems; in the continuance-in-use of
>central techniques, competences and theoretical structures;
>in the uncovering of areas of applicability for such
>techniques, competences and structures; perhaps in the
>maintenance of the group's image as a specialism with
>notable existing achievements; certainly in the availability
>of continuing opportunity for activity and the exercise
>of skills by members of the group. (Ibid: 53).

They go on to write:

>Such social interests serve to particularise the
>instrumental interests which prestructure the evaluations
>of the group....(Ibid: 53).

Barnes and MacKenzie point out that instrumentally related interests,

such as those just outlined above, are not the only interests possible

and that 'more general social interests' (Ibid: 54) can also

affect scientific evaluation.

As the interest literature has developed, it seems that it

has become usual to refer to these 'more general' social interests

- interests which derive from the wider social and political arena -

as <u>the</u> social interests. This certainly appears to be the way in

which Shapin uses the term in his (1979) study of the Edinburgh

phrenology debates. Likewise, MacKenzie (1978), in his study of

the Yule-Pearson controversy, identifies social interests with the

wider social and political milieu and, in particular, the changing

social structure in Britain at the time. It seems that the

instrumentally related social interests described above by Barnes

and MacKenzie which are 'internal to science' are to be identified

as 'cognitive' rather than social interests.[10]

Pickering (1980) in his study of the charm-colour debate in

particle physics also seems to use interest models in the sense

of interests internal to science (i.e., cognitive interests).

Pickering combines the Kuhnian term 'exemplar' with interests.

He writes:

> An exemplar is an example for some particular group - the
> group which has established the preceding body of practice..
> One can speak of the group or groups having expertise relevant
> to the articulation of some exemplar as having an 'investment'
> in that expertise, and as a corollary, as having an 'interest'
> in the deployment of their expertise in the articulation of
> the exemplar. An 'interest' then, is a particular constructive
> cognitive orientation towards the field of discourse.
> (Pickering, 1980: 109).

Having shown the sorts of factors which constitute interests,

it next has to be asked how such interests are relevant to the

construction of scientific knowledge.[11] Clearly, if interests in

some sense pre-determine scientific evaluation, then those knowledge

claims which are evaluated as 'false' must fail to intersect with

predominant interests. Unfortunately, Barnes, MacKenzie and Shapin

do not use interests specifically to show why some knowledge claims

succeed and others fail. Their concern has been more to understand

scientific activity in general and in particular how interests can

be used to explain such activity - that is they show how different

interests are manifest in different knoweldge claims. For example,

the scientific differences of Yule and Pearson are identified

with the possession of different cognitive interests which ultimately

might be linked with different social interests. Similarly, Shapin

is concerned to identify the different social interests of

phrenologists and professional anatomists. Pickering is more

adventurous in this regard, in that he offers an account of the demise

of the colour research programme. According to Pickering, the failure of colour is to be explained by its failure to become entrenched in the practices of the theoretical high energy physics community. In other words, the colour protagonists, unlike the charm protagonists, were not able to construct any exemplars which intersected with the interests (and practices) of other theoretical groups (for more discussion of this case, see Chapter 1).

The modus operandi of interest explanations for the construction of scientific knowledge would thus appear to be as follows. Those pieces of knowledge which are successful (i.e.,which become con-sensual facts or theories of the natural world) intersect with the presence of pre-existing interests. Conversely those knowledge claims which die do not intersect with any pre-existing interests, or at least if they do, these interests are not as powerful as those with which the successful claim intersects. This last caveat is necessary because it would seem from the writings of Barnes and MacKenzie (1979) on the variety of interests embodied in scientific practice that it is impossible to conceive of claims being put forward which do not interesect with any interest what-soever. As I read Barnes and MacKenzie, interests are the essential goals which motivate all scientific activity. In view of the many possible types of interest which can be identified, it seems that some sort of notion of a hierarchy of interests is required if we are to explain fully why some claims are successful and others fail. Although advocates of interest approaches do not specify explicitly what this hierarchy consists of, it would seem that narrow instrumental interests , as produced by the over-riding interest in prediction and control, are probably the predominant interests in

modern science. Pickering's account of the demise of colour,
thus translated, should perhaps be that the colour protagonists
were not able to intersect successfully with the predominant
interests of other theoretical groups in prediction and control,
such interests being embodied in the different pre-existing theore-
tical practices.

Given the above (brief and no-doubt over-simplified) account
of what is meant by 'interest explanations'[12] we next need to
ask how such explanations fit in with the concerns of the present
work. The first point to be made is that the wider meaning of
interest as 'social interest' (an interest reflecting political
and social processes which arise <u>outside</u> of the scientific community)
is not relevant to the present focus on processes internal to science.
The term 'social interest' is perhaps most suitable for the goals
of the third stage of the relativist programme - the demonstration
of the impact of wider social and political processes on the content
of scientific knowledge.[13] Indeed, those case studies which have
found the notion of social interests most useful have all been
located in just the sorts of areas where we might expect wider
social and political processes to have a part to play. That is
before the professionalism of modern science (Shapin's study of
phrenology) and at a time of paradigm incommensurability (Barnes
and MacKenzie on the Yule-Pearson controversy).

The narrower meaning of interest as 'cognitive interest' would
appear to be more promising. It is clear that many of the applications
of interests described by Barnes and MacKenzie (1979) cover just
the sort of scientific activities upon which I wish to focus.
For instance, many of the activities encountered within this thesis
fall within the category of interests which ensure the 'availability

of continuing opportunity for activity and the exercise of skills

by members of the group' (Barnes and MacKenzie,1979:53). In the

context of the development of solar-neutrino astronomy, the pursuit

of funding by the Caltech and Davis groups can be seen as an interest

which ties in with the 'availability of continuing opportunity'.

Indeed, as has been stressed repeatedly, the pursuit of instrumental

interests by the Caltech group and by Davis lies at the heart of

their collaboration.

The interest model thus seems suited to describe some of the

activities encountered in the present research. However, before

showing its fruitfulness in more detail, I briefly want to disauss

another approach to the social construction of knowledge which also

promises to be useful. This approach is the so-called 'credibility

model', as outlined by Latour and Woolgar (1979).

Credibility and the Social Construction of Scientific Knowledge

Let us start by recalling part of an earlier quote taken

from the writings of Pickering. It was noted by Pickering that:

> One can speak of groups [of scientists] ...as having an
> 'investment' in that expertise, and as a corollary, as
> having an 'interest' in the deployment of their expertise...
> (Pickering, 1980:109).

The association of the economic term 'investment' with the notion of

'interests' has not only been made by Pickering. The same association

has been made by Law. In a recent paper on sedimentology he writes:

> Crudely, the agent is seen as constructing action by selecting,
> manipulating or transferring his resources in accordance
> with his interests. In the context of science such resources
> include knowledge acquired through scientific training, as
> well as 'reputation'. Interests are partially related to
> scientific resources. Thus, everything else being equal,
> the scientist will utilise his resources to produce a return
> in the form of knowledge which may be exchanged for prestige,
> financial reward, or other commodities. Scientific action
> is thus structured in terms of the likely forthcoming rewards
> and the extent of prior investment in terms of given resources.
> (Law, 1980:16).

The current interest in 'investment' notions has stemmed largely from the study made by Latour and Woolgar (1979) of the day-to-day activity in a modern scientific laboratory.[14]   In order to make sense of inter-relationships between individual scientists, groups of scientists, the laboratory as a whole, and other scientific laboratories, within the overall context of the production of scientific knowledge, Latour and Woolgar found it useful to introduce the notion of 'cycles of credibility'. This idea stems from the reward system of science outlined by Hagstrom (1965) and from the economy of scientific authority outlined by Bourdieu (1975).[15] Latour and Woolgar suggest that it is 'credibility' which scientists invest. Such investments are made in a market in which there is a demand for credible information. Credibility, it seems, can take on different forms and individual scientists can be described as being caught up in a 'credibility cycle' whereby different forms of credibility are converted one to the other (a typical cycle is show in Fig. 10.1). Thus, the activity of producing scientific results enhances a scientist's stock of credibility. 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. In this view, knowledge is to be seen as a resource of previously acquired credibility.

If scientists are engaged in a struggle to maximise their credibility, then we can expect them to exploit the specialisation of modern science to further their investment strategies. By the formation of partnerships, it is possible for individuals or groups

Fig. 10.1 (From Latour and Woolgar, 1979)

Cycles of Credit
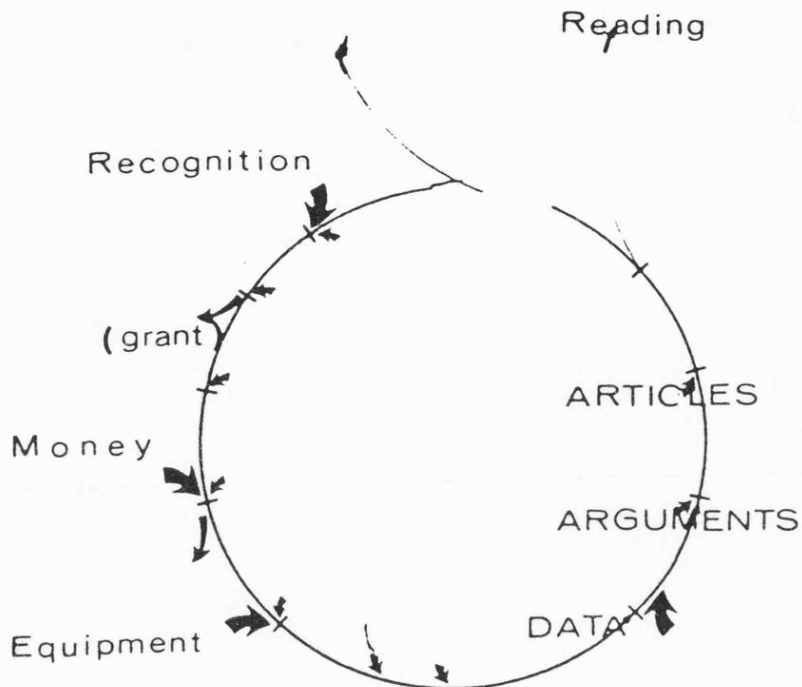
This figure represents the conversion between one type of capital and another which is necessary for a scientist to make a move in the scientific field. The diagram shows that the complete circle is the object of the present anaysis, rather than any one particular section. As with monetary capital, the size and speed of conversion is the major criterion by which the efficiency of an operation is established. It should be noted that terms corresponding to different approaches (for example, economic and epistemological), are united in the phases of a single cycle.

with quite different areas of expertise to come together to make a

joint investment. Latour and Woolgar mention several cases where

one group gives money and equipment to another group in the expecta-

tion that they will produce results that are of use to the first

group. This enables both groups to boost their credibility.

One of the most attractive features of the credibility model,

like the interest model, is that it describes the sort of activities

central to the present concerns. For instance, scientists' attempts

to raise funds, publish, pursue careers and enter into partnerships

with other scientists, are all activities described within the credi-

bility model - they are also the sorts of activities described

throughout this thesis. Given this potential fruitfulness, how

can it help us to understand why some scientists get their knowledge

claims accepted whilst others fail? Although Latour and Woolgar

and Law do not consider this question directly, I would suggest

that the _modus operandi_ of the credibility explanation is very

similar to that of the interest explanation. That is, those

scientists who are successful are those who have acquired the largest

amount of credibility, or have had the largest amount of credibility

invested in them, whilst those that fail have not been able to

accumulate sufficient credibility.

It is possible to see interest models and credibility models

as aspects of the same general type of explanation. The focus of

Latour and Woolgar on the internal workings of science seems ripe

for connection with the pursuit of narrow instrumental interests

which Barnes and MacKenzie associate with internal scientific

activity. I suggest that we regard the credibility model as pro-

viding a detailed account of structures and processes whereby

<u>particular interests become manifest in scientific activity</u>. Thus, the pursuit of any particular form of credibility (and remember it can take on many forms, such as, for example, funding and reputation) can be said to correspond to one of the subset of instrumental interests identified by Barnes and MacKenzie. In other words, the pursuit of a particular form of credibility is the pursuit of a particular cognitive interest.

This means of treating interests has the advantage of locating interests firmly in the practice of scientists.[16] Because scientists make investments of credibility in the hope of a future return there is a temporal constraint on them maintaining an interest until that investment is realised. And, of course, as further investments in the technique are made the interest grows. This means that interests develop along with investments of credibility and hence along with scientific practice itself. Without scientific practice and activity there can be no interests. Thus, interests are not to be viewed as abstract entities dissociated from scientific practice; they are defined by such practice.

One final reason why notions of investment and career considerations of scientists are such a fruitful way of describing modern science is that scientists themselves often think explicitly in these terms. This was discovered by Latour and Woolgar in their study and my own interview data are replete with such examples (see, for instance, the quotes from Bahcall on p.2 0 3 and p.2 2 2)· Often respondents would talk about their work as being a good or bad 'investment' and assess options explicitly in terms of investments and career considerations.

In bringing together interest models and credibility models in the above way, I have no doubt done violence to the intentions of

the proponents of such ideas.[17]   However, it seems to me that they

can be usefully combined as a way of describing and perhaps under-

standing the details of scientific activity and as a way of attempt-

ing to explain why some ideas meet with success and others fail.

The efficacy of the combination of interest and credibility can

be judged from the following sections where I attempt to account

in more detail for the social construction (and destruction) of

knowledge in solar-neutrino astronomy. The proof of the interest-

credibility pudding is, as always, in the eating!

The Social Construction of Experimental Knowledge

The main object of this section will be to try and account for

the experimental activities described in the previous chapters and,

in particular, to show the potential of the interest-credibility

model as a means for understanding such activity. The culmination

of the account will be an attempt to explain some of Davis's

success in getting his experimental claim accepted as a fact of the

natural world. Although the main focus will be on experimental

activity, the involvement of the theorists and the relationship

of Davis with them, will also be discussed. As mentioned before,

the acceptance of Davis's result can only be understood in terms

of this relationship. The detailed material of relevance to the

argument has already been presented (in Chapters 2,3,5,6 and 7).

Here, I will merely be paraphrasing some of the argument and

expressing it in terms of the interest-credibility model.

One of the main themes of the argument made in earlier chapters

was that Davis's research programme has throughout been orientated

towards the narrow experimental goal of detecting neutrinos.This

research goal can be described as his main 'interest'. This interest

does not seem to be connected with any obvious wider 'social'

interests. However, it can be seen to have arisen from a combination

of other pre-existing interests. It will be recalled that Davis's

involvement in neutrino detection commenced in the early 1950's

when he came to Brookhaven as a chemist. Dodson, his departmental

chairman, pressurised him into working on something nuclear -

mainly because the Brookhaven nuclear reactor was just becoming

available for research purposes. Davis's training as a chemist

constituted one pre-existing interest. Dodson's interest in building

up a group of nuclear chemists (chemists that could produce credible

information for the large nuclear-physics group at Brookhaven, and

in turn, exploit the physicists' facilities) was another important

pre-existing interest.

It was from these combined interests that Davis commenced

his neutrino-detection programme. Having made continual investments

in neutrino detection throughout the 1950's, it can be seen that

by 1956 this interest was part and parcel of Davis's professional

identity. This interest defined the types of experiments he

did and his involvement with such experiments defined his interest.

However, it would seem that by 1956, Davis's career had reached

an impasse. The major part of his work in the 1950's had

been in the development of the chlorine-37 neutrino detector. His

investment in this particular technique had shown some return with

the results of the reactor experiments. He had produced credible

information which was of some use to theorists and experimenters.

In particular, he had finally refuted the theory that the neutrino

and antineutrino were identical, and the four-component neutrino

theory. Both these theories were already unlikely possibilities

and thus Davis confirmed the theorists's expectations. Also, his

experimental result at Savannah River was useful to Reines for it

enabled him to conclude definitely that he had observed neutrinos.

However, having largely completed his work at nuclear reactors,

and not having detected anything, Davis, in 1956, had to find

another source of neutrinos to which his technique could be applied.

If he could not find such a source his previously acquired credi-

bility with this technique was in danger of not being put to use

in further investments.

The only source available that he might be able to detect with

this type of equipment was neutrinos from the Sun. Hence, it would

seem that the only way he could further his interests was by

mounting a solar-neutrino detection experiment.   The problem

Davis faced, however, was that it did not seem that there were

enough detectable neutrinos to warrant the rest of the scientific

community investing the large sums of money needed for such an

experiment.   In other words, it did not seem likely that such an

experiment would produce any information which would be of use

to anyone else;   that is, it did not seem likely until 1958.

The events of 1958 were of crucial significance because

they got the theorists involved with Davis's programme.   With

the new developments in nuclear physics which indicated the

importance of B^8 neutrinos in the Sun, the nuclear astrophysicists

immediately realised that Davis's experimental plans could potent-

ially be used to confirm their theories .   The nuclear astrophysicists'

goal was to test whether the energy source of stars was nuclear.

This was the assumption upon which many of the advances in stellar-

evolution theory and theories of the origins of the elements had

been based. Although these theories could be tested in many

indirect ways, and they seemed to hang together as a body of

knowledge, no direct test of the key assumption of nuclear-energy

generation, had been possible. The direct test provided by the
detection of solar neutrinos would thus serve to confirm a wide
body of theory. The nuclear astrophysicists aims, to use solar-
neutrino detection as a means of testing their theories, can be
described as their 'interest'   in the solar-neutrino project.

There is some evidence to indicate that the nuclear astro-
physicist's involvement may also have served another 'interest',
namely to bolster the image of nuclear astrophysics as a proper
'hard'science.   It will be recalled that nuclear physicists such
as Goldhaber regarded astrophysics in general as a 'loose' area
because astrophysicists could not make the precise quantitative
tésts familiar from other areas of physics. The precise prediction
and test made possible by a solar-neutrino experiment could thus
be seen as an opportunity to improve the image of astrophysics,
and perhaps convince other 'hard nosed' scientists that it should
be taken seriously.

