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An Element of Controversy

The Life of Chlorine in Science, Medicine, Technology and War

Edited by Hasok Chang and Catherine Jackson

from research by undergraduate students at
University College London

British Society for the History of Science

2007

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Front cover illustration: Blueprint for a chlorine chamber for the cure of respiratory diseases. Reproduced by permission of Edward G. Miner Library, Rocherster, New York.

Back cover illustration: Chlorine gas, courtesy of the Department of Chemistry, University College London. Photo by Gretchen Siglar.

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Looking into the Core of the Sun

Christian Guy, Emma Goddard,
Emily Milner, Lisa Murch and Andrew B. Clegg

1. Introduction

In 1967, a chlorine-based neutrino detector went online deep inside the Homestake gold mine in the town of Lead, South Dakota in the United States. The Homestake apparatus was an unlikely contraption: a gigantic tank containing more than 400,000 liters of dry-cleaning fluid (perchloroethylene, C_2Cl_4) buried over 1,500 meters underground. The main aim of the experiment was to measure the flux of neutrinos from the sun, which were understood to be bombarding the earth in unimaginably great numbers[1] but mostly passing right through it without a trace, due to their very minimal interaction with ordinary matter. The design of this experiment was based on the theoretical prediction that a particular isotope of chlorine would be effective in capturing neutrinos.

The Homestake experiment was apparently successful in detecting the elusive neutrinos — so much so that physicists started speaking confidently of "looking directly into the core of the sun" by means of neutrinos. The light we see from the sun originates from its surface layers. Neutrinos, on the other hand, come from the core of the sun, so they can give us information about the nuclear reactions taking place in those hidden depths. The result of the Homestake experiment, however, presented a problem: not enough neutrinos were found, not nearly as many as predicted by the most trusted astrophysical theory of the sun and the basic physics underlying the mechanism of neutrino detection. This was dubbed the "solar neutrino problem".

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[1] The estimate given at the time by John Bahcall (1969, p. 28), one of the architects of the Homestake experiment, was $10^{11}/cm^2sec$.

Hasok Chang and Catherine Jackson, eds., An Element of Controversy: The Life of Chlorine in Science, Medicine, Technology and War *(British Society for the History of Science, 2007).*

As both the experiment and the basic physics involved were highly complex, it was not at all clear what had gone wrong. Some took the result as a refutation of the standard model of the sun; others distrusted the experimental setup; yet others reasoned that there must have been something inadequate about the physicists' previous understanding of neutrinos. This fascinating episode has also engaged the attention of scholars in science studies. Some sociologists of science, most notably Trevor Pinch, have taken the indeterminacy in this whole episode as further evidence that science was socially constructed and knowledge was only an outcome of social negotiations. Some philosophers of science, taking Dudley Shapere's lead, have taken the solar neutrino case as an instructive example to help them re-think the very concept of observation.

In this chapter we aim to provide a critical account of this episode which is broad-ranging yet concise, and detailed yet accessible to the non-specialist. In Section 2 we begin by retracing the history of neutrino detection, paying attention to not only the Homestake experiment itself but also the developments that came before and after it. We have drawn from contemporary and retrospective accounts given by the physicists themselves, as well as secondary sources, including Allan Franklin's comprehensive and detailed narrative in his 2000 text, *Are There Really Neutrinos?* In Section 3 we start with a review of Pinch's sociological treatment of this episode; and then we give an up-to-date account of how the solar neutrino problem has now been resolved, and examine the character of that resolution. In Section 4 we take a closer philosophical look at neutrino detection, to see whether and how it can help us in reaching a better understanding of the nature of observations in science.

2. The road to Homestake, and beyond

2.1. The elusive neutrino

For readers unfamiliar with the relevant physics, it is necessary to recap briefly the origin of the neutrino concept, and the initial difficulty of its detection. (Those already familiar with this history can skip to Section 2.2.) In the early twentieth century physicists began to gain an understanding of the structure of atomic nuclei. One key source of this knowledge was the spontaneous decay of radioactive atoms. Two common types of radioactive decay were recognized: alpha-decay, which produced α particles (helium nuclei), and beta-decay, which produced β particles (electrons). The common understanding was that the alpha and beta particles were contained in the nuclei before their disintegration, and simply departed the nuclei in the process of radioactive decay. This understanding seemed to work well enough for alpha-decay, but there was a serious difficulty regarding beta-decay: the "continuous beta-ray spectrum".[2] In alpha-decay, all the alpha particles from a given type of nucleus were emitted with the same characteristic energy. In contrast, there were unpredictable variations in the energies of beta particles (electrons), and this seemed to pose a threat to the long-held belief in the principle of energy conservation. There seemed to be no way of explaining where the more energetic electrons acquired their excess energy, or where that available energy went in cases when less energetic electrons were produced. The other products of beta-decay did not show any corresponding differences in their energies, so it was impossible to balance the equation of total energy before and after the beta-decay event.

Many attempts were made to resolve the mystery of beta-decay, ever since James Chadwick identified the continuous energy spectrum in 1914. Chadwick's observation was decisively confirmed by Charles D. Ellis and William A. Wooster in 1927, and the 1920s and 1930s saw much debate regarding the validity of the conservation laws. The problem became ever more interwoven with the development of theories of the structure of the atom and quantum mechanics. Prior to this time theories were generally disregarded if they did not obey the laws of conservation, but now it began to seem that the conservation laws might be rejected. In the late 1920s Niels Bohr became convinced that energy conservation was violated in beta-decay, and mentioned it on a number of occasions, for example in his Faraday Lecture given in 1930.[3]

What became dominant in the end, however, was an energy-conserving solution by Wolfgang Pauli (1900–1958), advanced in the same year as Bohr's Faraday Lecture. Pauli postulated the neutrino in an *ad hoc* manner, as an attempt to save the laws of the conservation. Pauli himself was not entirely certain about his suggestion, and its first announcement was made in a private letter in December 1930:

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[2] Peierls (1980), pp. 1–2. Franklin (2000, pp. 30–53) gives a detailed account of the history of beta-decay.

[3] Peierls (1980), p. 2.

> there could exist in the nucleus electrically neutral particles, which I shall call neutrons [later re-named "neutrinos"], which have spin 1/2 and satisfy the exclusion principle and which are further distinct from light-quanta in that they do not move with light velocity. The mass of the [neutrinos] should be of the same order of magnitude as the electron mass and in any case not larger than 0.01 times the proton mass. — The continuous β-spectrum would then become understandable from the assumption that in β-decay a [neutrino] is emitted along with the electron, in such a way that the sum of the energies of the [neutrino] and the electron is constant.[4]

Shortly after Pauli's suggestion, two other major advances led to the birth of the neutrino as we know it. In 1932 Chadwick found an electrically neutral particle with a mass very similar to that of the proton, and named the new particle "neutron", and demonstrated it to be a combination of a proton and an electron. However, the conservation laws once again seemed to be violated, as the mass of the neutron was greater than the masses of the proton and the electron combined. Enrico Fermi (1901–1954) entered the scene at that point. In his 1934 paper "A Tentative Theory of β-Decay", Fermi adapted Pauli's hypothetical neutral particle and incorporated it into what we now recognize as a very early version of the theory of weak interactions. To distinguish Pauli's particle from Chadwick's neutron, Fermi called the former "neutrino", which in Italian means the "little neutral one".[5] In Fermi's theory, beta-decay was a process in which a neutron (n) disintegrated into a proton (p), an electron (e^-) and a neutrino (ν):

$$n \rightarrow p + e^- + \nu$$

The theory was revolutionary for its time, as it postulated the *creation* of elementary particles.[6] It did solve the continuous energy problem: "By incorporating the neutrino, Fermi's theory preserved the laws of the conservation of energy, momentum and angular momentum."[7] Fermi's paper did much more than simply solving the energy-conservation problem. As Nickolas Solomey puts it:

> it is generally regarded as a turning point that would eventually usher in a new field of study within the broader discipline of nuclear and

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[4] See Peierls (1980), p. 3; the quotation is taken from Franklin (2000), p. 71.
[5] Peierls (1980), p. 5.
[6] See Pontecorvo (1980), p. 47, for further discussion.
[7] Franklin (2000), p. 77.