Although the nuclear astrophysicists and Davis both had
different instrumental interests in the project, these interests
were of mutual benefit to each group. Davis would have a rationale
for doing the experiment and nuclear astrophysicists would have a
way of testing their theory. If a 'deal' could be struck between
the two groups then the credibility of both groups would be enhanced.
Davis, with the support of the nuclear astrophysicists, would stand
more chance of getting funding for his experiment and hence the
opportunity to further his credibility, and the nuclear astrophysicists
would have an opportunity to test their theories - a test, which,
if successful, would undoubtedly enhance their reputation as producers
of credible information.

As we saw in Chapter 3, the origins of the 'deal' lay in Fowler's

exchange of correspondence with Davis in 1958. In these letters

Fowler acknowledged Davis's abilities as an experimenter, pointed

to the importance of the experiment for the theory, and offered to

help him with getting funding. As subsequent events unfolded, the

key role of the Caltech group in getting funds for the project

became clear. Not only did they provide the predictions which

justified the experiment but they interceded directly with important

individuals who had a role to play in the funding decision. They also

helped publicise the experiment, which again was useful in the attempt

to get funding. Davis, in turn, kept the Caltech group informed

of his progress and he continued to strive towards showing the

experimental feasibility of the project. After the project was

funded, the theoreticians and Bahcall in particular, continued to

be closely associated with the project. Indeed, at one point

Bahcall's involvement was such that he became the 'house theorist'.

By this stage (1966), Bahcall had struck up a close relationship

with Davis. As Bahcall himself acknowledged, the carrying out

of the experiment became vital to the advancement of both their

careers. Their 'interests' and joint investments of credibility

had by now become intertwined with personal relationships and even

psychological states of mind.[18] Thus, between 1958 and 1967, a

successful partnership had been struck up and the nuclear astro-

physicists had invested heavily in Davis's project.

When Davis got his result in 1967, we saw that other chemists

and neutrino-experimenters were immediately convinced that he

was correct. This was largely because of Davis's pre-existing

credibility - he was known after a lengthy experimental career to

be the leading expert with this technique. The nuclear astro-
physicists,and in particular, Bahcall, were initially sceptical,
but they did not push their scepticism into a full-blooded public
denunciation of the experiment. As we saw, Bahcall for a time
expressed serious worries to Davis that he was not interpreting
the experiment correctly. The report of even lower results by
Davis, in 1972, seemed to give fresh impetus to the nuclear astro-
physicists' worries. Several such worries were expressed informally
to Davis at the Irvine conference. However, again, no public
denunciation resulted. By the time Jacobs's attack appeared, in
Nature in 1975, Davis seemed to have largely carried the day.

The reason Davis has been able to maintain his credibility
in the face of the nuclear astrophysicists' scepticism, lies, I
believe, in his pre-1967 partnership with the nuclear astrophysicists.
From the very start Davis was aware of what the Caltech group
had invested in the project and he realised that the partnership
entailed that he should make a special effort to convince them he
had got it right. His strategy, as we have seen, was to be
deliberately open with the data (all his data were mailed to the
Caltech group); to encourage nuclear astrophysicists to visit the
experiment and 'see it for themselves'; to take seriously all the
theorists' criticisms, no matter now bizarre they seemed; to go
far beyond the normal call of experimental duty and test unlikely
possibilities; to be 'cool' under pressure and not to get anta-
gonistic towards any of his critics; and, finally, to make no strong
claims about the theoretical consequences of his result - claims
which might upset the astrophysicists. The astrophysicists on
their part, have, as mentioned above, not done anything publicly

to discredit the experiment. That is to say, although they have

had reservations they have not, in general, taken any action which

might cast doubt upon Davis's credibility as an experimenter. In

short, the relationship entailed in the partnership pre-1967 has

not been broken. As Davis himself puts it:

> 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....

The claim of the argument here is that it was the pre-existing

interests, investments of credibility and social relationships

which were important for the success of the experiment. If these

interests, investments and relationships had been different then

Davis's experiment would perhaps not be treated as seriously as

it has been. In a way the large investments of resources (in the

shape of expertise, time and money - i.e.,credibility) in Davis's

experiment virtually guaranteed that the result would be taken

seriously. This, however, does not mean that after 1967, what-

ever action Davis took did not matter. Because he found a result

which was threatening to the credibility of the theorists he had

to fight hard to maintain his own credibility.

Davis's strategy of carrying out a programme of experimental

tests of his apparatus - tests which he largely considered to be

a waste of time in terms of his narrow experimental goals - illustrates

again the importance of ritualised rationality in science (see

Wynne, 1976 and Collins, 1981b). It was not enough for Davis simply

to present his experimental results, he had to work long and hard

to convince the theorists of his claims. A result, such as that of

Davis, which threatens to overthrow previous investments of

credibility, is clearly much harder to get accepted than a result

which everyone expects. As an extreme example, it was found in the

parapsychology study (Collins and Pinch, 1979, 1982) that the most

trivial experiments were unquestionably and immediately accepted

when they showed the paranormal did not exist. Such results,

of course, fitted in with the previous credibility structure.

Similarly, if Davis had found the result which the theorists had

predicted, we can assume that his result would have been accepted in

1968 and he would not have had to embark upon his lengthy programme

of experimental tests and refinements.

The solar-neutrino case is particularly interesting in terms

of models of the social construction of scientific knowledge because

it seems to present a puzzle. On the face of it, we would not

expect that a result which caused so many theoretical problems

would ever be accepted. Certainly, this is the reading to be

got from Pickering's (1980) study. Although Davis's result might

still lose credibility, and, as we have seen, it took a long time

to win acceptance, it has, nevertheless, done remarkably well.

The explanation for the success which has been given (i.e., the

importance of the partnership with the Caltech theorists) is thus

of some interest, generally, for it is an explanation of how it

is possible for radical innovations to occur in science. We are

badly in need of such explanations. Of course, Davis's achievement

may prove not to be particularly radical, as the consequences of

his result may not be particularly great. However, it does seem that

the first stage of any successful radical innovation in science must

involve getting a theoretically-unexpected result accepted even if

that result is then ignored.

In view of the complex of interests which are possible in science

we would be wise not to claim too much for this case.   It does

seem though, that if an experimenter finds a result which conflicts

with theory he stands a better chance of getting it accepted if

he has already established the backing of a powerful group of theorists.

However, it is quite possible that, even without such backing, an

experimenter might be successful. Certainly the ability to test a

theory can, in its own right, generate new interests, as Harvey (1981)

has shown. We do not yet know enough about the hierarchy of interests

in modern physics to conclude which interests are the predominant

ones. The above case is suggestive, but studies of other cases are

needed.   In particular, we need to look at cases where an experimenter

produces a theoretically unexpected result and fails to get it

accepted. Perhaps the pre-existing interests, investments and

relationships will be shown to be important there too.[19]

The Social Construction of Theoretical Knowledge

There are two aspects of the social construction of theory

to be considered.   Firstly, there is the social construction of

the theory itself. Secondly, there is the social construction

of the theoretical prediction. Each aspect will be discussed in

turn.   Again, the interest-credibility model will be shown to

be fruitful.

The social construction of the theory is in one sense a non-

issue since we do not yet know what the explanation for Davis's

results will be (we do not even know if a special explanation

will be needed). However, Davis's results have produced a phenomenon

which warrants some sociological explanation. Despite his results,

astrophysics, nuclear physics and neutrino physics go on much as

before. Nobody has, for instance, given up stellar-evolution theory

because of the solar-neutrino problem.   The standard theory and concomitant physical assumptions still hold to all intents and purposes.

The interest-credibility model provides a straightforward account of this state of affairs.   In view of the interests and investments made and still being made in the standard theory - investments which bring constant returns - there is no reason to give up. If the standard theory had reached an impasse and did not provide any new areas of experimental and theoretical practice, or puzzles could not routinely be solved, then there might be reason to abandon the theory. At the moment all the astrophysicists I spoke with could happily earn their'bread and butter'scientific credibility by using the standard theory.

Ironically, Davis's results have actually enabled most respondents to boost their credibility. By exploring non-standard solutions but not placing any belief in them (see Chapter 9), respondents have furthered their credibility by producing yet another scientific publication.

In   view of the interests associated with the standard theory it seems unlikely, at the moment, that Davis's results will lead to theoretical change. However, we would be foolish to predict that this will always be the case. New interests could easily arise which would make such an upheaval possible.

The above scenario bears close parallels with Kuhn's (1971) account of how anomalies (findings which do not fit in with a current paradigm) are treated.   Such anomalies are to all intents and purposes ignored. The interest-credibility explanation does take us a little beyond Kuhn, however, for it gives an explanation of why anomalous

results do not overthrow standard theory (i.e., scientists have no

need to invest in them). Also, we have seen that the Kuhnian scenario

is not quite correct - at least in this case. The veritable

theoretical industry which the result generated (i.e.,the large

number of papers offering explanations) is not expected in Kuhn's

account. The interest-credibility model can account for this

phenomenon in terms of scientists being instrumentally orientated

and thus being happy to publish a paper which they do not really

believe in. In the Kuhnian paradigm model it is difficult to see

why so many theoretical explanations should be attempted.

The social construction of the theoretical prediction can also

be understood in terms of the interest-credibility model. Indeed,

the separation of prediction from the theory draws attention to the

suitability of such an explanation. A prediction, unlike the theory,

is produced in a particular context for particular purposes. The

argument to be presented here is that the solar-neutrino predictions

expressed the over-riding interest of the nuclear astrophysicists

in getting a solar-neutrino experiment performed in order that they

might test their theories. Before showing how this interest

was manifest, let us first ask what it means to say that the

theoretical prediction is socially constructed.

It has been pointed out several times already that the pre-

dicted neutrino flux at any time is dependent upon a number of other

experimental and theoretical inputs. Such inputs are, of course,

themselves socially constructed. In view of the prediction being

derived from these socially constructed elements, cannot we then

say that this shows that the prediction itself can be seen as a

social construct? We can say this - but this is not a very interesting

thing to say, For,although all the inputs are socially constructed,
once consensus has emerged as to what each value is and how each
sub-theory should be constructed, then the inputs are fixed. It is
as if Bahcall was the perfect computer who was fed all the data
and theories, ran the programme, and gave the one possible numerical
answer. In this picture there certainly seems to be little room for
interests to have any effect. Bahcall, however, as we have seen,
was not a computer! He was a young scientist who worked long and
hard on the predictions and who fervently campaigned to get Davis's
experiment supported. And, when Davis got his low result it was
Bahcall, more than anyone, who had to face the consequences.
Furthermore, as we have also seen, the input data upon which the
prediction was based were not in themselves fixed for all time.
Values changed and often the data (i.e., S-factors for cross-sections)
had to be interpreted before they were fed into the prediction.
Also, there was interpretative flexibility in how the best prediction
was to be derived from the model calculations (i.e.,disagreement
over averaging techniques). In view of this, the production of a
solar-neutrino prediction can be described as a process in which
it is possible to exercise interpretative licence. In other words,
the predicted flux at any time is not necessarily an immutable
number 'forced' upon the theoretician by the natural world.

If there is such licence for the theoretician making the pre-
diction is it possible that Bahcall's interests and those of his
colleagues in nuclear astrophysics could have had some effect on the
generation of the prediction at any one time? It was claimed above
that the nuclear astrophysicists could be identified as having an
interest in getting a solar-neutrino experiment performed and
thereby testing their theories. How might such an interest affect

the prediction? We know that, in order to convince Goldhaber and others of the merits of such an experiment, a sizeable theoretical prediction was needed. In view of this, it seems possible that the theoreticians' 'interest' in getting the experiment performed could have led them to exercise interpretative license and produce a large prediction. That this may have been the case for Bahcall's 1964 prediction was argued in the latter part of Chapter 4.

Let us extend the argument by asking how the successful funding of the experiment in 1964 would be likely to affect the prediction. Clearly, the 'interest' in getting the experiment funded is no longer operative. However, it will be recalled that the overall purpose was to test the theory. I suggest that this aspect, of testing the theory and hence ensuring the accuracy of the prediction, became the predominant theoretical interest after 1964. This interest was manifest in Bahcall's and others' activity in measuring parameters much more carefully (S_{17} and S_{33}) than before; their concern over the uncertainty in Z; and a general emphasis on possible uncertainties (see Chapter 6).

Finally, we need to ask how the appearance of Davis's results in 1967 affected the prediction. I suggest that with the appearance of Davis's results the whole basis of the partnership between him and the nuclear astrophysicists was put in jeopardy. After all, the basis of the partnership was that each group should be able to produce credible information for the other. In particular, it now seemed that Bahcall's reputation for producing credible information was at stake (see Chapter 8, Part I). I suggest, that, in order to try and save some of his credibility, Bahcall immediately started to look for ways to lessen the impact of Davis's results. By again exercising interpretative license, he was able to produce lower

predictions and also emphasise the consistency of theory and experiment (see below). It is his attempt to maintain credibility that accounts for the dramatic fall in the prediction around about 1967-8 (see Chapter 8, Part I and Fig. 1.1).

One last aspect of the social construction of the theoretical prediction should be mentioned. The recent increase in the prediction (see Fig. 1.1) has happened just at the time when the arguments over the funding of second-generation experiments has occurred. As was pointed out in Chapter 8, Part II, a clear contradiction between the prediction and the experiment was one of the main planks of Bahcall's argument for the funding of new experiments. In view of this, it could be said that interpretative license was once more being exercised in order to 'push' the predictions upwards and justify the new experiments.

Several caveats must be made about the above account. Firstly, and most importantly, the argument is not completely watertight. As emphasised in the section on the social deconstruction of the theoretical prediction, restrictions on data and on the author's competence at making numerical computations has meant that the recovery of the full interpretative flexibility possible in the theoretical prediction for all predictions considered has not been possible. This means we do not know exactly what the interpretative license is at any one point. All that can be suggested is that there are some occasions when it is possible to exercise interpretative license. This means that we can merely point to possible trends (upwards or downwards) in the prediction and changes in emphasis in its general certainty. Questions such as,'Why did Bahcall not make an even larger prediction in 1964?' and 'Why did he not actually bring about agreement between theory and experiment in 1967, rather

than near agreement?' cannot be answered. It just is not possible to say what the quantitative constraints on the interpretative license (in terms of numerical values of the prediction) were.

Another point to be emphasised is that no implications of deceit or anything similarly disreputable is implied by the above argument. Indeed, the nuclear astrophysicists interest in bringing about a crucial test of their theory would be regarded in some circles (Popperian) as action of the highest scientific merit. Certainly the exercise of interpretative license is part and parcel of all scientific activity when it is viewed as social activity. It should not be misconstrued as a psychological trait or something peculiar to Bahcall or to theorists in general.

One last point to be made concerns the detailed evidence for the argument. As stressed at the end of Chapter 4, the evidence often leaves much to be desired. I have tried, where possible, to use evidence from correspondence files rather than interviews since it seems that this particular topic is peculiarly sensitive to the distortions which retrospective interview data can produce. Thus, looking back with hindsight, it is easy to say that Bahcall's 1964 prediction must have been exaggerated since the prediction has since come down. In other words, previous science is judged by today's science, and we know the problems this can raise. However, it was striking how many respondents drew my attention to the significance of the phenomenon of the decrease in the theoretical prediction over the years. Several explicitly made the point that pre-1964 theorists had pushed the prediction up to get the experiment funded and that post-1967 they had lowered the prediction to try and save their embarrassment.[20] One scientist made this point as part

of a vitriolic comment on the field as a whole, but most scientists

commented on it in the spirit of 'Here is something that will

interest you: isn't that a bit of a laugh?' That so many

respondents viewed the developments in this way, is a source of

encouragement that the argument here is essentially correct.

If we were talking, in this section, about only one prediction,

I do not think the argument would be convincing. However, to see

all the changes in the predictions correlating with the interests

over such a long period does seem to be very suggestive as to the

social construction of the theoretical prediction.

The Social Construction of Consistency and Contradiction

In this section, I want to show very briefly how the interest-

credibility model might account for the differing views of consistency

and contradiction encountered. In many ways this section overlaps

with, and is an extension of, the preceding one.

As was pointed out above, Bahcall's emphasis on consistency

between theory and experiment in 1967-8 could be understood as part

of his efforts to maintain his credibility within the partnership.

Bahcall argued that there was no contradiction between theory and

experiment because the appearance of a contradiction might seriously

jeopardise his credibility. Although Bahcall's views were challenged

by Iben (see below), he was largely able to maintain his credibility.

Indeed, his award, a few years later, of the Warner Prize, and a

Chair at the Institute for Advanced Study, would suggest that his

credibility had, if anything, increased.

As we saw, in Chapter 8, Part I, within a few years Bahcall

was himself arguing that there was a contradiction. Bahcall's sub-

sequent emphasis on the 'contradiction viewpoint' is perhaps not

unconnected with his long-term interest in getting solar-neutrino

experiments performed (in this case, second-generation experiments).

These experiments will undoubtedly provide further opportunities

for him to enhance his credibility. Thus it would seem that the

social construction of contradiction can also be explained by the

interest-credibility model.

Although the detailed evidence for the above argument is to

be found in Chapter 8 and Pinch (1980a)(and many of the reservations

of the previous section apply here also), I think enough has been

said to demonstrate the fruitfulness of the interest-credibility model

as a means of explaining different views of contradiction and

consistency.

The Social Destruction of Knowledge

In this final section, I would like briefly to look at the other

side of the coin to social construction - social destruction. Social

destruction is in principle similar to social construction. The only

difference is that we have to explain the social construction of

failure rather than success.

The first case of failure I would like to look at is the only

case of overt controversy encountered in the solar-neutrino field.