> particle physics. This new field was the study of the weak nuclear force; this force was first noticed in beta decay, and many advances in our understanding of it emerged from the study of the neutrino.[8]

Now, it was one thing to postulate a new particle theoretically, but quite another thing to show its existence experimentally. An experiment by Chadwick and T. D. Lee concluded that neutrinos had a negligible mass and no magnetic moment. That, together with lack of electric charge, implied that the only method by which a neutrino would react with anything else would be *inverse beta-decay*, first discussed by Hans Bethe and Rudolf Peierls in 1934.[9] The basic idea was rooted in an assumption of symmetry prevalent in modern physics, according to which any process that "goes forward in time must have an inverse, which can be described as the identical process going in reverse".[10] In beta-decay, a neutron disintegrates into a proton and other particles. In inverse beta-decay, a proton turns into a neutron, by combining with a neutrino; the reaction also produces a positron (the anti-particle of the electron):

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

(It turns out that the neutrino required in that reaction is an antineutrino, but that is not important at this stage of the story.) In a similar process, we can have a neutrino hitting a neutron, producing a proton and an electron:[11]

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

For many years physicists could not engineer inverse beta-decay, so they could only obtain very indirect evidence for the existence of the neutrino. As Rudolf Peierls put it:

> It seemed to us therefore that the most direct proof of the existence of the neutrino might have to be the observation of the recoil in β decay. If the momentum balance showed that the missing momentum correlated with the missing energy like the energy and momenutm of a particle, then this would be strong circumstantial evidence for the neutrino."[12]

8 Solomey (1997), p. 18.
9 See Pontecorvo (1980), p. 31.
10 Solomey (1997), p. 64.
11 See Arns (2001), pp. 315–316.
12 Peierls (1980), p. 6

2.2. The detection of neutrinos

Chlorine played a role not only in the Homestake experiment, but in two major preliminary steps leading up to Homestake. The first significant experiment explicitly designed to test the existence of neutrinos seems to have been that of H. Richard Crane and J. Halpern at the University of Michigan. Crane and Halpern submitted their report, titled "New experimental evidence for the existence of a neutrino", to the *Physical Review* in March 1938 (while Frederick Reines, the man often credited with first detecting the neutrino, was still an undergraduate). Crane and Halpern observed the beta-decay of a radioactive isotope of chlorine,[13] which they produced by putting a sample of salt (sodium chloride) in a cyclotron. The radioactive chlorine atoms were put into ethylene dichloride, which was in gaseous form and could be mixed into the water vapour contained inside a cloud chamber, in which the particle tracks were made visible.[14] This experiment was notable in that it measured not only the momentum of the electron ejected in beta-decay, but also the recoil momentum of the remaining nucleus, in each instance. As Crane and Halpern reported:

> This is the first experiment which has given any information at all regarding the momentum relations in the *individual* disintegration event. Although the results are of limited accuracy, they strongly indicate that momentum is not conserved between the electron and the nucleus alone. Hence the laws of momentum, as well as those of energy, indicate that a third particle participates in the disintegration.[15]

Crane and Halpern's results convincingly indicated the presence of a neutrino, although they were not able to give precise measurements of its momentum.

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[13] Crane and Halpern used ^{38}Cl in this experiment; see Section 1.4 for further explanation of isotopes and the technical notation for them.

[14] Crane and Halpern (1938), p. 790.

[15] Ibid., p. 789. Crane and Halpern had an ingenious method of overcoming the difficulty that the nucleus did not produce a track long enough to be seen and measured like the electron: "The nucleus will, however, produce a number of ion pairs concentrated in a very small region of space, and the number of ion pairs will be a function of the kinetic energy of the nucleus. It occurred to the authors that if these ions could be allowed to diffuse into a cluster several millimeters in diameter before the condensation were brought about, the individual droplets could be counted, and hence the kinetic energy of the nucleus estimated."

Shortly after Crane and Halpern's work, James Allen at Kansas State College demonstrated a different technique for measuring nuclear recoils. Allen's experiment, published in 1942, used a preparation consisting of a single layer of beryllium atoms on a platinum substrate. The beryllium underwent a transformation similar to beta-decay, turning into lithium and presumably emitting a neutrino.[16] Allen measured the energy of the lithium atoms jumping off the substrate following the reaction. His conclusion was similar to Crane and Halpern's findings:

> Unless some new mechanism for the removal of the energy produced in the Be^7 decay process can be discovered, it must be concluded that the recoils were caused by the emission of a neutrino of nearly zero rest mass.[17]

Another experiment worth mentioning was by C. W. Sherwin, military radar expert at the University of Illinois, who in 1948 studied properties of the beta-decay products of a phosphorus isotope.[18] Sherwin set up his apparatus so that a Geiger counter set off by a beta particle (electron) triggered an electron multiplier that measured the recoil energy of the decayed phosphorous nucleus. The angle between the Geiger counter and the electron multiplier was varied between 45˚ and 180˚. This apparatus was designed to catch and record recoiling nuclei for a variety of angles of recoil. The fact that *any* nuclei were caught at an angle less than 180˚ from that of the electron's trajectory showed that a third body was involved. At 180˚ the neutrino was emitted parallel to the electron, so the multiplier measured the whole component of its momentum; here a "striking" fit with the theory was obtained. Raymond Davis, who will feature prominently later in our story as the main architect of the Homestake experiment, did some recoil experiments of his own at Brookhaven, presumably after reading a review of the state of neutrino-detection work published by Crane in 1948. These were described fleetingly in a Brookhaven annual report, which teasingly says that the recoil "strongly pointed to the emission of a monoenergetic

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[16] The reaction here is $^7Be \rightarrow {}^7Li + \nu_e$; one of the protons in the beryllium nucleus captures an electron in its orbit, turning into a neutron and emitting a neutrino. This is an unusual case as there is no overall emission or absorption of an electron by the atom.
[17] Allen (1942), p. 697.
[18] Sherwin studied ^{32}P. See Crane (1948), pp. 286–287, for further details.

neutrino."[19] But the experiment was part of a classified research programme, and details were never published.

Inverse beta-decay induced under laboratory conditions paved the way to the next stage in the observation of neutrinos. This provided a more direct proof of the neutrino, as physicists were now using neutrinos as a tool for manipulating other entities, in a way that would satisfy Ian Hacking's "experimental realism".[20] The leaders in this development were Clyde L. Cowan and Frederick Reines of the Los Alamos Scientific Laboratory, the very same atomic-weapons facility where the Manhattan Project had been carried out in secret.[21] Reines believed that however attractive the neutrino was as an explanation for beta-decay, "the proof of its existence had to be derived from an observation at a location other than that at which the decay process occurred — the neutrino had to be observed in its free state to invert beta decay or otherwise interact with matter at a remote point."[22] Such an experiment required a localized source of a large flux of neutrinos, which was not something one could expect to have. However, during the Second World War, nuclear reactors were developed in tandem with atomic bombs, and now there were controlled nuclear reactions which produced neutrinos. As scientists started to turn their attention away from bomb design at the end of the war, Cowan and Reines launched "Operation Poltergeist", aimed at a direct detection of the neutrino.[23] The initial plan, for which Reines had actually got authorization, was to use an atomic bomb as a source of neutrinos for the experiment.[24] Perhaps more sensibly, the Reines and Cowan team later settled on using neutrinos from a nuclear reactor instead. The first experiments were carried out at the Hanford nuclear facility in Washington State in 1953, and later the experiment was moved to the Savannah River Plant of the U.S. Atomic Energy Commission in 1955.