This was Iben's attempt, in 1967, to argue that Davis's results

were in conflict with Bahcall's prediction (see Chapter 8, Part I).

Iben, as we saw in Chapter 4, had crossed swords with Bahcall

back in 1963. He had been reluctant to let Bahcall use his stellar-

evolution programme for work on the Sun - a problem Iben considered

to be of little interest in contrast to the advanced stages of

stellar evolution. After Iben left Caltech for MIT, in 1964, he

had had very little to do with solar neutrinos. His work on Be^7-

capture, mentioned in Chapter 6, arose almost by accident. He had

set the calculation as a problem for a class and, in the process of making the calculation, discovered the significance of bound-electron capture. Iben (interview material) told me that he was amused to make this discovery because it slightly reduced the predicted flux whilst he felt all Bahcall's work tended to push the flux upwards.

When, in 1967, Iben saw Bahcall going back (as he regarded it) on his prediction, he saw an opportunity to make a speedy investment. As a stellar-model specialist, it would not involve him with too much extra work to run some models which could be used for solar-neutrino flux predictions. Iben, unlike Bahcall, had little credibility staked in the earlier predictions. He had only played a small part in the Caltech calculation of 1963 and, as we saw, he played that part reluctantly. Thus, Iben had little to lose by finding a contradiction between prediction and experiment, and there was a real chance he could make a credible contribution since he could point to the need to consider more drastic theoretical options than Bahcall had yet considered.

As was pointed out in Chapter 8, Iben's work has had very little impact. This was partly because Bahcall was one of the referees for his papers and was able to hold up their publication . By 1967, the field of solar-neutrino theory was largely dominated by the Caltech group with most of the significant developments coming from either Fowler or Bahcall.[21] This intellectual domination was inseparable from institutional domination and it was very likely, for instance, that journal editors would send papers on solar neutrinos to someone at the Caltech group to referee. This institutional dominance no doubt played a part in the difficulties which Iben faced in trying to challenge Bahcall's views.

Iben's own decision not to pursue the battle further, in effect

meant that the controversy did not become a full-blooded confront-

ation, and that Bahcall was able to continue his dominance over the

theoretical work. Bahcall's own switch to the contradiction position

shortly afterwards made any further pursuit of the controversy rather

pointless, anyway. However, Bahcall's eventual agreement with Iben

has not meant that Iben has been vindicated. Most respondents seemed

genuinely puzzled as to what the controversy had been about and

certainly no-one perceived Bahcall's changing views as a volte-face.

What the Bahcall-Iben confrontation draws attention to, is the

important point that institutional resources in science accompany

accumulations of credibility. For instance, institutional affiliation,

and control of information (via refereeing) are both part of

the territory over which the battle for credibility is fought.

Bahcall's strong institutional position which accompanied his

investment strategy, meant that institutional resources

such as refereeing, worked to his favour. In other words, positions on

matters of science and institutional positions go hand

in glove. Iben's weaker position in regard to

institutional resources perhaps accounts for his failure to make

much impact.

There are two other episodes of social destruction encountered

that warrant discussion. Although these two cases involve substantively

very different scientific issues, the social processes of rejection

are essentially similar. I refer to the rejection of the work of

Jacobs (Chapter 7, Part II) and of Rouse (Chapter 9). Both scientists

managed to get their views into the literature (after some difficulty),

however, their work caused very little stir. They met the fate of

'implicit rejection' (Collins and Pinch, 1979). That is to say, no
great effort was mounted to show why they were wrong. Their views
were just allowed quietly to pass unnoticed.

In order to try and understand the reception (or rather the
lack of it) of the ideas of Jacobs and Rouse, we should first con-
sider the type of idea they were putting forward. Essentially both
scientists were being negative - they were disagreeing with what
other scientists did. Jacobs claimed Davis's experiment was wrong
and Rouse claimed that everyone who did stellar-evolution calcula-
tions did them incorrectly.   The problem posed by the negative
thrust of their work .is that it is difficult to see what other
scientists could do with it. Davis was already testing the
possibility that Jacobs suggested; short of becoming chemists
themselves there seemed to be little Jacobs's colleagues could do
in terms of active experimental or theoretical work. Similarly
Rouse's attacks on the basic methods of stellar-evolution theory
offered no active research programme since most people considered
Rouse's goals of deriving both the radius and the luminosity in
such calculations to be hopeless. Furthermore, both Jacobs and
Rouse were attempting to undermine a vast amount of work. If Jacobs
was correct then all the effort gone into the theoretical interpre-
tation of Davis's result, not to mention Davis's experimental
programme, would have been futile. Similarly, the whole industry
of stellar-evolution was challenged by Rouse's claims. In short,
a lot of pre-existing interests and investments of credibility
were at stake.

In terms of the interest-credibility model, it could be said
that, as well as challenging the credibility of many scientists,
Jacobs and Rouse were not offering any opportunity for scientists

to make investments in <u>their</u> ideas and hence obtain credibility

by that route. After all, there is very little credibility to

be obtained from publishing a paper agreeing that 'so and so has

got it wrong'. In this sense then, implicit rejection can be seen

to be most likely to occur when there is a failure to produce

credible information (that is information which can be used by

anyone else).

Implicit rejection is a very effective way of rejecting

knowledge claims. The danger that is run in explicitly attacking

anyone is that, by attacking them, it is also being said that they

are important enough to be attacked. Total silence is a far

more effective strategy of making certain a person's work is not

taken up. And, after all, what matters ultimately is, not saying

publicly that 'so and so has got it wrong', but rather that no-one

should invest in the 'wrong' ideas.

Of course implicit rejection does not always work; eventually

it might have to be shown explicitly what is wrong. However, it has

a much better chance when the proponent, whose work is being rejected,

is in an institutionally weak position. In view of the dominance

of the field by institutionally powerful groups such as the Caltech

nuclear astrophysicists, and in view of the institutionally weak

position of both Jacobs and Rouse (as outlined in Chapters 7 and 9),

it seems unlikely that Jacobs and Rouse could attract much attention

and thus they could be safely ignored.   The difficulty of challenging

the dominant interests and investments in the solar-neutrino field

should not be underestimated.   After all, if Iben, who was a full

professor at MIT and an acknowledged expert in stellar-evolution

theory, could not successfully mount such a challenge then we can

see what little chance Jacobs and Rouse had.

In summary then it can be said that the interest-credibility

model sheds light on some of the social processes of the rejection

of knowledge claims. In particular, it draws attention to the

importance of institutional resources which accompany investments

of credibility and it also provides a means of understanding 'implicit

rejection'.

Some Reservations Concerning Interest-Credibility Models and the Social Construction of Scientific Knowledge

Clearly whether or not it is thought that the interest-credibility

explanation offers anything to increase our understanding of the

above episodes depends on the detailed material presented in earlier

chapters. It seems to me that it is a useful way of making sense

of the developments in solar-neutrino astronomy over a twenty-year

period. The reason why interest-type and investment-type explanations

are so fruitful in the context of the present work is because they

take as their target the detailed internal workings of science

and furthermore they show the workings over a period of time. Invest-

ments take time to reach fruition and interests take time to form.

It is these sorts of explanations of detailed scientific activity

over a (not necessarily lengthy) period of time which are most

needed if the second stage of the relativist programme is to be

carried through.

The interest-credibility model, like all sociological models,

is not immune from criticism. One argument made against this type

of model is that it is not capable of explaining how scientific

revolutions occur since there seems to be no obvious reason why

scientists should ever, en masse, overthrow their previous investments

of credibility.[22] This criticism is perhaps not so germane in the

context of the present work where the episodes described are not of
the revolutionary type. However, it seems that, in principle, the
model could describe why revolutions occur. Perhaps, for instance,
the narrow technical interests described in this work are replaced
by wider social interests at a time of revolution and these lead
to the previous investments being overthrown. Another criticism
levelled at this type of model is that it tends to do too much -
that is, an interest and an investment are found to account for
everything.[23] This, however, need not always be so. Because in-
terest-credibility explanations apply to local contexts they depend
very heavily on detailed empirical studies for their reference.
Indeed, they often appear to be descriptive vehicles for re-describing
empirical episodes. But, as more and more empirical, and in particular
comparative, studies are carried out, it should be possible to
develop some notion of a hierarchy of interests. When this is
carried out, interest-credibility explanations should lose some of
their present 'unfalsifiable' character. Even without a hierarchy
of interests the interest-credibility model is valuable as a des-
criptive device. It draws attention to particular processes and
activities in science and gives a means whereby comparisons can at
least be made.

Afterword

Throughout this work there has been a tension between the
concerns of history and those of sociology. This tension appears
not only in the domain of aims and methods (as discussed in Chapter
1) but also as a matter of style. The historical style tends, on
the whole, to be narrative, as opposed to the more analytical style
favoured by sociologists. No claim is made here to have resolved

such tensions. However, it is hoped that, by a careful delineation of the aims and methods of the work, and, in particular, the separation of the two tasks of social deconstruction and social construction at least these particular tensions have not been left implicit. Unfortunately very little can be done about the tension in style. It is inevitable that some parts of this work which historians find intriguing may seem tedious to the sociologists and vice versa.

If the work has been successful then it should have illuminated our understanding of science as a social phenomenon. At the very least it should have provided a detailed account of how theory and experiment can interact together in one historical instance. Whether this study is 'just another (perhaps special) case' of science as a social phenomenon or whether it has wider implications depends very much on which of the tasks of social deconstruction and social construction is being considered. The social deconstruction of facts, theories and predictions is, by its nature, a universal phenomenon and this case is another illustration of the phenomenon. However, I would argue that it is not 'just another case' since all attempts to deconstruct knowledge within the modern physical sciences play a strategic role in the sociology of knowledge as a whole. Short of mathematics and perhaps logic itself, such cases are notoriously the hardest in which to show the fruitfulness of the sociology of knowledge. No -one is surprised that parapsychological knowledge can be deconstructed but they do tend to notice when it is modern physics which is being dealt with.

The difficulty of deconstructing scientific knowledge in a prestige area like modern physics should not be underestimated.

This difficulty is exacerbated when the scientists whose knowledge

is being deconstructed are generally regarded to be amongst the

world's best physicists.   The deconstruction of Davis's and Bahcall's

claims would not be possible without the generous help they have

provided me throughout the study.   It might seem perverse, therefore,

to claim that their work can be understood as a social phenomenon.

However, the reading of 'social' as being 'somehow less than solid'

is a misreading.   The veracity of Davis's claims and Bahcall's

claims are not diminished one iota by the arguments presented here.

Both Davis's and Bahcall's work have produced correct results in the

most profound sense.

The findings on the social construction of facts, theories and

predictions may, have a more limited relevance than the findings on

social deconstruction.   The lengthy and involved interaction of the

theorists and the experimenter which is the main characteristic of

the field may well be atypical.   I certainly do not know of any

other case like it. This means it is possible that the detailed

mechanisms and social processes operating in this case, may only

apply to this case.   However, by attempting to explain such

processes in terms of a general model - the interest-credibility

model - the relevance to other cases should be more apparent.

Indeed, as pointed out, there are several cases already where

similar ideas have been shown to bear fruit.   That having been said,

it seems imperative for more studies to be carried out which directly

compare the present findings with other cases where there is conflict

between experiments and theory.   Such comparisons are underway![24]

NOTES FOR CHAPTER TEN

1. For instance, Bahcall's calculation of the neutrino absorption
    cross-sections seems a promising case to take.

2. Thus, in Pinch (1980a) the main data are quotes drawn from interviews.

3. See, Brown (1965), and L. Festinger, H.W. Riechen and S.C.
    Schachter, When Prophecy Fails, New York: Harper Row, 1956.

4. MacKenzie did much more than this. As will become apparent
    in the section on the social construction of knowledge, he also
    identified differing cognitive and social interests associated
    with Yule's and Pearson's work.

5. The arguments are not even framed in terms of standard
    statistical theory - I am grateful to Donald MacKenzie for
    drawing my attention to this point.

6. The section of the paper which deals with uncertainties in the
    radiochemistry can be regarded as bolstering the arguments
    already presented on the social deconstruction of the experiment.
    It should be noted that the aim of the (1981a) paper was to
    deconstruct knowledge; hence the emerging consensus over
    Davis's experiment was not important to the aims of that paper.

7. This is assuming the scientist has decided he/she knows enough
    to reach firm conclusions. It is always possible that he/she
    can say 'we just don't know yet'.

8. Perhaps it is even possible that 'the moon is made of green
    cheese'. This example was raised by Barry Barnes at the
    conference, 'New Perspectives in the History and Sociology of
    Scientific Knowledge', Bath University, 27-29 March,1980.
    Barnes argued that it was patently ridiculous for the
    sociologist to take such a belief as this seriously. This
    is consistent with his views (e.g., Barnes, 1974) concerning
    the 'normalcy' of belief. However, Collins and Cox (1976, 1977)
    have pointed out that in some extreme cases, whether or not
    such deviant beliefs are treated seriously leads to very
    different kinds of analysis being pursued (see also, Law, 1977).
    In this case, if we treated the belief that the Sun is made
    of black holes as a patently false scientific belief then we
    would have been led to a very different type of analysis.
    By extension, in Rouse's case, we would have had to treat him
    in terms of the sociology (or more probably, psychology) of
    delusion. However, perhaps Barnes's point is that the 'moon
    is made of green cheese' is just not likely to be encountered
    as a serious belief put forward by any scientist. This
    obviously is true, but there is an important point of
    principle at stake here. If some beliefs cannot be entertained
    because they are not put forward by scientists, this raises
    problems with all hypothetical arguments put forward by
    sociologists to try and deconstruct knowledge, (e.g., my
    hypothetical argument concerning Jacob's possible response to
    the Cl^{36} test in Chapter 7). Barnes would presumably have to say
    that these arguments too, are ridiculous.

9.   See W. Heisenberg, <u>Physics and Philosophy</u>, London: George
      Allen and Unwin, 1958. For an attempt to describe the view
      of reality implied by quantum theory see Collins and Pinch
      (1982) - especially Chapter 4.

10.   MacKenzie (1978: 48) defines 'cognitive interests' as referring
      to 'those aspects of the actual or potential application of
      theories which "feed back" into theoretical development by
      structuring scientists' construction and judgement of theories'.
      It seems that this is a subset of the type of narrow interests
      which Barnes and MacKenzie identify as instrumental interests
      'internal to science'.

      It is unfortunate that the label 'social' is now used to refer
      to interests arising from the wider social-political sphere.
      The narrow 'cognitive interests' are equally social,(as
      Barnes and MacKenzie, 1979, point out), in that they are
      embodied in the practices of social actors.

11.   Two other studies in which interests form the explanatory
      vehicle are: D. MacKenzie and B. Barnes, 'Scientific Judgement:
      The Biometry-Mendelism Controversy' in Barnes and Shapin
      (1979: 191-210), and J. Dean, 'Controversy over Classification:
      A Case study from the History of Botany', in Barnes and
      Shapin (1979:211-30).

12.   A sizeable literature seems to be developing on 'interest
      explanations' . See for example, Woolgar (1981); B. Barnes,
      'On the 'Hows' and 'Whys' of Cultural Change', <u>Social Studies
      of Science</u>, 11, 1981, 481-97; D. MacKenzie, 'Interests,
      Positivism and History', <u>Social Studies of Science</u>, 11,1981
      498-503; and, S. Woolgar, 'Critique and Criticism: Two
      Readings of Ethnomethodology' <u>Social Studies of Science,</u> 11,
      1981, 504-14.

      The exchanges between Barnes, MacKenzie and Woolgar have
      occurred too recently for me to include any extended discussion
      here. Many of the issues, as Barnes points out, are ultimately
      standard philosophy of social science issues relating to
      explanations. It is assumed here that, like scientists, we
      can pursue our explanations without constantly having to look
      over our shoulders to see what philosophers (and ethnomethodo-
      logists) make of the work.

13.   However, as Shapin (1979) points out, the expunging of social
      interests from the work of the professional anatomists might,
      in itself, point to a powerful social interest predominant
      in all professionalised science.

14.   For similar work, see K. Knorr, 'Producing and Reproducing
      Knowledge: 'Descriptive or Constructive', <u>Social Science
      Information</u>, 16, 1977, 669-96.

15.   For an attempt to apply Bourdieu's ideas see Pinch (1977).

16. This conception of interest seems to overcome some of the difficulties over whether or not interests are psychological attributes (see the debate between Woolgar, Barnes and MacKenzie referenced in note 12). In this view, interests are to be seen to be part and parcel of scientists's activity.

17. The connection between interests and investments, as already mentioned, is maintained in Pickering's (1980) study. Also, the connection seems to be implicit in Harvey's (1981) 'plausibility model' (see Chapter 1). It seems to me that the dynamic notion of interests, as arising with scientific practice, is consistent with Harvey's notion of plausibility. For instance, in the timing-hypothesis case plausibility increased once it was clear that the hypothesis could generate scientific practice (i.e., Aspect's experiment). In other words, an interest developed in the timing hypothesis. Harvey also draws attention to the importance of 'gaining access to equipment and funds' (ibid: 104). What is not clear in Harvey's work (and in Pickering's) is exactly how investments, equipment and funds, connect with interests and plausibility. I suggest that the interest-credibility model makes this clearer.

18. For example, Bahcall referred to 'his emotions' depending in large part on his having got the prediction correct (see p.202).

19. Pickering's (1981b) study of quark detection is very suggestive and seems to indicate the importance of theoretical interests. However, the sociological conclusions are not drawn out. Harvey's (1980, 1981) study can be interpreted as showing the difficulty of establishing an experimental result, the implications of which overthrow interests and investments in dominant theory. Collins's (1975, 1981b) studies of the Weber affair are not really suitable for comparison because Collins did not look at the role of the theorists.

20. One or two respondents went on to point out that this was unlikely because of the sudden increase in the prediction in 1970 (see Fig. 1.1), stimulated by Watson's work on the opacity. Of course, by this point Bahcall was switching allegiance to the contradiction viewpoint anyway, so the 'interest' in a lower flux was not as strong. Also, it is amusing to note that this increase later went away (in 1973) when it was discovered that Watson had made a mistake in his earlier opacity computations.

21. The role played by the Caltech group in the field was indicated by my interview sample. Of 17 nuclear astrophysicists interviewed 14 had at some stage in their careers worked with Fowler at Caltech.

22. This argument originates with H.M. Collins in conversation.

23. Ibid!

24. T.J. Pinch. 'Cognitive Consensus: A Comparative Study', This is an SSRC-funded project which compares several cases of recent experimental results in physics which clash with theory. One case is the acceptance of Davis's results.

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APPENDIX I

The Data for the Study

As in any piece of research in the sociology of scientific knowledge, before fieldwork could be undertaken it was first necessary to become familiar with the technical knowledge culture studied. In this case most of the relevant technical literature (including semi-popular articles) was collected and read before interviewing commenced. The sample of scientists selected for interview was based on the reading of this literature. All scientists who had made significant contributions to the field (both experimentally and theoretically), and who were accessible within the logistics of the project were included in the sample. In addition, a number of scientists who had been only peripherally involved (such as those who had suggested, in passing, a theoretical solution to the solar-neutrino problem) were included. The sample was extended after fieldwork commenced as my attention was drawn to other potential respondents (snowball sampling). This led to the inclusion within the sample of officials who had been responsible for the funding of Davis's experiment. Interviews with the following respondents were tape recorded:

L. Alvarez. University of California, Berkeley, November 15, 1978.

D. Arnett, University of Chicago, November 8, 1978.

J. Bahcall, Institute for Advanced Study, Princeton , October 20,21, and December 4, 1978.

C. Barnes, California Institute of Technology, November 20, 1978.

J. Barnothy, Evanstown, Chicago, November 8, 1978.

H. Bethe, Cornell University, October 27, 1978.

D. Clayton, Rice University, Houston, November 29, 1978.

R. Davis, Brookhaven National Laboratory, October 23,24, December 6, 1978.

P. Demarque, Yale University, October 19, 1978.

R. Dodson, Brookhaven National Laboratory, October 23, 1978.

M. Dwarakanath, Bell Labs., New Jersey, December 5, 1978.

W. Fowler, California Institute of Technology, November 21, 1978.

M. Freedman, Argonne National Laboratory, November 9, 1978.

M. Goldhaber, Brookhaven National Laboratory, October 23, 1978.

D. Gough, Institute of Theoretical Astronomy, Cambridge, November 15,1979.

H. Hill, University of Arizona, Tucson, November 23, 1978.

I. Iben, University of Illinois, Urbana, November 6, 1978.

K. Jacobs, Bell Labs., New Jersey, December 5, 1978.

K. Lande, University of Pennsylvania, Philadelphia, November 31, 1978.

S. Lubow, University of California, Los Angeles, November 22, 1978.

Leona Marshall-Libby, University of California, Los Angeles,November 22,1978.

E. Parker, University of Chicago, November 8, 1978.

A. Poskanzer, University of California, Berkeley, November 15, 1978.

H. Primakoff, University of Pennsylvania, Philadelphia, November 31,1978.

R. Raghavan, Bell Labs., New Jersey, December 5, 1978.

F. Reines, University of California, Irvine, November 21, 1978.

R. Rood, University of Virginia, Charlottesville, November 29, 1978.

C. Rouse, La Jolla, San Diego, November 18, 1978.

E. Salpeter, Cornell University, October 27, 1978.

R. Sears, University of Michigan, Ann Arbor, October 30, 1978.

M. Schwarzschild, University of Princeton, December 4, 1978.

W.R. Sheldon, University of Southampton, May 14, 1979.

E. Spiegel, New York, October 26, 1978.

T. Tombrello, California Institute of Technology, November 20, 1978.

R. Ulrich, University of California, Los Angeles, November 22, 1978.

W.D. Watson, University of Illinois, Urbana, November 6, 1978.

W. Whaling, California Institute of Technology, November 20, 1978.

C. Wheeler, University of Colorado, Boulder, November 13, 1978.

A. Wolfendale, The Royal Society, London, October 10, 1980.

A typical interview would last for two hours. However, some interviews lasted much longer and were continued over lunch and, on some occasions, well into the evening.   All the above interviews were transcribed in full. Quotations by the above respondents which appear in the text are drawn verbatim from these transcripts.

In addition, several unrecorded interviews took place. These were with the following respondents:

R. Kavanagh, California Institute of Technology, November 17, 1978.

J. Pomeroy, interview conducted by telephone,December 7, 1978.

G. Ragosa, Department of Energy, Washington, November 30, 1978.

Virginia Trimble, interview conducted by telephone, November 14, 1979.

A. Van Dyken, interview conducted by telephone, November 30, 1978.

In the case of all these non-recorded interviews copious notes were taken.

Kavanagh was the only respondent who refused to be tape recorded.

Apart from the above formal interviews, many informal conversations with respondents took place during the course of the research. In particular, I had very helpful informal interaction with Davis and his group and with John Bahcall.   Often this informal interaction was continued over a period of several days.   The extent to which I became immersed in the field can be judged from the fact that on one occasion I was trusted to act as a courier for a tape of data which I carried from the Homestake gold mine (where Davis's experiment is located) back to the East Coast.

These informal interactions have helped shape many of the ideas presented in the study.   In general, respondents were very forthcoming in interview. However, there is evidence that when asked which field was responsible for the solar-neutrino discrepancy,

respondents were cautious because of the 'public inquisitor' role which I was perceived to have in connection with this topic (see Pinch, 1981a in Appendix II).

An additional source of data was my correspondence with respondents. I would like, in particular, to acknowledge correspondence with:

J. Bahcall; R. Davis; R. Dodson; T. Jenkins (Case Institute of Technology); R. Rood; C. Rouse; D. Schramm (University of Chicago), R. Stothers (NASA Goddard, New York); C. Wheeler.

Correspondence amongst respondents formed another important source of data. I was given access to the complete solar-neutrino correspondence files of the following scientists:

J. Bahcall; R. Davis; W. Fowler; C. Rouse.

From these files about 500 items were copied. Many of the letters were circulated amongst the various participants, thus copies of letters sent from Davis to Bahcall were found in Fowler's files. In addition, letters between other scientists of particular relevance were often to be found in the files of the above scientists (particularly Bahcall's and Davis's files). Many other scientists also gave me pieces of correspondence relevant to the study.

One, particularly useful, source of data has been the Brookhaven National Laboratory, Chemistry Department file on solar neutrinos. I was given access to this file by the kindness of R. Davis and R. Dodson. The existence of this file was due to Dodson's foresight. The file had a note in it written by Dodson which said: 'Save for History of Science'. Another useful source of correspondence was the solar-neutrino file housed at the Department of Energy. I was given access to this file by G. Ragosa. For security reasons I was not allowed to copy this file, but I was allowed

to make detailed notes on the correspondence it contained.

Apart from correspondence, I was given copies of other pieces of printed matter. These included: referees' reports, funding proposals, conférence presentations, early drafts of papers and other unpublished material. Another important source of data has been the proceedings of two informal conferences held on solar neutrinos (Reines and Trimble, 1972, and Friedlander, 1978a and b). These conference presentations, although 'informal' are no substitute for interview data, since they are still public occasions. Also, these conference proceedings are sanitised. Often, contentious points are edited out and actual remarks re-written by participants when they see the transcripts of what they said.

One final important source of data has been the recent article by Bahcall and Davis (1980) where they themselves trace the history of the field.

APPENDIX II

● ABSTRACT

This paper examines a debate over the certainty of scientific knowledge which has arisen in solar neutrino science. Material drawn from interviews with participants is used to show the different perceptions of certainty available. It is argued that the craft element of science gives scientists confidence in their own results, but is also a source of uncertainty for scientists unfamiliar with the craft practices in use outside their own fields. The fundamental uncertainty of scientific knowledge encountered by scientists working at the research frontiers is discussed, and it is suggested that this type of uncertainty is unlikely to be revealed in interview material because scientists are aware of the possible public audience for their remarks. The implications of this for the analysis of public-science debates where scientific certainty is a contentious issue are briefly discussed.

The Sun-Set: The Presentation of Certainty in Scientific Life

Trevor J. Pinch

Many issues which are normally thought of as technical matters within science have been shown, in recent studies, to be amenable to sociological analysis.[1] The technical conclusions reached by scientists can be understood as resulting from a process of 'social negotiation'. The apparent certainty of scientific knowledge is the outcome of these negotiations. This means that the degree of certainty of a piece of scientific knowledge is, in itself, available to different perceptions, interpretations and presentations.

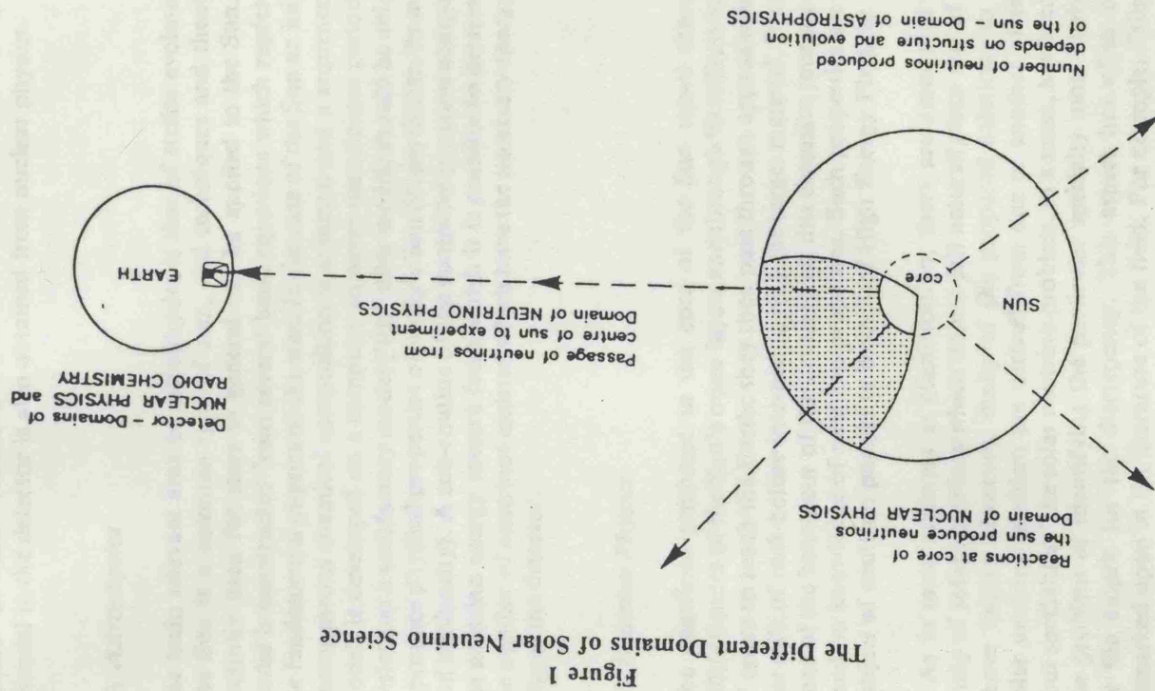The social permeability of scientific certainty is most obvious

where technical debate is thrust to the centre of public attention — for example, as in the nuclear-power debate. Those who favour nuclear power stress the reliability and certainty of the relevant scientific knowledge, whilst their opponents express doubts and scepticism.[2] In more esoteric areas, scientific certainty can be an equally contentious issue. In solar neutrino science, an area of physics which does not attract much public attention, an analogous debate is to be found. The perceived certainty and uncertainty of scientific knowledge in solar neutrino science forms the subject matter of this paper.

Much of the material presented below results from interviews conducted with solar neutrino scientists (the 'Sun-Set'). In the first part of the paper I present brief technical details of solar neutrino science and describe the emergence of the problem of scientific certainty in the technical literature of the field. I then outline my approach to the investigation of certainty amongst the Sun-Set, and discuss examples of the different perceptions of certainty and uncertainty. Finally, I elaborate briefly on the suggestion that the debate in solar neutrino science can be compared with debates over scientific certainty which occur in publicly contentious fields of technical knowledge, such as those related to nuclear power.

Solar Neutrino Science — A Focus of Uncertainty

Neutrinos are massless, chargeless particles produced as a by-product of nuclear reactions. Such aetherial particles are very difficult to detect, and billions upon billions pass unnoticed through the Earth every day. Solar neutrinos are neutrinos produced in the core of the Sun as a result of nuclear fusion. Solar neutrino science is the study of these neutrinos and it has been dominated by an outstanding problem — 'the solar neutrino problem'. This is the clash between the theoretical prediction of the neutrino fluxes emitted from the Sun and the results of an experiment designed to detect this flux — only one experiment has been completed so far.[3] Although the exact figures of both the theoretical prediction and the experimental result have changed since the first experimental result was reported in 1968, the number predicted has always been larger than the number detected.

Many suggestions have been made as to the cause of this discrepancy, but, so far, there is no agreement about where the dif-

Figure 1　The Different Domains of Solar Neutrino Science

Number of neutrinos produced depends on structure and evolution of the sun — Domain of ASTROPHYSICS

Passage of neutrinos from centre of sun to experiment Domain of NEUTRINO PHYSICS

Detector — Domains of NUCLEAR PHYSICS and RADIO CHEMISTRY

EARTH

SUN

core

Reactions at core of the sun produce neutrinos Domain of NUCLEAR PHYSICS

ficulty lies. Typically, participants tend to locate the cause in one or another of the sub-branches of solar neutrino science. Since both the theoretical computation and the experiment are very complicated the debate draws upon expertise from several scientific areas. The four main specialties[4] involved are radiochemistry, nuclear physics, astrophysics and neutrino physics. In order to understand the arguments concerning these different fields, a brief technical description of their contribution to solar neutrino science is necessary. In reading the description given below it may be helpful to refer to Figure 1.

(1) Radiochemistry

This field is exclusively concerned with the design and interpretation of the experiment itself. The solar neutrino detector consists of a large tank (100,000 gallons) of target material (perchloroethylene containing chlorine-37 atoms) situated approximately one mile under the Earth's surface. Radiochemistry's contribution concerns the processes whereby the radioactive isotope of argon (argon-37), formed by the interaction of neutrinos with chlorine-37, is extracted and counted. Only a few atoms of argon-37 are formed and these are removed by purging with helium. The argon-37, after purification, is placed in a small proportional counter, where it eventually decays by emitting an electron of characteristic energy, which can be detected. Although both the tank and counter are shielded from cosmic rays and other background emissions, great care has to be exercised in the experiment to ensure separation of the signal from the background 'noise'.

(2) Nuclear Physics

At the heart of the Sun is the nuclear-energy production process whereby hydrogen is converted into helium via the proton-proton chain of reactions. The details of these reactions and the rate at which they occur govern the numbers and the energy of neutrinos emitted and are established by nuclear physics. Fluxes of neutrinos are produced by several of the nuclear reactions in the Sun, but only one reaction (the decay of boron-8) produces significant numbers of neutrinos of the necessary energy to trigger the detector. The

number of these neutrinos expected to interact with the target material in the detector is also obtained from nuclear physics.

(3) Astrophysics

The main relevant area of astrophysics is that of stellar evolution. The Sun is a common type of star, and techniques and theories routinely used for stars in general can be applied to the Sun. A model is constructed from several basic equations which represent the fundamental physics of the star. In the case of the Sun an initial homogeneous chemical composition is assumed and a sequence of models is developed on a computer to cover the relevant period of evolution: a satisfactory model ('the' solar model) should be able to reproduce physical properties of today's Sun (in particular, its mass and luminosity). A sub-routine of the computer program computes the neutrino energy spectra from which it is possible to determine the number of neutrinos emitted which have the necessary energy to trigger the detector.

(4) Neutrino Physics

The neutrinos produced in the core of the Sun travel through 400,000 miles of the Sun's outer layers and through 93 million miles of space to reach the Earth; they then pass through approximately one mile of rock before interacting with the target material. Predictions of the behaviour of the neutrino on this extensive journey depend on knowledge of neutrino properties. Such properties are the subject of neutrino physics (a branch of High Energy Physics).

As in most attempts at classification, there are aspects of the study of solar neutrinos which do not fall naturally into any of the above fields.[5] However, many of the proposed solutions to the solar neutrino problem are derived from one or another of these four specialties. The solar neutrino problem, in a sense, has become the problem of identifying the particular specialty most likely to be the culprit for the discrepancy. This aspect has often been remarked upon in the literature of the field. For example, Trimble and Reines, in an influential review of the area, have written that 'the critical' problem is to determine whether the discrepancy is due

to faulty astronomy, faulty physics or faulty chemistry'.[6] Similar views have been expressed by other scientists.[7] Because each of the different fields is highly specialized, solar neutrino scientists tend to have expertise in one specialty only. The blame for the solar neutrino problem is thus likely to be placed at someone else's door. As Cameron has described it:

> There is a tendency for scientists working in any one of these fields to feel that the chances that his field is the culprit is so remote that one of the other fields must be involved.[8]

Scientists may emphasize one particular specialty conflict or another in response to the problem. For instance, the conflict between physics and astrophysics has been stressed by two key members of the Sun-Set, John Bahcall and Ray Davis:

> The attitude of many physicists towards the present discrepancy is that astronomers never really understand astronomical systems as well as they think they do, and the failure of the standard theory in this simple case just proves that physicists are correct in being sceptical of the astronomers' claim. Many astronomers believe, on the other hand, that the present conflict between theory and observation is so large and elementary that it must be due to an error in the basic physics...[9]

The contentious nature of scientific certainty in solar neutrino science is evident from these kinds of statements. It appears that different perceptions of certainty are available and that such perceptions tend to be polarized between different specialties, with each blaming the other for the problem. However, the situation is not as straightforward as would appear from the written comments, as we shall see.