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[19] Brookhaven National Laboratories (1951).
[20] Hacking (1983), esp. chapter 16.
[21] See Arns (2001), and Franklin (2000), pp. 165–179, for a detailed account of Reines and Cowan's work; see Galison (1997), pp. 460–463, for a succinct summary.
[22] Reines (1980), p. 12.
[23] Solomey (1997), p. 64.
[24] Arns (2001), p. 317.

The essential part of the apparatus for these experiments was a large tank of liquid scintillator containing a cadmium chloride solution, which emitted short light pulses as the neutrinos interacted with the protons contained in it. The reaction was that of an "anti-neutrino from fission products in the reactor . . . incident on a water target containing cadmium chloride."[25] The products of the reaction were a positron and a neutron. The positron annihilated an electron, producing a distinctive burst of gamma radiation; the neutron, after a short delay, drifted over to a cadmium atom and became captured, releasing more gamma rays. The data were two pulses on an oscilloscope produced by the scintillation counter's detection of the gamma rays. The results were encouraging, and the experimental techniques and the accuracy attained were continually improved. When different trials were made with different rates of neutrino flux, the flux was shown to be proportional to the number of signals detected. The experiment also checked that all the pulses represented "honest-to-god" particles by using known positron and neutron sources. A telegraph was sent to Pauli on 14 June 1956: "We are happy to inform you that we have definitely detected neutrinos from fission fragments by observing inverse beta decay of protons."[26]

2.3. Homestake: using neutrinos to "see" inside the sun

After some time there was a general consensus among physicists that the Reines–Cowan experiment had observed neutrinos.[27] As scientists felt more at ease with the concept of the neutrino, they started to consider how they could use it to observe other phenomena. One obvious promise was based on the very elusiveness of the neutrino, as explained already: since neutrinos did not react much with anything, most of them would be able to escape from the inner depths of the sun and bring out some vital information about what goes on in there — if we could capture them.

The nuclear physics of stars was in a promising yet nascent state when the experimental promise of the neutrino was becoming clear. Arthur Stanley Eddington became the "first astrophysicist", with his

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[25] Reines (1980), p. 17.
[26] Ibid., p. 25; also quoted in Arns (2001), p. 324.
[27] Arns (2001, pp. 327ff) tells a more nuanced story about the reception of the Reines–Cowan work.

pioneering investigations into stellar structure.[28] Eddington was greatly helped by his collaboration with Francis W. Aston, whose mass-spectrometer enabled an extremely precise determination of the masses of atoms. Eddington became sure that the generation of heat within a star was from the fusion of atomic nuclei. Aston's measurement had shown that a helium nucleus had a mass that was less than the combined mass of four hydrogen nuclei, although it was understood that the fusion of the latter would make the former. Eddington deduced from his knowledge of relativity that it was likely that this missing mass would be converted into energy, providing fuel for the stars. Subsequently, much more sophisticated theories regarding such fusion mechanisms were developed by Hans Bethe and others. The most significant reactions include the proton–proton (pp) chain, and the carbon–nitrogen–oxygen (CNO) cycle. Both of those processes lead to the formulation of helium, which forms a core at the centre of the star. The helium core would eventually ignite, forming heavier elements.

By the end of the Second World War, theories regarding stellar evolution and the methods of energy-production in stars were firmly set in the lexicon of science; however, there was still no conclusive evidence by the end of the 1950s that these mechanisms were accurate. While scientists had been very successful at the building of fission and fusion bombs, they could not get much information about the stellar interiors. They only had reasonable confirmation that the proposed reaction mechanisms had approximately the right amount of energy. Only the neutrino offered a potential means of observation that would yield direct information about the reactions taking place at the cores of stars.

Many of the stellar nuclear reactions have a neutrino as one of their end-products. As John Bahcall, a nuclear astrophysicist at the Kellogg Radiation Laboratory at Caltech, put it:

> Of the particles released by the hypothetical thermonuclear reactions in the solar interior, only one species has the ability to penetrate from the center of the sun to the surface (a distance of some 400,000 miles) and escape into space: the neutrino. The massless particle, which travels with the speed of light, is so unreactive that only one in every 100 billion created in the solar furnace is stopped or deflected on its flight

[28] Gribbin (1998), p. 115.

> to the sun's surface. Thus neutrinos offer us the possibility of "seeing" into the solar interior because they alone escape directly into space.[29]

The quantity and energies of the neutrinos created by the sun were considered to be dependent upon the types of elements fusing in the nuclear reactions. Thus by observing neutrinos, it would be possible to test theories concerning not only the mechanisms of energy production but also the proportions in which the various mechanisms contribute to the total energy of the sun. The concept of the neutrino telescope was born.

Crucial in the observation of solar neutrinos was the work of the Italian physicist Bruno M. Pontecorvo (1913–1993), a former student of Fermi and a participant in the Manhattan Project, later notorious for his controversial move to the Soviet Union. Pontecorvo suggested that chlorine-37, the heavier of the two common isotopes of chlorine, might capture a neutrino and transmutate to form a radioactive isotope of argon by inverse beta-decay.[30] This reaction requires some explanation.

The nucleus of chlorine-37 contains 17 protons and 20 neutrons. The chemical properties of the element are determined by its atomic number, namely the number of protons (equal to the number of electrons in the neutral state of the atom), 17 in this case. The mass of the atom is roughly proportional to the total number of protons and neutrons, in this case 37. The term "isotopes" designates chemically identical atoms that are different in mass, possessing the same number of protons and electrons (atomic number) but different numbers of neutrons.[31] For brevity physicists use the notation in the form of ^{37}Cl (sometimes written as Cl^{37}); for those of us who cannot remember our periodic table, a fuller notation is in the form of $^{37}Cl_{17}$, with the atomic number indicated as well.

When one of the neutrons in the ^{37}Cl nucleus interacts with a neutrino, it turns into a proton and emits an electron, by inverse beta-decay. So we end up with a nucleus consisting of 18 protons and 19 neutrons, which is argon-37, or, $^{37}Ar_{18}$. This is an instance of the second type of inverse beta-decay noted in Section 2.1 above:

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[29] Bahcall (1969), p. 29.

[30] Solomey (1997), p. 18.

[31] See Chapter 3, Section 2.3, on the history of isotopes, including a discussion of Aston's work. As explained there, the more common isotope of chlorine on earth is chlorine-35.

$$n + \nu_e \rightarrow p + e^-.$$

When that happens within a ^{37}Cl nucleus, we have:

$$^{37}Cl_{17} + \nu_e \rightarrow {}^{37}Ar_{18} + e^-.$$

In general, Pontecorvo believed that a neutrino could interact with an atomic nucleus and turn it into a different chemical element by inverse beta-decay. But why was chlorine chosen, from all the different possibilities? There were several reasons.[32] First of all, neutrino-capture by ^{37}Cl to form ^{37}Ar has a sufficiently low threshold energy. A given type of inverse beta-decay will only occur if the incoming neutrinos possess energy over a certain value. Solar neutrinos generally have low energies, predominantly below 1 mega electron-volt (MeV),[33] so their detection requires a target material with a low energy-threshold. The ^{37}Cl reaction has the threshold energy of 0.81 MeV, which is just low enough to capture neutrinos generated in the reaction that produces beryllium-7 in the sun. Moreover, the neutrino-capture rate by ^{37}Cl was theoretically known with some accuracy. There were also more pragmatic concerns. The main product of the reaction, ^{37}Ar, is an inert gas and easily extracted. It is radioactive in the most convenient way: in decaying it emits a single gamma ray, easy to observe and count, and besides has a very convenient half-life (35 days), not too short and not too long. To top all the advantages, chlorine was cheap and abundant, and about one-quarter of naturally occurring chlorine on earth is ^{37}Cl.