The Investigation of Certainty and Uncertainty in Solar Neutrino Science

The purpose of the investigation was to find how scientists in the different specialties of solar neutrino science evaluated the certainty of their own and other areas.[10] The sample consisted of nearly all the scientists who had made major contributions to the solar neutrino problem, and it was constructed from the literature of the area. The sample was crosschecked by asking scientists themselves

who they thought it was important for me to interview (snowball sampling).

Unfortunately for sociologists, the scientific world rarely, if ever, provides a neatly divided population on which to found sociological research. It would be felicitous indeed if the four specialties identified above were represented in equal numbers amongst the sample. Further, it would make things much more straightforward if scientists had kept within one specialty or another. Although the four specialties can be broadly identified, and many scientists have worked exclusively in one, there are a number of complications. Other specialists can be seen to have made a contribution — notably atomic physicists, fluid hydrodynamicists, climatologists, and (non-radiochemical) neutrino experimentalists — and there are always maverick scientists to be found who have worked in more than one specialty (in one case, in aspects of all of them). Also, there is a preponderance of nuclear physicists and astrophysicists; and, to make things even more complicated, some of them refer to themselves as 'nuclear astrophysicists'. Of the forty scientists in the Sun-Set that I contacted, only eight could be unambiguously identified as having expertise in nuclear physics, eleven in astrophysics, three in neutrino physics and two in radio-chemistry. The remainder of the respondents came from yet other fields or combinations of fields.

The methods used in the study involved in-depth semi-technical interviews with respondents. Rather than asking scientists directly about the degree of confidence they attached to their own and other areas, I would often approach the topic obliquely by discussing with them a number of proposed solutions to the solar neutrino problem chosen from the different specialties. By the end of the interview it would usually be clear where (if anywhere) they felt the problem lay.

Ideally, when trying to interpret people's actions, the sociologist aims to share the 'frame of meaning' of the actors under study. However, in research on scientists, where the frame of meaning is provided by an esoteric knowledge culture, the situation of the sociologist is nearly always far from ideal.[11] In this particular study, where respondents are drawn from four different scientific specialties, the problem is compounded because the sociologist is required to become familiar with four different sets of competences. In a short fieldwork trip, where on average only two hours is spent in conversation with each respondent, it is doubtful

Most respondents seemed aware of such dangers and stressed their efforts to explore possible avenues of uncertainty in their own field as thoroughly as possible. For instance, one reaction (not untypical) to the initial appearance of the solar neutrino problem was:

It's very easy to say it's somebody else's problem, but really you had to go back and look at your own field.

It must be borne in mind that the solar neutrino problem has been an outstanding scientific problem for over a decade. In November/December 1978, the standard solar-model predictions were of the order of a factor of three greater than the most recently reported experimental value. An agreed-upon solution would thus be heralded as a major scientific achievement. There is therefore every reason to believe that the solar neutrino scientists are being perfectly sincere when they stress their efforts to find a loophole within their own specialties first.

I will now go on to outline the more detailed comments of respondents. I have divided the material into three sections. The first section, entitled 'Uncertainty', deals with comments made by respondents about *other* areas, and in particular indicates the lack of certainty which respondents attach to these areas. In contrast, the second section contains comments made by specialists about their *own* areas: in general these reveal a much greater degree of certainty. The third section again contains comments made by specialists about their own areas, but in this case they refer to sources of doubt and uncertainty. This third set of comments is somewhat atypical, and is discussed separately towards the end of the paper in the context of a more general discussion of certainty and uncertainty in science.

Uncertainty

All four specialties — radiochemistry, nuclear physics, astrophysics, and neutrino physics — were deemed to be sufficiently uncertain as to be suspect.[13] I will give illustrations of the types of comment engendered by each in turn. In every case it is specialists from one or more of the *other* areas who make the comment.

whether sufficient empathy can be built up for scientists to confide fully in the interviewer. It is therefore important to place respondents' comments on certainty in solar neutrino science in terms of their wider perceptions of the role of the interviewer, and, in particular, the likely audience for their remarks.[12]

Many respondents felt that being a sociologist and an outsider put me in a privileged position because I could talk to everyone, whereas most respondents themselves only talked to colleagues working in their own specialties. It was felt that I would be in a position to provide an overview of the whole field. My role was frequently taken to be that of a scientifically informed public inquisitor who was able to go from specialty to specialty and listen to each one's defence in turn. My relativist claim that I could not take sides in the scientific debate was interpreted in some cases as a licence to try and convince me of the merits of the work done in that particular area. Because I was identified with the wider public audience for the debate, many of the comments I received tended to be overly defensive. We will return to this point later but it should be borne in mind when reading the comments drawn from interview data.

Uncertainty and Certainty Amongst the Sun-Set

Most of the comments I received firmly support the analysis of scientists cited above; researchers in the field tend to blame any area other than their own for the problem. Respondents were well aware of this tendency. A typical comment was:

Since there is this discrepancy, I don't think it's going to disappear except by some give on the part of either a physicist or an astronomer or Z [the main experimenter].

Some members of the Sun-Set even expressed concern that this attitude had led, or might lead, to neglect of the problem. As one told me:

People were saying 'Well, it's an interdisciplinary problem', and therefore if there were difficulties it lies with some other discipline...so it was a kind of swept-under-the-rug kind of thing.

Uncertainties in the Radiochemistry

This area was criticized by a nuclear physicist:

I find it very hard to see how we can pick up a few of these argon-37's, and maybe you are losing some of them... I just sit back here and think of this enormous big tank... and you are trying to flush out a few atoms.

A similar criticism was levelled by an atomic physicist:

The experiment is so complicated and it's so big, and for a number too, there must be something in this experiment that's not coming out right.

One respondent (an astrophysicist) mentioned a specific worry to do with the cosmic-ray background in the tank:

He has to make estimates of the cosmic-ray background, for example... It has to be calculated, it's not actually measured... I think he has to extrapolate.

Doubts concerning the background were also expressed by another astrophysicist. In this case it was the background in the counter where the radioactive argon-37 decays which was considered problematic:

He's getting his background too high and there is a lot he's throwing away that are actually counts. There is some reason to suspect that.

This respondent's objection hinged on the point that, in the extraction of signal from noise in a low-counting experiment such as this, the designation of 'signal' and 'noise' is to an extent arbitrary because the noise can produce the same signal-like characteristics as the 'real' signal. What is to count as 'signal', as opposed to 'noise', is not immediately obvious, and depends on assumptions concerning the nature of the expected signal. Similar reservations were expressed by another astrophysicist who stressed the difficulty of interpreting low-counting experiments in general:

That's ultra small statistics, and in particle physics, when you deal with that kind of thing, you have to be incredibly careful — in fact you don't believe anything you do at that level... Like I say, this is a very marginal experiment, right on the very edge.

Already from these comments certain themes are starting to emerge which, as we shall see below, tend to recur throughout the remarks about uncertainty. References to large complicated systems, extrapolated data, and arbitrary assumptions, reappear amongst the comments on uncertainties in areas other than the radiochemistry.

Uncertainties in the Nuclear Physics

As with the radiochemistry, the main criticisms of nuclear physics came from outside the specialty. A worry shared by many astrophysicists, for instance, was the need to extrapolate the cross-section data (the probability of nuclei interacting), obtained from laboratory measurements, down to energies comparable to those found in the core of the Sun. As one explained:

It's all measured at much higher energies and then extrapolated downwards on the basis of theory, and one knows what a risky business that has been in the past. [He went on to describe an infamous case.]

A specialist in the neutrino physics had similar reservations:

The nuclear physics inputs... They are also extrapolations. We haven't got measurements... and by making so many extrapolations, if something goes wrong?

Some of the scepticism concerning the reliability of the cross sections seemed to stem from the fact that there had been changes in the past. The original prediction of the solar neutrino flux to be detected had been of the order of 36 SNUS (solar neutrino units) in 1964, but this had dropped to approximately 10 SNUS by 1968, partly as a result of changes in the nuclear physics. One crucial cross section had been found to be in error by a factor of five. Again it was an astrophysicist who felt that this was particularly significant:

And you know W [a well-known nuclear physicist] used to holler about how important it was to get laboratory-based cross sections, which is very true. On the other hand, to go back into your data and find you screwed up by a factor of five is, to me, evidence that one ought to be a little more cautious in statements about how certain things are based on laboratory experiment...

If anything I've learnt from all this exercise it is that the physicist tends to be extremely arrogant about the precision of his work, yet in instance after instance, this precision has been less than exact.

Another astrophysicist remarked:

My feeling from talking to a lot of people who are experts in the field is [that] they tend to say 'Oh, no, no! We couldn't possibly have made an error...'[or] 'We've found them all now'. [But] how the hell do you know you've found them all? You've found all the known ones...One knows from experience that there are sometimes funny little things that creep in that nobody anticipated. That's the way it is.

What is particularly of interest here is that the uncertainties are suggested as arising from laboratory measurements — the area that many would regard as the most solid part of any science. As we shall see later, these respondents were not alone in their diagnosis.

Uncertainties in the Astrophysics

This area received more blame from the interviewees than any other. Again most of the criticism came from outside astrophysics. The point should be made at the outset that most scientists carry with them the notion of a prestige hierarchy of scientific disciplines.[14] One factor related to this hierarchy is judgements as to the confidence to be placed in the knowledge produced by different disciplines. In general, astrophysics has a low place in such a hierarchy. Indeed the astrophysicists would often be the first to point this out. However, as far as I could see the appeal to such a hierarchy exerted little concrete influence on the assessment of this *particular* problem. Some scientists felt that astrophysical evidence was rarely good enough to overthrow more 'fundamental' physics, such as weak interaction theory. But no respondent claimed that the uncertainties lay in the astrophysics *simply because it was astrophysics.*

One common source of worry amongst those who were not astrophysicists arose from the need to base the data used in the construction of the solar model on the (directly observable) outermost layers of the Sun — thus leaving room for an interior which might be quite different. This is, of course, the familiar problem of extrapolation. The neutrino physicist quoted above (on p. 141) who

was worried about extrapolations in the nuclear physics pointed to a similar problem in the astrophysics:

The astrophysicists have to make a number of assumptions. They have to conclude how the Sun looks inside from the observations outside, and it's quite an extrapolation....

The complicated nature of the solar model and the need to make many assumptions was another source of worry. One neutrino physicist said:

[The calculations] are pretty complicated, they involve considerable numbers of assumptions. I myself think that it may well be that one or another of the assumptions that enter into the astrophysical calculation doesn't hold.

However, a differing view seems to be implicit in the following comment expressed by a specialist in the radiochemistry:

I feel the Sun is very complicated and that there are many things that aren't taken into account — and in essence a lot of this is a gross extrapolation of elementary physics.

In this case it is the complicated nature of the *Sun*, and the failure of the solar model to account for it, which forms the basis of the criticism.

Several respondents drew attention to the uncertainties in the opacity calculations. These calculations are used to determine the rate of flow of energy out of the Sun and involve the computation of the abundances and distribution of elements throughout the Sun. A nuclear physicist remarked:

This is now really a question of fairly advanced atomic physics and somewhat beyond anything one can measure.

Some semi-philosophical objections were expressed about the basic procedures used in stellar evolution and the construction of a solar model. It was pointed out that stellar-evolution theory was not a deductive theory and that in order to construct a viable solar model various free parameters had to be adjusted to fit empirically. One respondent (a fluid-hydrodynamicist) summed up the situation as follows:

Stellar evolution is not a deductive theory... There are parameters and certain aspects of the theory when applied to stars like the Sun that are just empirically determined, that are now known.... The recipe seems reliable.... People have a certain faith in it. But someone outside the field wouldn't get too worked up over it.

Again these types of criticism are more compelling for those on the 'outside' of the immediate specialty.

Uncertainties in the Neutrino Physics

This area was not blamed by many respondents. The neutrino physics is probably the area that has been least investigated, as reflected by the small number of neutrino physicists who make up the Sun-Set. However, in the last few years, with the discovery of a new neutrino (the 'tau'), and with the possibility of a new generation of solar neutrino experiments which provide a more direct test of hydrogen fusion in the Sun, there has been increased interest in the neutrino physics. Some respondents felt that it was a distinct possibility now that there might be some process, such as 'neutrino oscillation', occurring and that such a possibility certainly could not be ruled out. One astrophysicist cited a famous quotation by Gell-Mann within his comment to me:

One has learnt more about high energy particles and extra neutrinos... and there is some more phase space that we didn't know about. Clearly there are some things that we don't understand yet about weak interactions. Now there is no way that you can rule it out, and what Gell-Mann said: 'If you can't rule it out, it's mandatory!'

Another astrophysicist claimed that the neutrino physics was a

...whole big complicated process, the part that has been least subject to direct experimental or observational test.

A cosmic-ray experimentalist with whom I talked also had doubts about this area — in this case connected to the elusive nature of the neutrino. As he described it:

It's because it's a particle without mass and charge, so you don't have clear interactions and whatever interaction you assume exists is always an assumption.

As with other areas, we can see that some respondents tarred this field with the same brush of complexity, lack of observation and over-reliance on assumptions, and it is again the non-specialists who find such doubts compelling.

Discussion of Arguments on Uncertainty

The main conclusion to be drawn from the above comments is that respondents tend to have least confidence in the areas outside their own immediate specialties. This is not to say that every area with which scientists were unfamiliar was held to be suspect. Many were happy to rely on the expert opinion of their colleagues who claimed that *that* particular area was solid. It would be wrong to give the impression that members of the Sun-Set are automatons who always apportion blame into any field but their own. As has been stressed already, there were good reasons to believe that scientists had first looked carefully in their own fields. The irony of putting the blame elsewhere was not lost on respondents. In addition not everyone put the blame exclusively in one area. Some thought a combination of small uncertainties in several fields was the most likely explanation. Yet others were quite prepared to admit that they just did not know where the uncertainty lay.

From the various comments on uncertainty recorded in the preceding section, several common themes are apparent. It is striking that most of the criticisms are based on a highly idealized notion of what well-grounded reliable science should look like. It would appear that such science should only deal with simple systems, make direct observations, and have as few assumptions, extrapolations and complex theoretical calculations as possible. The use of such characteristics perhaps reflects the rhetorical power which overidealized pictures of science still exercise.

If a more realistic picture of scientific activity is taken, it can be seen why it is always possible for scientists to make the above types of criticisms of *other* specialties. We know that scientific activity incorporates many craft practices and areas of tacit knowledge.[15] It is the ill-defined and messy procedures inherent in craft activity which seem to provide the bulk of the ammunition for the criticisms documented above.

The certainty of any area can always be challenged by attacking

its craft practices. In most cases such a challenge is redundant because the specialty can be seen to be producing 'correct' results. However, when there is doubt as to this, as in the specialties which make up solar neutrino science, the craft practices can become a source of criticism. Critics can concentrate their attacks on the non-rigorous craft-element of research practices and use this lack of rigour as a justification for suspicions of the knowledge produced in that particular specialty. The taken-for-granted assumptions and extrapolations[16] which are part of the routine craft practices of one area may seem mysterious and open to doubt to scientists unfamiliar with that specialty. It is these types of criticisms which seem to form the basis for many of the doubts expressed about the certainty of areas of solar neutrino science.

In essence, when scientists call into question the certainty of another area of science in this way, they are doing something very similar to the work done in recent sociology of science.'' By the examination of the microprocesses of scientific activity, sociologists have been able to show that scientific knowledge rests upon detailed social negotiations and that, in principle, all the assumptions that go into scientific arguments can be challenged. That such a messy and contingent process can lead to reliable knowledge is a problem not only for the sociologist then, but also for the scientist.

Such doubts are less likely to be expressed by those scientists who themselves routinely use the craft practices in their own specialist areas. In the section which follows we find that scientists do indeed mostly express a great deal of confidence in their own specialties.

Certainty

Many respondents who found uncertainties in other areas were quite prepared to defend their own particular specialty. To quote the detailed technical defences made of each area would make this paper unnecessarily tedious. Instead, I will briefly describe the attitudes of the experts in the various fields, and give some representative quotations.

Certainty in the Radiochemistry

In response to the accusation that the radiochemistry is suspect

because the experiment is big and complicated, I can quote a specialist in this area:

Experimentally, this is a simple thing and it's just a big tank and a bunch of pumps, and you just collect the sample, and put it in a counter, and if you ask chemists 'will that work?', they say, 'sure, why not?'...Chemists have been doing this all along, and they're used to these things in a sense...There are many experiments of that general sort, this is a little more extreme than most, but it's for argon too, it's not for any complicated chemistry.

Doubts that such a small number of atoms could be extracted from such a large tank were answered by reference to detailed experimental tests of the procedure, which demonstrated that this could be done. It was freely admitted that the cosmic-ray background at this depth (approximately a mile below the Earth's surface) was extrapolated from measures of the background made at lesser depths, but this extrapolation was considered to be quite standard. It was felt that if there was any uncertainty over the background it was more likely that it had been *underestimated*, thus lowering the detected signal and making the problem even worse.

Doubts about the procedure for the separation of the signal from the noise were discounted by appealing to more detailed and refined analyses of the experimental data which seemed to give support to the particular assumptions on which the procedure was based.

The general impression was that the radiochemistry was solid enough as far as the radiochemists were concerned. As one put it:

Now you see the chlorine experiment has gotten to be a sorta hard-science experiment. You are going to test it and you know everything, and there's not anything loose, there's nothing hanging out, right?

Certainty in the Nuclear Physics

A specialist in the nuclear physics had recalculated the uncertainties in this area shortly before my visit. I was told that, based on this work:

I would say at the 99% confidence level that I'm convinced the nuclear physics parameters aren't large enough to make a change bigger than about 1.4 SNUS. [The discrepancy between experiment and theory at that time was 3.1 SNUS.]

The need to extrapolate the cross-section measurements from higher energies was acknowledged, but it was considered to be a quite straightforward technical issue for each of the relevant cross sections. A typical comment was:

We do have the method of using cross-section factors in which the Coulomb-barrier factor, which causes all the big variations in energy, is taken out in a perfectly straightforward way. And then the extrapolations are matters of ten or fifteen percent or so because these cross-section factors are in general constants.

It appears that the main source of distortion in the extrapolations is low-lying resonances, but it was considered that the checks for these in all the relevant reactions had been sufficient. As one expert in the nuclear physics told me:

I think we're in good shape frankly. I don't think we missed any bets on any of the cross sections. I think that one can always sharpen them up. I don't think the problem lies there.

The response to the point that the values for the cross sections had moved around previously was to lay stress on the fact that there had not been any change for some time now. The situation was summed up for me as follows:

There just hasn't been any substantial new change since the '72 Irvine Conference [an informal solar neutrino conference]. There are lots of things that have been checked out and eliminated and so forth, but the nuclear-physics problem basically seems to have sorta stabilized... And it's not really clear looking at the nuclear physics even what we would do next, quite frankly.

Many of the nuclear-physics measurements were made at the Kellogg Radiation Laboratory of the California Institute of Technology. The attitude of this laboratory to the nuclear-physics side of the problem was described to me by one of the physicists who had been closely associated with the work:

Essentially, a decade ago, the measurements in this laboratory had been pushed to the point where they were self consistent, where the statistical errors were in the order of ten percent or so... My attitude over that decade has been [that] we have done the best we can.

Certainty in the Astrophysics

The view that the Sun is tremendously complicated and poorly understood was not shared by those astrophysicists responsible for

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making solar models. The predominant view of the solar-model scientists is perhaps that expressed by Iben, one of the leading experts in stellar evolution theory, at the Irvine conference in 1972:

Let me remind you why it's pretty tough to find holes in the solar models. The Sun is just a big gasbag... [18]

Iben then went on to outline the basic principles and assumptions of the solar models. His conclusion was:

In sum, my feeling is that the best we can do, varying all the available parameters, is to vary the neutrino flux estimate between three and nine SNUS... [19] [that is, not enough to bring the theory within the bounds of the experimental results].

When I talked to the solar-model scientists they still held the view that the basic models were sound. However, they were prepared to entertain the notion that there were uncertainties in the opacities which were fed into the models. A typical comment was:

I come from the point of view where the astrophysics is basically fundamentally correct and that the discrepancy we have now at the present time is probably an uncertainty in the opacities or something else.

It is interesting to note here how this solar-model specialist dissociated himself from the opacity calculations, an area which many respondents assumed to be an integral part of the astrophysics. [20] We will return to the opacity calculations below.

The general feeling amongst those involved in stellar-evolution theory was that the Sun was the most straightforward star to compute, as it was in the best understood phase of evolution (quiescent main sequence) and was the star for which we had the most detailed knowledge. Although the computation itself was complicated, they pointed to the widespread agreement amongst different groups doing independent computations as to the value of the predicted solar neutrino flux. It was felt that to make the assumption that the interior of the Sun was basically different from the exterior, say in heavy-element abundance, was to make the Sun a special case, when all the indications were that it was a typical main-sequence star. It was acknowledged that the figures for the neutrino flux could always be depressed by making 'non-standard' assumptions concerning the Sun, but most astrophysicists could see no justifica-

tions for making such assumptions. Objections to the basic procedures for the generation of solar models were discounted. It was acknowledged that there were weaknesses in the model (such as the lack of an adequate theory of convection), but it was considered to be a reasonable practice to adjust parameters until the model reproduced the observable properties of the Sun.

That there were some uncertainties in the opacities was widely recognized amongst the astrophysicists. I was unable to talk to the scientists responsible for making the opacity calculation, but I did talk to scientists familiar with the calculations. The general impression I received was that, although there might be some uncertainties, these would not be enough to accommodate a discrepancy on the scale of the solar neutrino problem. As one told me:

I don't think they can be off anywhere near large enough to make the required effect because 75% of the opacity comes from the hydrogen and helium. A first year graduate student can calculate their opacities.

Another told me:

I can imagine phenomenal errors in the opacity, no trouble...but they all go the wrong way. What you have to do is make the material more transparent....If anything it's got to be the other way.

Certainty in the Neutrino Physics

The few neutrino physicists I was able to speak with were reluctant to rule out absolutely the possibility of an explanation emerging from their field. There was agreement, however, that the present experiment and theory were not direct enough to show the need for a change in a fundamental theory of neutrino physics. As one told me:

By making many extrapolations [in the astrophysics and nuclear physics], if something goes wrong you are not permitted to make a fundamental conclusion, that's my point.

The neutrino physicists felt that other areas should be ruled out first, before definitely concluding that the problem was in their area.

Discussion of Arguments on Certainty

What the above section has shown is that scientists find no trouble in defending their own areas. Activities, which are regarded by non-specialists as overly complex, appear as simple and straightforward from within. Areas of doubt, such as the recourse to extrapolations and assumptions, seem solid enough to those who routinely make such extrapolations and assumptions. It would seem that the confidence which the specialists place in their own techniques results in part from the fact that they produce self-consistent answers in terms of this problem (and in some cases have produced such answers for a number of years) — and, furthermore, that such techniques to all intents and purposes 'work' when they are used on other less controversial problems. The ability to produce the 'correct' result outside of the solar neutrino problem must give the specialists confidence in the validity of their techniques. Even if uncertainties are acknowledged, as in the cases of the cosmic-ray background for the experiment and the opacities for the solar model, they can be discounted because the uncertainties are held to go in the wrong direction to solve the problem. No one, of course, wants to make the problem worse!

Again, if we take the picture of science as a craft activity, it is not surprising that the practitioners who routinely use the criticized procedures have the greatest confidence that they work. Craft practices form an essential part of scientists' grasp of their particular area of science. They enable the scientist to get on with the everyday business of doing science. Such practices reside within a particular scientific sub-culture, and as long as the normal practices for that area are followed the scientist can be confident in the knowledge produced. Because of the nature of such practices (they rest on tacit knowledge, they are frequently messy, and they cannot easily be generalized outside the local situation) the scientist may not be able to spell out all the details of the procedures followed in such a way as to convince the determined critic. Thus, as we saw earlier, scientists unfamiliar with the details of the particular practices locate the centre of uncertainty within such practices.

Uncertainty and Certainty in Science — the Place of the Core-Set

The comments documented in the section on certainty are consistent with the predominant view of the reliability of science as a

whole. There is little doubt that for most intents and purposes science is characterized by many areas of certainty. There is confidence in most results and widespread agreement as to what constitutes sound and reliable knowledge. However, not all of science is like this. There are areas where controversy rages and the certainty of scientific knowledge, *even to the practitioners*, seems far from being assured. Collins, in a recent paper, has defined these comparatively small areas of science where controversy exists at the research frontiers in terms of 'Core-Sets'.[21]

The Core-Set comprises a group of scientists who, in a controversy, are 'actively involved in experimentation or observation, or make contributions to the theory of the phenomenon, or of the experiment such that they have an effect on the outcome of the controversy'.[22] Since most of science is uncontroversial there are few opportunities to form Core-Sets, and most scientists will rarely be involved in Core-Set-type activity. Collins goes on to argue that it is only scientists within the Core-Set who are aware of the lack of compulsion of experimental claims, and are, therefore, aware of the general uncertainties encountered at the research front.[23] It is activity within the confines of the Core-Set, where reality is 'up for grabs', that is characterized by an aura of uncertainty.

In view of the conflict between measurement and theory the scientists working on the solar neutrino problem can be said to constitute a Core-Set. In view of this there is something puzzling about the above comments of specialists concerning their *own* areas. There are no references to the doubts and uncertainties which are typically felt by Core-Set practitioners. None of the respondents appear to have been overly zealous in drawing my attention to lacunae and weaknesses in their own fields. However, there were some indications that such feelings of doubt *do* exist. Some respondents, perhaps in their less circumspect moments, were prepared to entertain the possibility of uncertainties *within their own specialties*.

Uncertainty Amongst the Sun-Set (Again!)

The first indication I had that some members of the Sun-Set were prepared to admit that their own field could be faulted came from an astrophysicist; he made the following remarks towards the end of the interview:

I suppose that in a way the nuclear physicists are right when they say that the problem is with the astronomers — at least that's the way I feel. It's just that we are doing things in a very crude way. Obviously, we have a very complicated phenomenon happening there... It's impossible to take into account all these complications, so I am sure [the solar model] enormously underestimates the uncertainties.

Before making this comment, the respondent had to be assured that he would not be quoted by name on this point. It would seem that to admit publicly to uncertainties in one's specialist area was not normal practice among the Sun-Set. A similar episode occurred with a nuclear physicist, who expressed worries about his field — he was not even prepared to be tape-recorded on this point. He had taken me aside at the end of the interview and made his remarks privately. He was worried about the extrapolations in the cross sections because they sometimes involved very large factors. During the course of the interview he had not given the impression that he was deeply worried about such extrapolations.

Another respondent expressed misgivings about his field, having earlier given an impression of confidence. In this case it was the astrophysics that was the subject of concern. He told me:

I also get very distressed at times when I want to make myself paranoid or upset about the whole thing. I can decide that it's not worth doing astrophysics... at all. And that is why I reflect upon the success of the controlled-fusion programme and people having predicted the properties of a magnetic plasma beforehand. Now that's been one of the most dismal series of failures in the past decade and, after all, the solar interior is a plasma with reasonable magnetic fields. And who knows, maybe that's how [one possible uncertainty in the astrophysics] gets in.

One respondent was quite candid about admitting the uncertainties in his particular field (nuclear physics). He stressed that uncertainty could never be eliminated from any measurement, and, as the crucial cross sections for the solar neutrino experiment were measured rather than calculated, he felt there could be errors:

There is no way that we can calculate those cross sections or the rates in the Sun in principle, they have to be measured in the laboratory... Over the years these things have changed... and the point I'm trying to make is that there still could be systematic errors of which we are unaware.

This again was not the predominant view to be found expressed in the literature by the nuclear physicists.

Despite the remarks of these few respondents who felt there could be uncertainties in their own fields, there is no doubt that the major reaction which I encountered was the defensive one of laying stress on the certainty of their own various specialties and the uncertainty of others'. Rather than portraying their activity as very tentative and uncertain, as we would expect members of the Core-Set to do, respondents, on the whole, stressed the reliability of what they were doing. In this respect the comments received are similar to the views contained in the literature, although they are more extensive.

In the analysis of respondents' comments in interviews it is useful to think of the likely audience for their remarks. As I stressed earlier, many respondents seemed to regard me as a representative of the public and hence as a vehicle by which to transmit the public face of the debate. No one wanted to give public encouragement to the critics in the other fields. The point is that those specialists who are worried about their own fields are not able to translate such worries into specific solutions to the solar neutrino problem. If they could do so, then clearly there is much to gain from 'coming clean' and no one would keep quiet about it. However, at the moment, to confess in public to general worries (such as that the Sun is too complicated, or nuclear-physics measurements might not be reliable) will not do one's specialty any good at all. It is the perceived audience for the comments which, to a large part, may have determined the presentation of scientific certainty in this case.

Conclusion and Implications for Public-Science Debates

What I hope I have been able to illustrate in this paper is that the assessment of certainty in science is far from being a cut-and-dried issue. I have argued that it is the craft nature of many scientific practices which makes it difficult to assess certainty in science. Confidence is high amongst those who routinely use such practices; but, to those unfamiliar with them, they provide a source of doubt and uncertainty. The uncertainties expressed by critics can, on occasions, be shared by practitioners too — particularly when their work is shrouded in the aura of uncertainty which accompanies a frontier field or a Core-Set.

Solar neutrino science seems at the moment to be ridden with different assessments of scientific certainty and it is by no means clear,

on the basis of the arguments recorded here, which area, if any, is the 'real' culprit. If one set of statements — those on uncertainty — are pooled together, solar neutrino science appears to rest on very shaky foundations. On the other hand, if the comments on certainty are amalgamated, every area of solar neutrino science seems so solid that it is difficult to see how a solar neutrino problem could arise at all.

The lack of agreement over scientific certainty recorded in this paper is consistent with the view that the origins of assessments of certainty lie in the social world. Scientists, it seems, can 'rationally' express either certainty or uncertainty in their own and other areas. If such assessments are social in nature then it is not surprising that one constraint is the potential audience for comments on certainty. In particular, I have argued that, in view of the public shape which the solar neutrino debate has taken, when scientists perceive a possible public audience they tend to act defensively and stress the certainty of their own areas — while, at the same time, doubting the certainty of others'.

The relevance of the argument of this paper for public-science debates, such as nuclear power, is two-fold. Firstly, as scientific certainty can be a contentious issue in an area of basic science, it is no surprise to find similar debates appearing in public-science controversies. The second implication is that in public science, too, it may be useful to take into account the relevant audiences for comments on scientific certainty. For example, it may well be that scientists committed to the nuclear power programme will express greater confidence in public than they would admit in private, or perhaps to close colleagues. This suggests that the way to analyze such debates is not in terms of whether the contentious scientific knowledge is really certain or uncertain, but in terms of the different audiences for such claims. This is not to accuse scientists of cynical bias but to point out that nuclear power science is as much a social product as solar neutrino science.

Postscript March 1980

Since I interviewed the Sun-Set there have been some changes in the solar neutrino discrepancy. At that time the standard solar-model prediction was 4.7 ± 1.4 SNUs, and the experiment was registering a flux of 1.6 ± 0.4 SNUs. However, shortly after my trip one

nuclear physicist claimed to have discovered an error in one of the crucial cross sections. This would revise the theoretical prediction down to 2.6 ± 1.4 SNUs. The experimental value has subsequently also been slightly revised upwards to 2.2 ± 0.4 SNUs. Thus, for a time it seemed as if the solar neutrino discrepancy was going away: but the prediction has recently been revised yet again, using the latest opacity calculations! It would seem that the previous theoretical prediction was too low — the new prediction is 7.4 SNUs,[24] thus restoring the discrepancy. All the above changes are very tentative and await verification.

Postscript July 1980

Recent results of experiments carried out at nuclear reactors indicate that the problem could lie in the neutrino physics because of 'neutrino oscillation'.[25] If such results are confirmed, the theoretical flux could be reduced by a factor of two, and the problem would thus be almost solved. Whether the Sun-Set will accept this, and the problem cease to worry scientists, remains to be seen. Negotiations are in progress!

● **NOTES**

An earlier version of this paper was presented at a Conference on 'New Perspectives in the History and Sociology of Scientific Knowledge', jointly sponsored by the British Society for the History of Science and the British Sociological Association Sociology of Science Study Group, at the University of Bath, UK, 27-29 March 1980. I am exceedingly grateful to the many scientists who were prepared to give up their time to me.

1. See, for example, H. M. Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology*, Vol. 9 (1975), 205-24; B. Latour and S. Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills, Calif.: Sage, 1979); and G. D. L. Travis, 'Replicating Replication? Aspects of the Social Construction of Learning in Planarian Worms', in this issue, *Social Studies of Science*, Vol. 11 (1981), 11-32.
2. See, for example, Roger Williams, *The Nuclear Power Decisions* (London: Croom Helm, 1980).

3. The original experimental result is reported in: R. Davis, Jr, D. S. Harmer and K. C. Hoffman, 'Search for Neutrinos from the Sun', *Physical Review Letters*, Vol. 20 (1968), 1205-09. Useful reviews of the area are: J. N. Bahcall and R. L. Sears, 'Solar Neutrinos', *Annual Review of Astronomy and Astrophysics*, Vol. 10 (1972), 25-41; V. Trimble and F. Reines, 'The Solar Neutrino Problem — A Progress Report', *Reviews of Modern Physics*, Vol. 45 (1973), 1-5; and J. N. Bahcall and R. Davis, Jr, 'Solar Neutrinos: A Scientific Puzzle', *Science*, Vol. 191 (23 January 1976), 264-67.
4. Throughout this paper the terms 'specialty', 'field', 'discipline' and 'area' are used interchangeably. This usage reflects the same lack of precision which solar neutrino scientists themselves accorded these boundaries.
5. A good example is the opacity calculation, needed in order to work out the rate of energy diffusion through the Sun. This calculation has inputs from several fields such as cosmochemistry, geophysics, and lunar science. Although many solar neutrino scientists consider the opacities to be part of astrophysics, those engaged in making solar models consider them to be part of atomic physics — or else, simply, 'input physics'.
6. Trimble and Reines, op. cit. note 3, 1.
7. See, for example, A. G. W. Cameron, 'Summary of Conference on Solar Neutrinos, 25 February 1972', in F. Reines and V. Trimble (eds), *The Irvine Conference on Solar Neutrinos*, Proceedings of an informal conference held at the University of California, Irvine and the Western White House, San Clemente, California, 25-26 February 1972, Section D, 1-9. (Copies available from F. Reines, University of California, Irvine.)
8. Cameron, op. cit. note 7, 3.
9. Bahcall and Davis, op. cit. note 3, 264.
10. The data drawn upon in this paper are part of a wider study of the development of solar neutrino astronomy. The results of the wider study will be contained in my University of Bath PhD thesis (forthcoming).
11. For a more detailed account of the methodological problems posed by research on scientists, see H. M. Collins, 'The Investigation of Frames of Meaning in Science: Complementarity and Compromise', *The Sociological Review*, Vol. 27 (1979), 703-18.
12. The importance of the perceived role of the interviewer in analyzing interview data has been stressed recently by Gilbert: see G. Nigel Gilbert, 'Being Interviewed: A Role Analysis', *Social Science Information*, Vol. 19 (1980), 227-36.
13. Of those respondents who apportioned blame solely to one area, eleven blamed the astrophysics, four the radiochemistry, four the nuclear physics and two the neutrino physics.
14. See R. D. Whitley, 'Changes in the Social and Intellectual Organisation of the Sciences: Professionalisation and the Arithmetic Ideal', in E. Mendelsohn, P. Weingart and R. D. Whitley (eds), *The Social Production of Scientific Knowledge*, *Sociology of the Sciences Yearbook*, Vol. 1 (Dordrecht: Reidel, 1977), 143-70.
15. The craft element and tacit dimension of science have been stressed by various authors: see, for instance, Michael Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* (London: Routledge and Kegan Paul, 1958); H. M. Collins, 'The TEA Set: Tacit Knowledge and Scientific Networks', *Science Studies*, Vol. 4 (1974), 165-86; G. Nigel Gilbert and M. Mulkay, 'Contexts of Scientific Discourse: Social Accounting in Experimental Papers', in K. D. Knorr, R. Krohn and R. D.