Pontecorvo's description of his proposed experiment sounds remarkably similar to the Homestake experiment:

> The experiment with chlorine, for example, would consist in irradiating with neutrinos a large volume of chlorine or carbon tetrachloride for a time of order of one month, and extracting the radioactive Ar^{37} from such a volume by boiling. The radioactive argon would be introduced inside a small counter; the counter efficiency is close to 100 percent. . . . .[34]

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[32] Gary Royle, private communication to Emma Goddard, 10 March 2003.

[33] An electron-volt (eV) is the amount of energy gained by an electron in being accelerated through a potential difference of 1 volt; it is equal to 1.60 x 10-19 joule. And 1 MeV is 1 million eV.

[34] Pontecorvo, quoted in Franklin (2000), p. 251. It should be noted that Pontecorvo himself was not satisfied that there was sufficient scientific evidence on the existence of the neutrino. Even after both the Reines–Cowan and Davis–Bahcall experiment,

Raymond Davis, a radiochemist at Brookhaven National Laboratory, followed the suggestion from Pontecorvo and the "careful unpublished feasibility study of Louis Alvarez".[35] In the 1950s Davis built what amounts to a prototype of the Homestake apparatus. This experiment used 3,800 liters of carbon tetrachloride (CCl_4) nineteen feet underground. The upper limit of the flux of neutrinos was recorded to be 40,000 SNU (SNU stands for "solar neutrino unit", 1 SNU indicating 1 capture per 10^{36} target atoms per second). The results were greatly incompatible with theoretical predictions, and met with scathing reviews.[36]

For the Homestake experiment Davis teamed up with John Bahcall. The latter was an expert on beta-decay rates in stellar interiors, and was excited by the prospect of a test of the theory of nuclear synthesis in stars.[37] Bahcall and Davis in 1964 gave a new theoretical analysis, which supported the idea that an experiment such as Homestake would be a feasible test for solar models. As Bahcall recalls: "we never discussed the possibility of using neutrinos to learn about particle physics"; the only motivation they had was "to see into the interior of a star and thus verify directly the hypothesis of nuclear energy generation in stars."[38] Bahcall described the rationale for the experiment as follows:

> The principal energy source for main-sequence stars like the sun is believed to be the fusion, in the deep interior of the star, of four protons to form an alpha particle…. No *direct* evidence for the existence of nuclear reactions in the interior of stars has yet been obtained because the mean free path for photons emitted in the center of a star is typically less the 10^{-10} of the radius of the star. Only neutrinos, with their extremely small interaction cross sections, can enable us to *see into the interior of a star* and thus verify directly the hypothesis of nuclear energy generation in stars.[39]

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many physicists were still not convinced that there was any conclusive evidence regarding the existence of the neutrino.

35 Bahcall (2001), p. 10.

36 One referee remarked: "Any experiment such as this, which does not have the requisite sensitivity, really has no bearing on the existence of neutrinos"; quoted in Franklin (2000), p. 251.

37 Pinch (1986), pp. 74–76.

38 Bahcall (2001), p. 11.

39 Bahcall, quoted in Franklin (2000), p. 253; emphases original.

The construction of the Homestake apparatus began in 1964 and finished in 1967. Davis used perchloroethylene (C_2Cl_4) this time, just as effective as carbon tetrachloride (CCl_4) but cheaper (and less toxic). Four hundred thousand liters of CCl_4 was placed in a tank, one thousand and five hundred meters underground. The apparatus was placed so far underground in an attempt to eliminate background radiation, such as muons, which were also known to cause inverse beta-decays. The apparatus was also made thoroughly airtight, to avoid contamination. The extraction of the argon was a relatively simple process. As Bahcall and Davis described it:

> The neutrino capture process produces an ^{37}Ar atom with sufficient recoil energy to break free of the parent perchloroethylene molecule and penetrate the surrounding liquid, where it reaches thermal equilibrium. Initially the recoiling argon atom is ionized. As it slows down, it will extract electrons from a neighboring molecule and become a neutral argon atom. A neutral argon atom behaves as dissolved argon, which can be removed easily from the liquid by purging with helium gas. These chemical processes are of crucial importance to the operation of the detector.[40]

The radioactive argon was allowed to build up over a period of a few weeks, and was then periodically removed from the tank. Helium was circulated through the tank to sweep out the argon atoms.[41]

Since ^{37}Ar is radioactive, the number of its atoms can be counted exactly. As Bahcall explained:

> The argon isotope produced by neutrino capture is unstable and reverts to ^{37}Cl by capturing one of its own orbital electrons. Fifty percent of a sample of ^{37}Ar atoms will undergo such a transformation in about 35 days. The decay process shakes loose a low-energy electron from the argon atom, and this electron can be detected by counters placed around the sample.[42]

As each interaction of a neutrino with a ^{37}Cl atom creates an atom of ^{37}Ar, counting the number of the ^{37}Ar atoms produced also gives the number of neutrinos captured. Combining that information with the probability of neutrino-capture, one can calculate the flux of neutrinos.

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[40] Bahcall and Davis (1976), p. 265.

[41] For a summary of the argon-isolation process, see Franklin (2000), pp. 255–256. The recovery methods of argon were tested in two different ways. Both mechanisms showed over 95% accuracy.

[42] Bahcall (1969), p. 33.

2.4. The solar neutrino problem

The first results from the Homestake experiment were published in 1968, and the observation was repeated many times. Wolfgang Hampel summarizes the results of the 35 runs of the Homestake experiment between 1970 and 1979:

> There is some scatter in the data which has led to the suggestion that the capture rate may be varying in time, but the data are fully consistent with a constant production rate, the scatter being due to statistical fluctuations only. The average ^{37}Ar production rate is 0.50 ± 0.06 atoms per day. If the cosmic ray muon contribution, 0.08 ± 0.03 atoms per day, is subtracted . . . . then the remaining rate amounts to 2.2 ± 0.4 SNU. This is the so-called "rate above known backgrounds" which may (or may not) be attributed to solar neutrinos.[43]

The predicted result from the standard solar model theory was 7.8 ± 1.5 SNU. That is to say, there was a significant discrepancy between the observed and the predicted values. And this discrepancy was confirmed by further experiments. According to Bahcall and Davis's retrospective appraisal published in 2000:

> The initial results have been remarkably robust; the conflict between chlorine measurements and the standard solar model predictions has lasted over three decades. The main improvement has been in the slow reduction of the uncertainties in both the experiment and the theory.[44]

In Bahcall's reckoning, the numbers from various experiments settled down to the following: "assuming nothing happens to the neutrinos after they are created, the measured rates range from 33 ± 5% of the calculated rate (for chlorine) to 58 ± 7%."[45]

Trevor Pinch relates from his interviews with Bahcall:

> "when he first got an indication that Davis' result was out of line with his prediction, he fought long and hard to try and work out what had gone wrong. For some time Bahcall maintained that there was no serious contradiction between theory and experiment and, it was not until 1970, that he was finally prepared to state that there was such a conflict"[46]

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[43] Hampel (1982), pp. 62–63.
[44] Bahcall and Davis (2000), p. 3 (online version).
[45] Bahcall (2001), p. 11.
[46] Pinch (1986), p. 130.