Whiley (eds), *The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook*, Vol. 4 (Dordrecht: Reidel, 1980), 269-94; and J. R. Ravetz, *Scientific Knowledge and its Social Problems* (Oxford: Clarendon Press, 1971).

16. Craft skills are usually associated with experimental activity, but theoretical activity also involves many such skills. For instance, the complex calculations of physics require many craft skills; such skills are employed in the choice of the correct simplifying assumptions, the most appropriate formalism, and in the solution of arrays of equations.

17. See, for instance, Latour and Woolgar, op. cit. note 1. Similar conclusions are reached in H. M. Collins and T. J. Pinch, *Frames of Meaning: The Social Construction of Extraordinary Science*, final report (1978) on (UK) SSRC-sponsored project on 'Cognitive Dislocation in Science' (Bath: Bath Science Studies Centre Manuscript, 1979), to be published by Routledge and Kegan Paul.

18. Comment made by Iben during discussion. See Reines and Trimble (eds), op. cit. note 7, Section C, 16.

19. Ibid., 17.

20. We are dealing here with subspecialties within specialties — it would seem that the tendency to 'blame the other guy' reappears at this level also.

21. H. M. Collins, 'The Role of the Core-Set in Modern Science: Social Contingency with Methodological Propriety in Discovery', presented to the conference on 'Innovation and Continuity in Science', Vrije Universiteit, Amsterdam, 12-14 December 1979: to be published in *History of Science*, Vol. 19 (March 1981), in press.

22. Ibid.

23. Collins's remarks are mainly based on studies of experimental scientists, but there is no reason why they cannot be extended to cover theory as well — in which case we can expect a similar awareness amongst the Core-Set concerning the lack of theoretical compulsion.

24. J. N. Bahcall, 'Solar Neutrinos: Theory Versus Observation', *Space Science Reviews*, Vol. 24 (1979), 227-51.

25. J. N. Bahcall, W. F. Heubner, S. H. Lubow, N. H. Magee, Jr., A. L. Merts, P. D. Parker, B. Rozsayai, R. K. Ulrich and M. F. Argo, 'Neutrino Oscillations and the Solar Neutrino Problem', Institute for Advanced Studies, Princeton, preprint, June 1980.

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APPENDIX II cont.

THE THREE-SIGMA ENIGMA

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In a previous paper (The Sun Set: The Presentation of

Certainty in Scientific Life)[1] it was argued that the assessment

of certainty in science was essentially a social process. The

argument was illustrated by reference to my sociological study of

solar-neutrino science[2] - a field which has been dominated by one

outstanding problem (the 'solar-neutrino problem'). This is the

clash between the theoretical prediction of the neutrino flux

emitted from the sun by hydrogen fusion and the results of an

experiment designed to detect this flux.[3]   It was found that,

in response to the 'trouble' caused by this result, scientists

tended to claim that one or more of the various sub-fields within

solar-neutrino science were uncertain enough to allow for the

discrepancy between theory and experiment.Given the range of views

of certainty available, it was not possible to conclude that any

area of solar-neutrino science was more certain than any other.

The certainty of solar-neutrino science seemed to be a matter for

social negotiation.[4]

One of the difficulties of the previous paper was that

scientists' assessments of certainty were made across whole disciplines

and specialties.   It might be thought that the issue of scientific

certainty can more appropriately be investigated in the context of

individual scientific results - especially as, in many such cases,

statistical argument and probability theory can be brought into

play to produce a precise meaning of certainty.   In other words,

statistics in itself might be enough to settle the issue of

certainty in science.

In this paper, I hope to extend my previous conclusion by

showing that even a comparatively narrow statistical issue dissolves

into a complex web of interpretation and negotiation. In order

to do this, I draw again upon my study of solar-neutrino science.

In this case, I focus on a matter right at the heart of that

field - how scientists assess the significance of the discrepancy

between theory and experiment. In short, I will be looking at the

constitution of the solar-neutrino problem itself.

The Solar-Neutrino Problem - November 1978

The empirical basis of my study arises from a fieldwork trip

to the U.S. in November 1978, during which most of the scientists

who had worked on the solar-neutrino problem were interviewed.[5]

Shortly before my fieldwork the latest values for the theoretical

and experimental flux of neutrinos were presented at a solar-

neutrino conference.[6] These results are shown in Figure (1).

It can be seen that the most up-to-date theoretical value was

given as 4.7 ± 1.5 SNUs (solar-neutrino units or 'snews' - 1 SNU

equals 10^{-36} neutrino absorptions per target atom per second).

The average experimental value was given as 1.6 ± 0.4 SNUs. The

error bars on the experimental value are at one standard deviation

from the mean (known as one-sigma error bars). The meaning of the

error on the theoretical value is less clear-cut, as we shall see

below. In general this theoretical error cannot be determined

in a straightforward manner because of the large number of experi-

mentally determined parameters (and their associated errors) which

go into the theoretical calculation.

I showed a graph similar to Fig. (1) to most of the solar-

neutrino scientists with whom I talked. I was particularly interested

in their assessment of whether or not the discrepancy was significant.

In general, the comments I received fell into two categories.

FIGURE (1) EXPERIMENTAL RESULTS OF SOLAR NEUTRINO EXPERIMENT (1978 (1964))
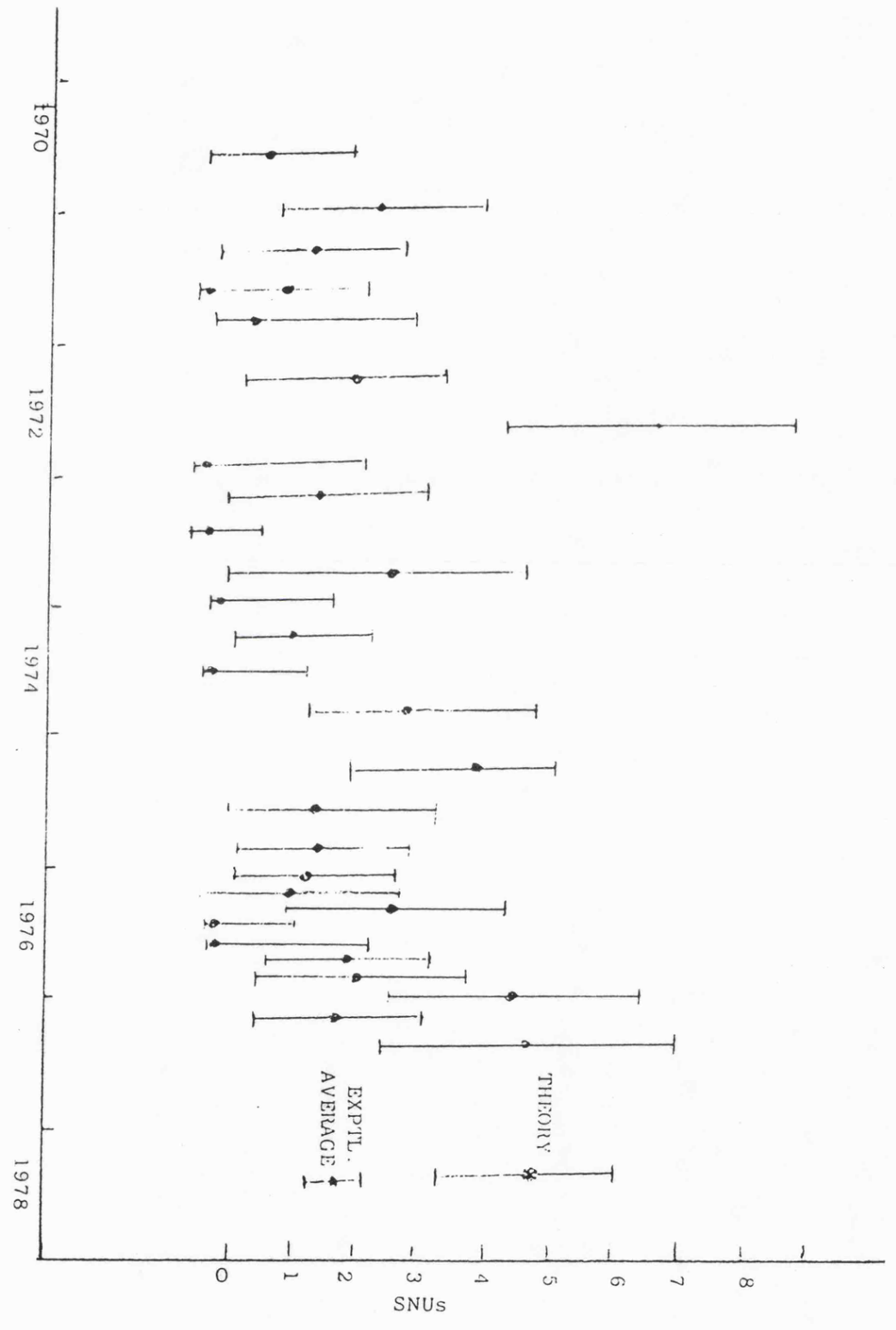
The most common view was that the data indicated that a significant

and serious discrepancy existed. Examples are the following:

> I think it's a major discrepancy. I mean, if you consider
> a science seriously, as stellar evolution is considered
> now - one of the soundest parts of astrophysics - and get a
> discrepancy like that. The whole thing is wrong, it's just
> disastrous.

> Well I would regard being 3 sigmas off as just being a
> disastrous discrepancy within the framework of the theory
> of this particular problem. [By '3 sigmas' the respondent
> means that the gap between the upper bound of the experimental
> value, 2 SNUs, and the lower bound of the theoretical value,
> 3.2 SNUs, is three times the one-sigma error, 0.4 SNUs,
> on the experimental value].

On the other hand, the following comments suggest another viewpoint -

that the discrepancy is not significant:

> There are some people who regard this problem as a very
> serious problem...Working in astrophysics I am used to large
> discrepancies between theory and observation, and often
> discrepancies that are much larger than this. And we just
> have to live with them to a certain extent and they clear
> up eventually...But after all, the discrepancy now is down
> to a factor of three.

> The accumulating data, which is still so scant as to contain
> a large statistical uncertainty...is no longer entirely out of
> line with the theoretical predictions. The most optimistic -
> i.e. the lowest - predicted value is only three sigmas from
> the mean observational value. What confidence do you place
> on a one in a hundred possibility?

This comment is particularly interesting because doubt is cast

on the significance of the discrepancy by an appeal to the three-

sigma level of significance. This claim, that the discrepancy is

non-significant because it is 'only three sigmas' (hereafter

referred to as the 'three-sigma argument'), forms much of the

subject matter of this paper.

The three-sigma argument seems to provide a straightforward

technical means by which the significance of the discrepancy

can be decided. By treating a three-sigma gap between theory

and experiment as non-significant, the respondent implies that we

need to know with a greater certainty than 99% that the discrepancy
is a real one. The three-sigma significance level, like significance
tests in the social sciences, provides a means by which a real
effect can be distinguished from error.

In order to understand the basis of the three-sigma argument,
it will be necessary to briefly discuss the nature of error in
general in physics.

Systematic and Random Error

In physics a distinction is usually drawn between two possible
classes of error - systematic error and random error. Systematic
errors arise in a series of measurements or calculations in a
uniform way.   Such errors are very often constant or at least
vary over time in a regular manner. Provided the cause of the
error is known its effect can usually be taken into account.

Random errors, on the other hand, are, by definition, unpre-
dictable random fluctuations in results.   Such errors, being un-
predictable, cannot be corrected for in individual cases. However,
their effect can be taken into account for many cases, such as
a series of measurements, by the use of probability theory.

The two classes of error are not well defined and it is
difficult to specify in advance which particular systematic
and random errors might be encountered.   The problem is parti -
cularly acute when novel physical measurements are attempted, as
with the solar-neutrino experiment. In such cases it is often not
clear just what the likely sources of error are. Thus, if there
is a discrepancy, a variety of explanations as to its cause are
possible. The following three explanations form the main
candidates:

(1) The discrepancy is caused by one or more (unknown) systematic errors.

(2) The discrepancy is caused by one or more random errors.

(3) The discrepancy is not caused by any error but is in fact a 'real' effect.

In addition, there is always the possibility that some combination of the above explanations is the cause of the discrepancy. For instance, there may be a small real effect coupled with systematic and random errors.

It seems that the three-sigma argument provides a means of assessing the likelihood of explanation (1) - the possibility of systematic error. If the discrepancy is no larger than three sigmas then the most likely cause is one or more systematic error. This can be seen clearly from the following formulation of the argument:

> One knows from experience that if you don't have three sigmas or more distinction there, you'd better watch out because Nature plays dirty little tricks on you. All you need is a small systematic error in here somewhere and the whole thing goes down the drain.

It should be noted, that even if there were a larger discrepancy than three sigmas it need not necessarily signify a real effect (explanation (3)). There is still the possibility of random error (explanation (2)). We shall return to the discussion of random error below, but it is important to remember that the distinction between the two classes of error is not absolute and that remarks made concerning the assessment of systematic error often apply equally to the assessment of random error.

The Application of the Three-Sigma Rule

Those scientists who expressed the view that the discrepancy

was not significant at the three-sigma level often felt justified

in using this criterion because of their previous experience -

either in physics in general (as some of the above comments

indicated) or in the context of this particular problem. A typical

comment was:

> There is always the chance that there has been systematic
> errors made. We've seen that in this field in the past, you
> see it in all kinds of fields...And physics is full of cases
> where two standard deviations in the long run turns out to
> be due to some systematic unknown error.

Apart from appealing to their own experience, some respondents

felt that it was appropriate to apply the three-sigma rule because

it paid to be 'cautious' or 'conservative'. The need for caution

is clear when it is considered that a lot of time and energy may

be wasted trying to solve a 'pseudo' problem. As one respondent

remarked, when referring to a possible avenue of exploration

which might solve the problem:

> It's so difficult and because it takes so much work, that's
> why you need something more than 3 sigmas to convince yourself
> there. is really a discrepancy. If there is a 10 sigma
> discrepancy and you're convinced there are no systematic
> errors, then you're willing to think bold. But if there is
> three sigmas are you going to go out and make a fool of
> yourself...and then five years later people will say 'Oh it
> was just the statistics, there's only a sigma and a half
> discrepancy now.' No you're not going to do that. It wouldn't
> be good science.

How scientists view the risks will depend on a variety of

circumstances and it is likely that risk assessments will differ

from individual to individual. For instance, if a scientist

happens to have a technique or approach which can easily be applied

to the problem then the risks involved will be reduced. On the

other hand, if a lot more work is required, as in the case above,

then a greater risk is entailed.

It is already clear that whether or not the three-sigma argument is found to be compelling is likely to depend on a range of contingent factors. In particular a scientist's previous experience and competences could both be important. Thus far, it does not seem as though scientific opinion on the significance of the discrepancy is likely to be forced one way or the other.

An integral part of the view that the discrepancy was not significant was the belief that the significance of the discrepancy had been exaggerated by some scientists. This belief was closely connected with the recent attempts by some solar-neutrino scientists to get funding for new experiments. These experiments are likely to be very costly (one estimate is $25 million) and part of the argument put forward for funding them has been based on the outstanding discrepancy discovered by the present experiment. Clearly, if the present discrepancy is considered to be non-significant, this particular argument for funding is in danger of becoming a non-starter. The following extract of interview material refers to Titus, a scientist who is a well-known exponent of the view that the solar-neutrino problem is a serious discrepancy:

> I believe that in their enthusiasm for getting support for these experiments, which are, after all, very expensive, some people have overdone the problem.

> Overdone it?

> Picture this as a worse crisis than I feel it is. Maybe for Titus it is a very serious problem...Some times one gets the impression that he has kept the problem alive...There is a little bit of showmanship involved...It's showmanship to make sure the agencies will keep supporting experiments.

Similar motivations were attributed to Titus to another respondent:

> People like Titus and others are firmly committed to the enormous discrepancy...

> Why are Titus and others committed to the discrepancy?

> ...I think some of them have a feeling...that if they don't
> claim there is a discrepancy you see they can't justify
> the gallium experiment [this is one particular new experiment].

It would be wrong to give the impression that the scientists

who made these comments did not themselves support the funding of

new experiments.   Both were careful to stress their support.

Indeed, the respondent quoted immediately above, who criticised

Titus for trying to justify new experiments by arguing there was a

serious discrepancy, felt that the lack of a clear discrepancy

was a better justification:

> I think one has to justify [the gallium experiment] on more
> honest grounds. Namely the thing is very muddy, for some
> reason the experiment doesn't give a clean answer...Let's
> stop pronouncing the answer based on this data...Let's go
> out and do the crucial experiment.