By 1976 Bahcall and Davis were both convinced that the Homestake results were stable, and that it constituted "a scientific puzzle":

> For the past 15 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 (observed on the earth as sunlight). All of us have been surprised by the results: there is a large, unexplained disagreement between observation and the 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.[47]

Michael Rowan-Robinson reports that afterwards four other neutrino experiments have "all detected electron-neutrinos from the Sun at a rate significantly lower than predicted by the standard solar model, typically by a factor of 2."[48]

In other words, the solar neutrino problem was there to stay, and it had to be dealt with. What could have gone wrong? As Solomey puts it: "There were three possible explanations for the discrepancy: the experiment was incorrectly counting the neutrino rate, the neutrino had some still-hidden mysteries, or our Sun does not function as the solar model predicts."[49]

Initially it may have seemed that the solar model was the weakest link in the chain. However, it withstood some serious critical examination. This came chiefly in the area of helioseismology, which is the "science [of] studying wave oscillations in the Sun."[50] It basically applies to the sun the seismological models developed for investigating tremors on the earth. A study was made of the movement of gases at the sun's surface; a recording of shifts in the spectral lines induced by Doppler shift revealed that these surface vibrations could be described in terms of sound waves moving through the sun.[51] The sun can be thought of as a resonant sphere which, when perturbed, oscillates at frequencies corresponding to its normal modes of oscillation; the predicted modes of the

[47] Bahcall and Davis (1976), p. 264.
[48] Rowan-Robinson (1996), p. 152.
[49] Solomey (1997), p. 73.
[50] "Helioseismology", http://soi.stanford.edu/results/heliowhat.html (accessed most recently on 7 January 2007).
[51] Matthews (1990).

oscillations of the sun are worked out using models of the solar interior.[52] Thus by observing changes in the surface, size and brightness of the sun astronomers can deduce what is happening inside the sun, to the extent that they are now able to discover internal variations in temperature, composition and rotation! When the helioseismological analysis was carried out, it only served to lend further support to the Standard Solar Model. As Bahcall put it in his retrospective account in 2001:

> Could the solar model calculations be wrong by enough to explain the discrepancies between predictions and measurements . . . ? Helioseismology, which confirms predictions of the standard solar model to high precision, suggests that the answer is 'No.'[53]

This result tended to shift the blame for the solar neutrino problem to either the basic physics of the neutrino or the experimental arrangements for neutrino detection.

If the solar model was not to blame, perhaps it was more believable that there was something wrong with the new and complicated Homestake neutrino-detection method, rather than the fundamental theory of particle physics. Therefore it is not surprising that physicists attempted several different experiments after Homestake in order to confirm the solar neutrino deficit. Here we describe four of the most important: Kamiokande II, SAGE, GALLEX and BOREXINO.[54]

Kamiokande II operated from 1990 to 1996 in the Kamioka mine in Japan.[55] It observed neutrinos by tracking their scattering with electrons in water. If the incoming neutrino imparted sufficient energy to the scattered electron, the electron emitted Cerenkov radiation,[56] which was carefully recorded in the Kamiokande apparatus. Kamiokande II was targeted specifically at solar neutrinos produced from the third neutrino-producing reaction in the proton–proton chain ("pp3"):[57]

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[52] Davies (1998), p. 120.
[53] Bahcall (2001), p. 11.
[54] There are many sources to consult on these experiments, but Filippini (2002) provides a convenient and accessible summary.
[55] Franklin (2000), pp. 272–281, gives a good summary of the Kamiokande experiments (which he spells "Kamiokonde" there).
[56] Radiation emitted by particles travelling faster than the speed that light has in a material medium, named after the Soviet physicist Pavel Alexeyevich Cerenkov (or Cherenkov).
[57] Joseph Lazio, "What is the Solar Neutrino Problem?", Internet FAQ Archives, http://www.faqs.org/faqs/astronomy/faq/part5/section-5.html (accessed initially on 11

$$^{8}B_{5} \rightarrow {}^{8}Be_{4} + e^{+} + \nu_{e}, \qquad \text{(pp3)}$$

in which a proton in boron-8 beta-decays into a neutron, a positron and a neutrino. Kamiokande II had a threshold energy of 7.5 MeV, so it was not able to detect solar neutrinos originating from other reactions. Results from Kamiokande II confirmed the solar neutrino problem. An improved version of the experiment, called Super-Kamiokande, produced similar results from 1996 until it was severely damaged in an accident in 2001.

Kamiokande II was also able to tell when and from which direction the neutrinos arrived, in addition to the energy spectrum of electrons they produced.[58] The broader potential of such a detector for astronomy was recognized when it recorded a blast of neutrinos hitting the earth from a supernova explosion several thousand light years away in another galaxy. Practically all the neutrinos passed straight through the earth but a few (just 19 out of a total of one billion billion) were stopped by the detector.[59] The witnessing of this event was described as being "like pointing a conventional telescope at the ground and seeing a star on the far side of the world".[60]

The SAGE (1988–92) and GALLEX (1991–97) experiments were constructed in order to detect the solar neutrinos that other experiments missed out. Although chlorine-37 has a low energy threshold it is not sufficiently low to catch the most abundant types of solar neutrinos. Gallium-71, used in both GALLEX ("GALLium EXperiment") and SAGE ("Soviet–American Gallium Experiment"), has a threshold energy of only 0.237 MeV.[61] The process used in both detectors is the following:

$$^{71}Ga_{31} + \nu_{e} \rightarrow {}^{71}Ge_{32} + e^{-},$$

in which gallium-71 turns into germanium-71 by inverse beta-decay. Crucially, the low threshold of this reaction enables the detection of neutrinos produced in the first reaction in the proton–proton chain ("pp1"), whose average energy is 0.26 MeV:

November 2003, most recently on 6 January 2007). See also Bahcall and Ostriker (1997), p. 196.

58 Franklin (2000), pp. 272–273.

59 Padamsee (2003), p. 459.

60 Chown (1994), p. 40.

61 Gary Royle, private communication to Emma Goddard, 10 March 2003.

$$p + p \rightarrow D + e^{+} + \nu_e, \qquad \text{(pp1)}$$

where *D* indicates a deuterium (heavy hydrogen) nucleus, consisting of a proton and a neutron.[62] To illustrate how much more inclusive the GALLEX and SAGE experiments are in comparison with the other experiments, consider that the pp3 chain observed by Kamiokande II occurs in only 0.02% of the solar nuclear reactions involved in converting hydrogen to helium.[63]

The results from SAGE, in the North Caucasus Mountains of Russia, and GALLEX, in the Abruzzo region of central Italy, confirmed the solar neutrino deficit found in previous experiments. However, these results were rather inconclusive, in terms of providing additional hard evidence. As Bahcall put it: “If you believed new physics was required before GALLEX then you are not disappointed. If you did not believe new physics was required before GALLEX then you are still not convinced.” The SAGE results were equally ambiguous; the size of the error on the first set of results was described as “so large that it was possible the experiment had detected no neutrinos”.[64] Although this was improved on subsequent runs, it seemed that the desire to reduce background effects (by choosing to use metallic gallium instead of the gallium chloride solution used in GALLEX) had come at the price of increased error in the chemical extraction process.[65]

The BOREXINO detector at the National Laboratory of Gran Sasso in Italy is currently the only available low-threshold detector that employs a live-time counting method. Like Kamiokande, it is a scintillator device monitoring neutrino–electron scattering events. However, thanks to its low energy threshold, BOREXINO can detect neutrinos from the second neutrino-producing reaction in the proton–proton chain (“pp2”):[66]

$$^{7}Be_4 + e^{-} \rightarrow {}^{7}Li_3 + \nu_e, \qquad \text{(pp2)}$$

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[62] Quoted as equation 1 in Bahcall and Ostriker (1997), p. 196. See also Lazio, op. cit. (note 57).

[63] Sutton (1992). In addition, the frequency of the pp3 reaction depends strongly on the temperature of the sun, which means that the deficit of pp3-neutrinos could have been resolved by a change in the estimate of the core temperature of the sun.

[64] Quoted in ibid.

[65] Filippini (2002).

[66] Lazio, op. cit. (note 57). See also Bahcall and Ostriker (1997), p. 196.

in which a proton in a beryllium nucleus combines with an electron and turns into a neutron, transforming the whole into a lithium nucleus with a neutrino as a by-product. A "strong suppression of the Be(7) neutrinos"[67] is suggested when the BOREXINO results are combined with those of the other experiments.

In sum, the Kamiokande II, SAGE, GALLEX and BOREXINO experiments produced data which confirmed the Homestake results, and suggested that the neutrino detectors were most likely not responsible for the solar neutrino problem. Now physicists were truly forced into the conclusion that there was something wrong with their understanding of the basic physics of neutrinos.

3. The resolution of the solar neutrino problem

3.1. Pinch's social constructivist view

The sociologist of science Trevor Pinch picked up the neutrino-detection story as an illustration of the "strong programme" in the sociology of scientific knowledge (SSK), in his 1986 book *Confronting Nature: The Sociology of Solar Neutrino Detection*. In line with Harry Collins's "empirical programme of relativism", Pinch believes that a scientific controversy does not reach closure through "better experimentation, more knowledge, more advanced theories, or clearer thinking."[68] Rather, the course of science is determined by social causes. A controversy lifts the veil of scientific consensus from taken-for-granted theory and methodology; a careful empirical study of controversies can thus reveal the rhetoric and social reality operating beneath. The book that resulted from Pinch's investigation is admirably detailed and well-informed, containing a great deal of the relevant physics and a thorough step-by-step description of events that took place, as well as excerpts from over thirty interviews with the principal scientists involved.

If Pinch is correct in his conclusions, the implications for science are profound. While SSK and social constructivism do not necessarily entail relativism, Pinch clearly regards scientific knowledge as a cultural product: "By studying how scientists themselves can provide different

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[67] "Borexino Solar Neutrino Experiment", http://pupgg.princeton.edu/~borexino/welcome.html (accessed most recently on 6 January 2007).

[68] Pinch (1986), p. 20; Collins and Pinch (1998), pp. 144–145.

interpretations of Nature, the truth or falsity of scientific findings is rendered an achievement of scientists, rather than of Nature."[69] Pinch's and other similar sociological arguments have contributed a great deal to provoking the "Science Wars" of recent decades.

To regard knowledge as socially constructed is to take the view that scientific facts are not inevitable, that if social conditions were different, the resulting knowledge would probably be different, too. As Jan Golinski puts it, constructivism seeks to explain how knowledge is acquired, without assessing its truth or validity.[70] It seeks to examine science purely in its social dimension, without engaging with issues of normative criteria of what makes "good" science. A pioneer in the articulation of this approach to science studies was David Bloor, who articulated the "strong programme" in 1976. He aimed to facilitate a "scientific" sociology of science, which would "embody the same values which are taken for granted in other scientific disciplines."[71] Bloor put forward four basic methodological tenets:

> (a) Causality: sociologists should explain the causal conditions that bring about scientific beliefs.
>
> (b) Impartiality: their investigations should be impartial with respect to rationality or irrationality, truth or falsity, success or failure.
>
> (c) Symmetry: beliefs on both sides of the above dichotomies should be explained by the same type of causes.
>
> (d) Reflexivity: the above principles should apply to the sociology of scientific knowledge itself.

As Bloor and his colleague Barry Barnes put it, "all beliefs are on a par with one another with respect to the causes of their credibility". There are no absolute proofs which establish the superiority of one theory over another, only locally credible arguments. All beliefs are equally in need of causal, social explanation.[72]

Pinch argues that rational epistemological and methodological rules are unable to account for the resolution of scientific controversies. Thus, the sociologist must offer some alternative to explain how

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[69] Pinch (1986), p. 20.
[70] Golinski (1998), p. 7.
[71] Bloor (1991), p. 7.
[72] Barnes and Bloor (1982), pp. 22–23.

agreement arises. Pinch acknowledges his commitment to a Bloor-style symmetrical analysis, and declares that he will "capture some of the realities of research at the frontiers of modern science".[73] Pinch's choice of the neutrino episode was partly motivated by the fact that it was not a case that had been closed and "black-boxed" yet, all the better to reveal the social forces at work in the process of shaping scientific beliefs. In an episode like this, a number of different competing scientific views can be observed, providing an ideal starting point for a symmetrical study.

Pinch uses four key concepts in his analysis. (1) *Evidential context:* this is the context in which results of an experiment are interpreted and given significance.[74] (2) *Interpretive flexibility:* this is the degree of ease with which aspects of the evidential context can be altered. For example, Bahcall's interpretive flexibility included his "fiddling" of the parameters of his prediction after the experimental results emerged. Pinch says that "if it can be shown that different views of the logical relationship between theory and experiment are possible and indeed are held by scientists then we can . . . say that logical decision making does not force the issue."[75] Scientific agreement arises when interpretive flexibility "vanishes from scientific findings"[76] and theory is "black-boxed". (3) *Negotiation:* this is an instrument through which social causes operate, the "mechanism through which science works in cases of disagreement" as Martin Eger puts it. Negotiation consolidates evidential contexts, and facilitates interpretative flexibility. Pinch says that Bahcall "negotiated" his theoretical prediction down to a closer match with Davis's result, and "negotiated" with Maurice Goldhaber, the Director of the Brookhaven Laboratory, in order to convince him that the experiment was worth funding. Pinch "assimilates very different kinds of activity under the same rubric", labelling them all "negotiation"; any confirmations and refutations in science are "locally managed rhetorical achievements which are inseparable from the particular social contexts in which they occur."[77] Pinch says that all perceived logical relationships should be treated as "epiphenomena of underlying social relationships".[78] (4) *Credibility*: this

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[73] Pinch (1986), p. ix.
[74] Ibid., p. 31.
[75] Pinch (1985), p. 172.
[76] Pinch (1986), p. 21.
[77] Eger (1997), p. 347.
[78] Pinch (1986), p. 208.

is something for which every scientist strives. Pinch places particular emphasis on how Davis would relentlessly test any criticism thrown at his experiment, even when he himself believed his experiment to be perfectly in order. He was rigorous and thorough, and soon built up a reputation as an extremely good experimenter possessed of openness, caution and modesty.[79]

Within the analytical framework formed by the above concepts, Pinch identifies a number of social causes behind the negotiations taking place among the neutrino scientists. Ambition is perhaps the most important source of social causes identified by Pinch. He takes it as given that a chief motivation of each scientist is to further his career and raise his own prestige. The young astrophysicist John Bahcall, in particular, is seen as fiercely ambitious. Initially, to make the experiment appear a "crucial" test of solar theory, which would place him at the forefront of research and bolster his reputation, he made his prediction of neutrino flux as high as possible. Later, when the results failed to match his prediction, he became depressed and attempted to negotiate his theoretical prediction down to a closer fit. However, it soon occurred to him that this may not have appeared professional, and he switched his stance to a declaration that it was impossible to match experiment with theory. This was a calculated decision that would enable him to continue his career in solar neutrinos. Bahcall's interpretative flexibility was not forced by logical evaluation of the evidence; rather, it was guided strategically by his social aspirations. Because of the Homestake experiment's supposed status as a crucial test, which Davis and Bahcall carefully engineered, more people became involved with the experiment than might have been expected, further elevating the significance of the whole episode.

The experiment's link with the evidential context is essentially the personal relationships between the scientists involved, according to Pinch. Davis had a technique to detect neutrinos in 1954, but lack of support led him almost to give up the project. In 1957 the cross-section measurement of a helium reaction provided cause for speculation that neutrinos created inside the sun could be detected. Davis then teamed up with Bahcall. A bond of mutual trust and shared aims provided a consolidated base from which funding could be sought, and support

[79] Ibid., pp. 172–175.

provided for any claims and decisions made. Pinch sees this close relationship which developed between Bahcall and Davis, and subsequently between the two camps of theorists and experimenters, as responsible in large part for the sidelining of dissenting physicists such as Kenneth Jacobs, who suggested that the apparent solar neutrino deficit was an artifact created by the trapping of argon ions in polymerized C_2Cl_4.[80] According to Pinch, both sides of the Davis–Bahcall collaboration were primarily concerned with helping and appearing trustworthy to each other, and less to the astrophysics community as a whole.