It is clear that both views of the significance of the dis-

crepancy can be used in the attempt to secure new funding. There

is perhaps here a lesson for sociologists to learn in the art of

fund-raising - physicists are, after all, very successful at it!

A more serious point is the degree of interplay which exists

between remarks on funding and remarks concerning the gravity of

the discrepancy.   It seems that scientists do not make any firm

separation between the two areas.   The need to fund new experiments

is an integral component of the argument over the assessment of

significance.

It would appear from the discussion so far that the assessment

of significance resides in a far wider set of concerns than a

first inspection of Fig. (1) would suggest. The significance or

not of the discrepancy is to be found not in the data shown there,

but in the interpretative apparatus brought to bear upon the data.

The three-sigma argument is part of this interpretative apparatus.

If the three-sigma argument is to be compelling then other scientists

must share the same set of wider concerns which go into this

particular interpretation of Figure (1). If scientists have

different concerns then, with sufficient interpretative skill, they

may be able to produce rival interpretations - interpretations which

will indicate the defeasibility of the three-sigma argument. It is

to such rival interpretations that we now turn.

Counters to the Three-Sigma Argument

It has already been mentioned that most respondents regarded

the solar-neutrino problem to be a very serious discrepancy indeed.

Naturally such scientists were concerned to question the legitimacy

of the three-sigma argument. Most were aware of the argument

and recognised that three sigmas was a widely applied level of

significance. Most were happy to acknowledge the validity of

this level of significance. Dissent is, however, possible. For

instance, the following comment was made by the eminent stellar-

evolution theorist, Martin Schwarzschild, at the recent U.S. solar-
                              7
neutrino conference:

> I myself, in my personal life, would take much higher risks
> than are implied by two sigmas. If I am really after
> something, I take even a one-sigma risk. The modern habit
> of requiring three sigmas, I don't understand at all - it seems
> to be the habit of old men with bad experiences.

Clearly, given this view the three-sigma argument is likely to

have little appeal. Most respondents, however, did not favour

this means of avoiding the argument - they were prepared to accept

that three sigmas was the appropriate significance level. They

gave other reasons as to why the three-sigma argument did not

show the solar-neutrino problem to be a non-significant discrepancy.

Two general ways around the argument emerged. One view was that the

solar-neutrino problem was exempt from the three-sigma rule.

The other main way to avoid the argument was the view that the

discrepancy was actually larger than three sigmas anyway. I will

look at each of these arguments in turn.

The Solar Neutrino Problem is Exempt

Several reasons were given as to why it was inappropriate to

apply the three-sigma argument to this case. In general the

unique character of the experiment was cited. It was felt that

the importance of this particular experiment warranted further

investigation of the discrepancy, despite it being only three

sigmas in magnitude. One reason given was that measurements

made on the Sun were of special relevance:

> I wouldn't in the least say at this stage 'Oh well, there
> is no discrepancy. What the heck, a factor of three, forget
> it'. Not at all. I think that the Sun is a star of great
> importance. It's a prime astrophysical target and we had
> better be able to understand it in all the gruesome detail
> we can manage.

One respondent felt that the unique nature of the experiment required

a novel interpretation of systematic errors which meant that the

three-sigma argument should not be applied. As he told me:

> In a fundamental experiment like this one you should not
> apply the three-sigma rule because the whole question is
> what is the systematic item that we are overlooking. And in
> that sense if one says that at the three-sigma level the
> discrepancy is only marginally significant, I think all one
> says is that there is something unrecognized. In this case
> the unrecognized systematic error is the whole scientific
> point.

In other words, because the experiment is of such fundamental import,

the resolution of the discrepancy is in itself bound to be of

significance. Therefore this is a significant discrepancy!

This view contrasts with that held by those who argue that

the discrepancy is non-significant. For them the explanation in

terms of systematic errors is likely to be fairly mundane and

not worth pursuing in detail.   They stress the non-uniqueness of

the situation by comparing this experiment with other cases where

three-sigma discrepancies existed but which later went away.

It can be seen that the three-sigma argument does nothing

to resolve the debate over the significance of the discrepancy, for

assessments of significance seem to depend on the importance

attached to the experiment in the first place.   What are trivial

systematic errors for one scientist become the whole scientific

point of the enterprise for another.

The Discrepancy is Larger than Three Sigmas

The thrust of this argument was centred on the theoretical

prediction of 4.7 SNUs. As has been mentioned already the error

to be attached to this number is a matter of some contention. Most

respondents treated the error bars as defining a possible range

of uncertainty in which the theoretical value could lie, but did

not regard them to be normal one-sigma error bars.   They reasoned

that it was actually more likely that the true theoretical value

was larger than 4.7 SNUs. In other words, the error bars had a

skewed probability distribution that made a larger value than

4.7 SNUs more probable.   This interpretation of the error was

basdd on the history of the theoretical prediction. The variation

over the years is shown in Figure (2). They felt that the downward

tendency in the predictions, since the first experimental results

were available, was caused by theorists 'pushing' their theory

to the limit in order to try and bring agreement with the experiment.

An 'unbiased' theoretical value would actually be larger than

4.7 SNUs, and thus give a larger discrepancy than three sigmas.
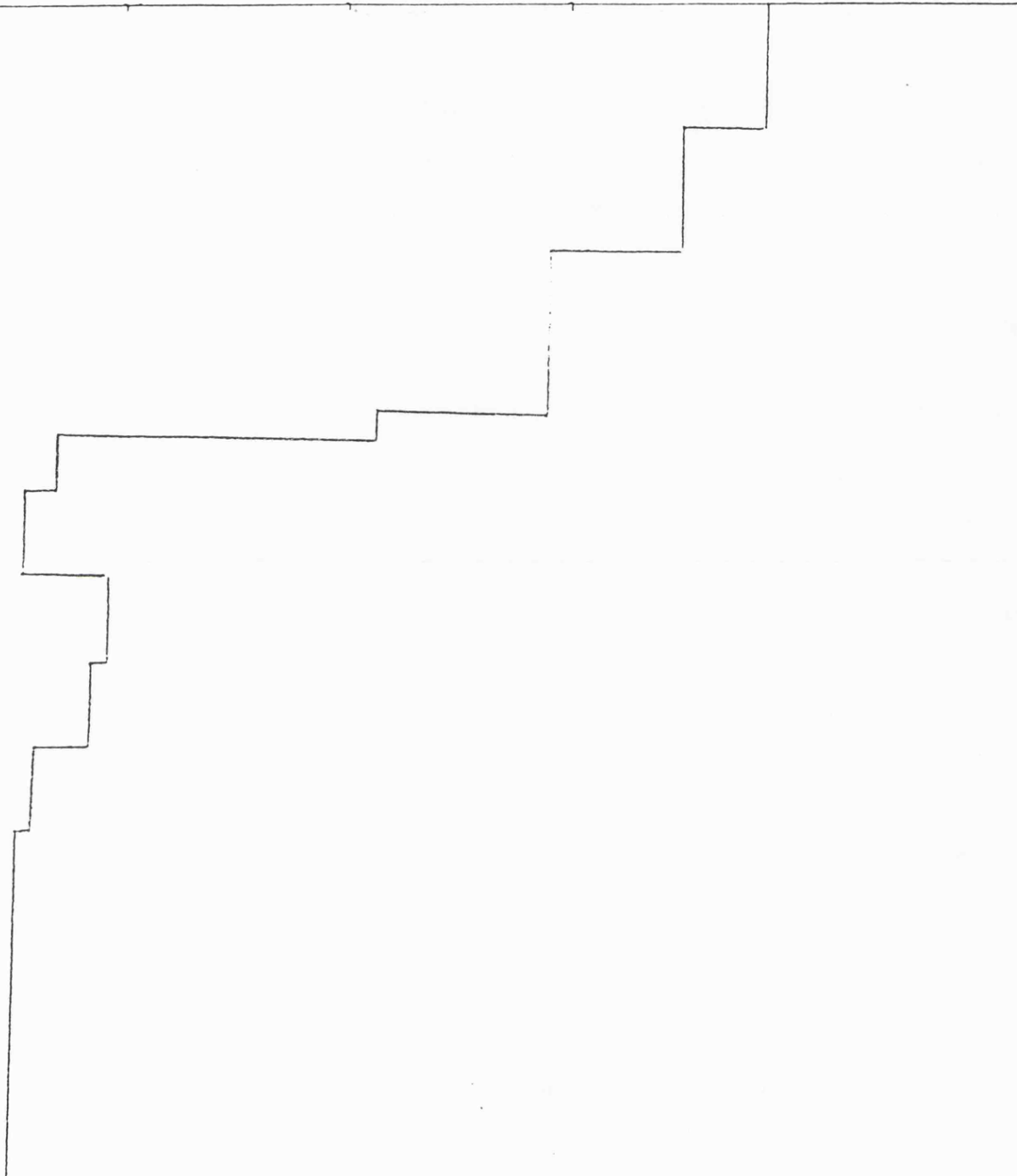
The following comment reflects this possibility:

FIGURE (2) VARIATION IN THEORETICAL PREDICTION OVER TIME

> People when they push things, they push them in one direction.
> So that while now I think you could get a number which is
> down to 3.5 or something at the one percent confidence level,
> you could also get a number at the one percent confidence
> level that would be around nine.

The view that the theoretical value is more likely to lie above

4.7 SNUs provides an effective counter to the three-sigma argument:

> You can see a genuine discrepancy between the theory and the
> data...The theoretical result [4.7 SNUs]... is the lower bound...
> You can easily push the models to push it up.

A related view was that the rate of decrease of the theoretical

prediction over the years had got less and it had now reached a

point where it was starting to level out or asymptote. For this

reason it was felt to be unlikely that the gap would decrease

sufficiently to solve the problem.   The remaining gap was therefore

that much more significant.   The following comments reveal such

an attitude:

> The slope is decreasing, the rate of change with time...It's
> now beginning to reach an asymptotic limit.

> Well the thing is these things always asymptote...The chances
> that there is some sleeper [systematic error] that's going
> to change things...are pretty small.

These responses to the three-sigma argument further illustrate

the complexity of the issue.   Factors such as the motivations of

scientists (the theorists' pushing their theories to agree with

the experiment), the history of this problem, and generalisations

drawn from the history of other problems(the asymptotic shape of

theoretical-prediction curves over time), are all part of the

assessment of significance and the evaluation of the three-sigma

argument. What at first sight might seem to be a narrow technical

issue capable of resolution by statistics has been shown to depend

on a whole web of wider assumptions and interpretations.   If the

appropriate interpretation is made, the three-sigma argument can

be avoided. The argument does not force the conclusion that the

discrepancy is non-significant - it is a matter for negotiation.

Random Errors

In the final section of this paper I return again to the issue

of random errors. The point was made earlier that it is difficult

to separate comments referring to the possibility of systematic

error from those referring to random error. However, in some

cases respondents's remarks did seem to be addressed exclusively

towards random error. As random errors can be treated by probability

theory, it might be thought that the assessment of the discrepancy

in terms of random error alone is a more straightfoward business.

From the comments I received it did not seem that this was so.

Several respondents felt that the discrepancy shown in Fig.

(1) could not arise from random effects. Typical comments were:

> The chance statistically that Davis would get 4.7 SNUs...
> is very small.

> I think the formal probability of an error still being there
> that exceeds three sigmas is ridiculously small. I do not
> think that that is really what we scientists proceed on.

However, not everyone agreed that three sigmas were enough to

rule out the possibility of random effects. As one respondent

told me:

> Once the random error gets four or five sigmas then experience
> shows it's probably correct, provided there are no systematic
> errors...If that [Fig. (1)] were five or six sigmas..I would
> say that's a clear-cut discrepancy.

As well as possible differences over the appropriate level of

significance to take, there is also the problem of how much data

is needed to make a discrepancy significant. As one respondent

commented:

> What a three-sigma discrepancy means depends on the amount
> of data you've got. If you have got three data points and

> you formally calculate sigma and you are three sigmas away,
> so what?   What does that mean? If you have got a million
> data points and you are three sigmas away, then you're a
> long way away. If you've got thirty five data points and
> you're a long way away, I'm not sure what that means...
> it's intermediate.

This comment would seem to touch on a problem which has arisen with the use of significance tests in general.[8] If a sufficiently large sample of a population is taken it is always possible to boost the significance of a small effect (as in statistical demonstrations of purported ESP effects). This means the sample size (or in this case the amount of experimental data) can be a key issue.

Another issue which has arisen is what the appropriate statistical analysis for data from a low-counting experiment such as this should be. It seems there are several alternative statistical procedures available which give slightly differing results. For instance, a Bayesian analysis of the data has produced an experimental value of 2.2 ± 0.3 SNUs - which means the size of the discrepancy is diminished.[9]

Questions of statistical nuance apply to the analysis of the theoretical error as well as the experimental error. The possibilities of producing different sizes of theoretical error according to the statistical assumptions employed are indicated in the following pair of comments.   In both cases it is felt that the errors on the parameters fed into the theoretical calculation could be combining in a non-Gaussian way. However, the first comment implies that this would lessen the severity of the discrepancy, whilst the second comment indicates that such an analysis would make the discrepancy worse!

> If the errors were conspiring rather than adding in a
> Gaussian way...Things like that do happen...I would
> suspect that a factor of three is not really a bad miss.

> I think the error in the predicted neutrino flux at the
> earth is not at all Gaussian....i.e., I think there is still
> a real problem even though formally you might now say the
> standard model is 4.5 plus or minus 1.5; there is likely
> still a real disagreement with the observations even though
> it sort of doesn't look like it.[10]

It would seem that the fine details of the statistical arguments

concerning the random errors on both the theoretical and experimental

values are far from being settled.

Respondents in general tended to regard the above questions

of statistical nuance as being rather beside the point. A not

atypical attitude was the following:

> These statistical arguments are very difficult to assess.
> You can calculate probabilities, but the probability depends
> very much on the question you ask.

This particular respondent felt that unless you could actually see

an effect upon visual inspection of the data, it probably was not

worth taking seriously. As he told me:

> Basically you can use statistics to measure things but if
> you want to be able to know whether a thing exists, you
> have to be able to look at it and see it basically...The
> best probabilists and statisticians in this university...
> have this attitude.

In the case of the solar-neutrino discrepancy, he felt that Fig. (1)

showed a clear discrepancy without any need to apply statistical

argument:

> You can look at these formal error bars [on the experimental
> data]...OK and the theory is up here...There's only one
> [experimental] point up there. How can these [experimental]
> numbers represent what's up there? No way whatever, they're
> all down there. So either there's a systematic error or there's
> a discrepancy.

Statistical issues were considered to be beside the point by many

respondents.   It was felt that if the existence of the solar-

neutrino discrepancy depended on the details of statistics then it

was not worth taking seriously anyway!

Of course, if the demonstration of the discrepancy depends more upon the visual impact of data, rather than upon statistical argument, then the question arises as to whether data can be presented in such a way as to exaggerate or diminish the appearance of discrepancy (the choice of scales will be of some importance). This, in itself, is a fascinating issue but not one which can be pursued further here.

Summary

The message of this paper has been that statistical argument does not necessarily provide a means of settling the problem of certainty in science.[11] Statistical arguments, like other arguments, are a matter for negotiation. In particular, we have seen that the question of the significance of the solar-neutrino problem has not been settled by the appeal to statistical argument. The assessment of significance in this case seems to have been a thoroughly social process in the sense that different perceptions and interpretations of significance are to be had. Of course, just because statistical arguments are defeasible in this case does not mean that such arguments cannot be used in a routine way in the rest of science. One suspects that in most areas the interpretative license available in solar-neutrino science does not exist. Agreement over statistical matters is likely to be reached in most sciences - and will presumably be reached eventually in this area too.

One final implication of this paper lies in the social sciences.[12] All too often the attitude taken towards statistics there is that they can always be used in the apply-the-rules, crank-the-handle, read-out-the-answer, 'next-problem-please', sense. Such an attitude is frequently founded on the belief that

statistical practice in a 'hard' science, such as physics, is always

like this. It is indeed ironic that it is within physics that a

case of the negotiated character of statistical inference should

come to light.

Notes

1. T.J. Pinch, 'The Sun Set: The Presentation of Certainty in Scientific Life', to be published in Social Studies of Science.

2. The wider study will be reported in my University of Bath PhD (forthcoming).

3. Useful reviews of the area are to be found in J.N. Bahcall and R.L. Sears, 'Solar Neutrinos', Annual Review of Astronomy and Astrophysics, 10, 1972, 25-41; and J.N. Bahcall and R. Davis, Jr., 'Solar Neutrinos: A Scientific Puzzle', Science, 191, 23 January 1976, 264-67.

4. The classic paper on social negotiation is, of course, H.M. Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', Sociology, 9, 1975, 205-24.

5. In all about forty scientists were interviewed.

6. G. Friedlander (ed.), Proceedings Informal Conference on Status and Future of Solar Neutrino Research, I. and II, Brookhaven National Laboratory, BNL 50879, 1978.

7. M. Schwarzschild in Friedlander op. cit., note 6, II, p. 275.

8. D.E. Morrison and R.E. Henkel (Eds.), The Significance Test Controversy, London: Butterworths, 1970.

9. A. Aurela, 'Progress Report on the Statistical Analysis of the Brookhaven Solar Neutrino Experiment', In Friedlander, op. cit., note 6, I, p. 36.

10. R. Rood in Friedlander, op. cit., note 6, I, p. 177.

11. For a sociological analysis of statistical theory itself see D. MacKenzie, 'Statistical Theory and Social Interests: A Case Study', Social Studies of Science, 8, 1978, 35-83.

12. For a provocative account of the use of statistics in the social sciences, see J. Irvine, I. Miles and J. Evans (eds.), Demystifying Social Statistics, London: Pluto Press, 1979.