To sum up, Pinch appears to regard the construction of science as taking place in the following way: a suitable evidential context is negotiated, the results of the experiment are produced, and then copious negotiation takes place based around the credibility and the interpretative flexibility of various actors, dealing with anything from the calibration of the instruments to the parameters of the theoretical predictions. He concludes that "the case documented here seems to add support to the view that contradiction and non-contradiction are social accomplishments, and that scientists are not forced by nature to take one view or the other".[81] Pinch's narrative makes sense, and the causes he postulates for the developments are plausible and consistent enough. But does his case study of neutrino-detection really provide significant support for social constructivism?

3.2. Critique of Pinch's analysis

There are several concerns we would like to raise regarding Pinch's argument. The first point is an irony: at the time of Pinch's writing, the solar neutrino problem had indeed not been resolved — that is to say, social causes had no more forced the issue than had nature. Even if we set aside that discomfort, and even if we accept Pinch's explanation of the neutrino episode, there is another obvious problem: this is just one case. Saying that *some* science is socially constructed is not the same as saying that there is no possible way that rational epistemic methods can ever apply. Even in this particular case, if we accept social causes, we also need to know more about how the social causes operate. In particular, we need a more convincing explanation of

[80] Ibid., pp. 157ff.

how scientific results are *maintained*, after the initial social conditions which brought about the consensus cease to be present. Since Pinch regards logical relations as mere epiphenomena, they cannot have the causal power to continue to compel assent to a scientific fact from the next generation of scientists. There is also a problem of reflexivity: if we accept that scientific facts have no special epistemic warrant, why should we treat social factors as phenomena about which Pinch can gain reliable and objective knowledge? He is surely selecting social causes that conform usefully to his hypothesis and narrative structure. Much of Pinch's "evidence" comes from interview data, which are "not the royal road to the scientific minds, but on the contrary, data themselves in need of interpretation."[82] Pinch's interpretations would have been negotiated to suit the tastes and interests of his sociologist colleagues.

All of these points warrant a search for an alternative sociological framework of analysis, if we must have a sociological account. A possible alternative is the kind of "social functionalism" that Warren Schmaus defends, in which social facts are understood in terms of their functional relationship to other social facts, environmental conditions, and types of actions.[83] (This social functionalism is defined in analogy to the functionalism about mental states that is discussed in the philosophy of mind.) It could be that the behaviour of a scientist is influenced by social causes that affect him or her; however, as the behaviour of the scientist is characterized by its scientific functions, an exact analysis of its personal causes is irrelevant. The meaning of the action for the individual may not be the same as its functional meaning, and many different personal causes may lead to the same functional result. To take an example from Pinch's analysis: Davis repeatedly tested his experiments. The motivation that Pinch identifies is that Davis wished to appear credible and keep his reputation gleaming in the eyes of the theoreticians, yet the wider *function* of Davis's behaviour could be accuracy or openness, two key requirements of a rigorous scientific methodology. Functionally, it makes no difference whether or not Davis believed his experiment to be perfectly accurate before he performed the requisite tests. The role of negotiations could also be understood in a functional

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[81] Ibid., p. 153.
[82] Gregersen and Køppe (1988), p. 460.
[83] Schmaus (1999).

way. The fact that this process operates through personal relationships located in a social nexus does not negate its scientific functions.

In other words, even if we can give a credible sociological account of a scientific episode, it would be rash to assume that we have thereby killed off the possibility of an objectivist account that appeals to the truth or reliability of the consensus that scientists have reached. As André Kukla writes:

> The most favourable data that constructivists can hope to obtain for their thesis are one-to-one correlations between types of scientific decisions and the social circumstances in which the decisions are made. But such data will *always* be compatible with the non-constructivist view that the social circumstances *cause* the decision to be made as it is, but that the decision is nevertheless correct or incorrect depending on the properties of an independent nature, and that it's either warranted or unwarranted on the basis of absolute epistemic standards.[84]

Michael Friedman makes a similar point:

> why should the enterprise of empirically and naturalistically describing how beliefs become locally credible as a matter of fact compete or stand in conflict with the enterprise of articulating the non-empirical and prescriptive structure in virtue of which beliefs ought to be accepted as a matter of norm?[85]

Acknowledging that science is a social process is not tantamount to denying that epistemic evaluations are invalid or pointless.

3.3. Neutrino oscillations

At present, twenty years after Pinch's pioneering sociological analysis, the dominant opinion among physicists seems to be that the solar neutrino problem has now been resolved in an objective way, on the basis of good theoretical and experimental arguments. The recent consensus revolves around the seemingly unlikely idea of "neutrino oscillations". In this section we trace the origin and growing credibility of this idea, and critically examine the assumption that it provides an unequivocal solution to the solar neutrino problem.

The basic idea of the neutrino-oscillation hypothesis is that the observed neutrino flux is low because some of the solar neutrinos evade

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[84] Kukla (2000), p. 41.
[85] Friedman (1998), p. 244.

detection by a feat of shape-shifting on their way to the earth. There are three different types, or "flavours", of neutrinos, called electron-neutrinos (ν_e), muon-neutrinos (ν_μ), and tau-neutrinos (ν_τ) — because they are associated with electrons, muons, and tau particles respectively. The neutrinos generated in the fusion reactions in the sun are electron-neutrinos, and that is what the Homestake and other standard detectors are equipped to detect. If, for some reason, the solar electron-neutrinos should change into muon- or tau-neutrinos in transit from the sun to the earth, then they would escape detection, because each type of neutrino interacts in a different way with other matter.

When Dudley Shapere published his philosophical review of neutrino detection in 1982, which we will have further occasion to discuss in the next section, neutrino oscillation was only "the most serious possibility" among the "more extreme possibilities" that might resolve the solar neutrino problem:

> variation of the gravitational constant…over the lifetime of the sun…; neutrino decay or oscillation of neutrinos between the "electron" type and other types . . . ; the ubiquitous black hole, this time at the center of the sun; and even, perhaps at the very fringe of scientific speculation (but not for that reason necessarily incorrect), a rehauled weak interaction theory in which the existence of neutrinos is not assumed. It should be added that there is very little reason to accept any of these proposed non-standard solutions of the solar neutrino problem; indeed, many of them suffer from extreme difficulties.[86]

Neutrino oscillation may seem like an entirely *ad hoc* hypothesis, but it was initially postulated for independent reasons, in fact even before the solar neutrino problem arose. (See Appendix to this chapter for more details on the idea of neutrino oscillations and its origins.)

Bahcall's attitude to the prospect of proving the existence of neutrino oscillations as the solution to the solar neutrino problem is typical of the scientific community: though "research began with attempts to discover the properties of the sun with neutrinos . . . it now appears likely that a large community of physicists, chemists, astrophysicists, astronomers and engineers working together may have stumbled across the first observed manifestation of physics beyond the electroweak model

[86] Shapere (1982), p. 499. Shapere added in a footnote that there were "grave doubts as to whether [the neutrino-oscillation] hypothesis could really reduce the predicted neutrino flux to the observed level."

. . . . we may have been incredibly lucky." Bahcall concludes that "the marvellous lesson of solar neutrino physics is that work on the forefront of one field of science has the potential to lead to important and completely unanticipated developments in a different field of science. This seems . . . both humbling and beautiful."[87]

Allan Franklin is rather more blunt and pragmatic, concluding that "there doesn't seem to be any plausible way of modifying the SSM [Standard solar Model] to accommodate the solar neutrino results",[88] so the neutrino oscillation hypothesis "is the sole remaining plausible explanation of the deficiency in the number of solar neutrinos."[89] This is somewhat unexpected, given Franklin's stated commitment to provide a historical reconstruction of "an argument that one might give for the reality of neutrinos"[90] in order to persuade his audience "that science is a reasonable enterprise, which produces knowledge of the physical world on the basis of valid experimental evidence and reasoned and critical discussion."[91]

A sceptic, or a non-scientist, might form the impression that scientists had simply run out of ideas to explain the solar neutrino problem; all other explanations had been refuted, leaving neutrino oscillations as the theory which the scientific world chose to believe, while congratulating itself on the incredible luck of having stumbled on the first instance of a new strand of physics. Could it be that the shakiness of the neutrino-oscillation hypothesis is conveniently disguised by emphasizing the new field of study it generates? Isn't the idea of neutrino oscillations really an *ad hoc* hypothesis after all, designed only to reinforce accepted, well-established theories? Pinch confronted scientists working in the field with these questions. The responses ranged from agreement ("it is something that has been proposed merely to solve the problem that has no other observational consequences"), through a dismissal of the importance of the *ad hoc* status of a theory ("most hypotheses in physics are *ad hoc* . . . most physical theories, after all, are

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[87] Bahcall and Ostriker (1997), p. 217.
[88] Franklin (2000), p. 311.
[89] Ibid., p. 289.
[90] Ibid., p. xii.
[91] Ibid., p. xi.

invented in response to an experimental result"), to disagreement ("in fact Pontecorvo suggested it before there was any negative result").[92]

In one sense, the theory of neutrino oscillations is definitely not *ad hoc*, as it was indeed proposed before the solar neutrino problem became known. However, what was more *ad hoc* was the scientific community's decision to accept and develop the idea of neutrino oscillations. There are always many little-known unconventional ideas around, and Pontecorvo's original idea might have just remained in that category. As we have seen, it only became widely accepted because it was convenient for solving the solar neutrino problem, when all other attempted solutions failed. If we follow Karl Popper and Imre Lakatos, an *ad hoc* modification is defined as one which "has no testable consequences that were not already testable consequences of the unmodified theory".[93] The spirit of this definition is that "before it can be regarded as an adequate replacement for a falsified theory, a newly and boldly proposed theory must make some novel predictions that are confirmed", as well as explaining everything that could be explained by the old theory.[94] Does the theory of neutrino oscillations have to its name any confirmed novel predictions?

There are currently various experiments aimed at verifying the hypothesis of neutrino oscillations. First of all, there are attempts to measure neutrino mass (e.g. ITEP, Bergkvist), since the oscillation hypothesis requires at least some neutrinos to have non-zero mass.[95] The mass estimates have steadily decreased over the years, and it is now generally accepted that the mass of the neutrino has a very low upper limit. It was thought that proving that neutrinos have mass would constitute indirect yet significant evidential support for the oscillation hypothesis, but present methods for measuring the mass of the neutrino turned out to be insufficient. The "long-baseline" experiments which hoped to measure neutrino mass indirectly mostly have yet to obtain conclusive results and, anyway, are still limited to measuring the mass difference between neutrino flavours, not the absolute masses. It seems that the means for more accurate direct mass measurements are not yet available, so the

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[92] Pinch (1986), p. 188.
[93] Chalmers (1999), p. 75.
[94] Ibid., p. 80.
[95] For a summary of mass-measurement experiments, see Franklin (2000), pp. 183–201.

focus has turned instead to developing experiments which attempt to detect the oscillations themselves.

Super-Kamiokande was important because it was one of the first neutrino experiments to provide data which was independent of the Standard Solar Model.[96] The fact that it detected atmospheric neutrinos meant that its results did not rely on calculations made on the basis of the nuclear reactions assumed to take place in the sun. It obtained measurements which seemingly indicate that "muon-neutrinos are disappearing into undetected tau-neutrinos or perhaps some other type of neutrino". These data could be taken as supporting muon–tau neutrino oscillations and hence could be considered as "evidence in favour of neutrino oscillations which is independent of the Standard Solar Model",[97] although it could also be argued that without actually detecting the tau neutrinos we cannot rule out the possibility that some other mechanism might be at work. The Super-Kamiokande team was candid about the limitations of interpretations of their results with regard to the mass of the neutrino: "the experiment does not determine directly the masses of the neutrinos leading to this effect, but the rate of disappearance suggests that the difference in masses between the oscillating types is very small."[98] There have also been several long-baseline experiments (e.g. K2K, MINOS, Los Alamos) which are trying to measure the oscillation potential (how likely a neutrino is to oscillate) and oscillation length of neutrinos in a more direct manner.[99]

Attempts to gain independent evidence supporting electron–muon neutrino oscillations eventually resulted in positive data from the Sudbury Neutrino Observatory (SNO) in Ontario, Canada.[100] The SNO detector is similar to the Kamiokande detector, except that SNO uses heavy water,

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[96] See the Super-Kamiokande website, http://www-sk.icrr.u-tokyo.ac.jp/sk (accessed initially on 10 January 2004, most recently on 7 January 2007).
[97] Franklin (2000), p. 289.
[98] "Evidence for Massive Neutrinos" (press release of 5 June 1998), on the Super-Kamiokande U. S. Collaboration website, http://www.phys.washington.edu/~superk/sk_release.html (accessed on 10 January 2004).
[99] Much information about long-baseline experiments can be found on the website for K2K (From KEK to Kamioka — Long Baseline Neutrino Oscillation Experiment), especially the FAQ page, http://neutrino.kek.jp/k2k_faq.html#numass_k2k (accessed initially on 10 January 2004, most recently on 6 January 2007).
[100] See the SNO website, http://www.sno.phy.queensu.ca (accessed initially on 10 January 2004, most recently on 6 January 2007).

which allows the detection of muon- and tau-neutrinos as well as electron-neutrinos. In 2001 the SNO team stated:

> Earlier measurements had been unable to provide definitive results showing that this transformation from solar electron neutrinos to other types occurs. The new results from SNO . . . now reveal this transformation clearly, and show that the total number of electron neutrinos produced in the Sun are just as predicted by detailed solar models.[101]

Later phases of the experiment over the next couple of years refined the accuracy of the results and made them even more reliable. This "direct" evidence for solar neutrino oscillations lent credence to neutrino oscillations as a theory with previously unknown testable consequences.

4. Neutrinos and the nature of observation

4.1. Observation vs. detection

Some philosophers of science have been captivated by the story of neutrino detection, and have used it as a striking example to illustrate their philosophical theories of observation. In this section we make a critical examination of these works. For philosophers, it remains an important consideration that neutrinos are unobservable, if "observation" means direct perception. Even with today's advanced instrumentation, it is still true that "no detector exists that can record the neutrino. Instead, physicists have to rely on stopping a neutrino and converting it to other particles that can be observed."[102] However, physicists do routinely talk about observing neutrinos, and this is one of the things that prompted the philosopher Dudley Shapere to write his pioneering 1982 paper on the philosophy of observation using a detailed case-study of neutrino detection.[103]

The first thing that Shapere emphasizes is that scientists have generally been sloppy in their usage of words like "observe", "detect", and "see".[104] That is, they have often been insensitive to the epistemological ramifications of their work. This is even true of those, like Reines

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[101] "Press Release: First Results from the Sudbury Neutrino Observatory, June 2001", SNO website, http://www.sno.phy.queensu.ca/sno/first_results/ (accessed on 10 January 2004).

[102] Solomey (1997), p. 56.

[103] Shapere (1982), pp. 486, 489.

[104] Ibid., p. 489.

and Cowan, who were concerned with proving the reality of the neutrino when designing and describing their experiments. Our consideration of the primary sources will therefore require a layer of interpretation: philosophers must judge for themselves what counts as proof of realism for neutrinos.

Shapere proposes and defends simple criteria of "observation" and "observability":

> *x* is directly observed (observable) if: (1) information is received (can be received) by an appropriate receptor; and (2) that information is (can be) transmitted directly, i.e., without interference, to the receptor from the entity *x* (which is the source of the information).[105]

We hinted earlier that "observe" is a potentially problematic word, and thankfully, Shapere goes to great pains to qualify its usage. He means by "direct observation" not that *x* is necessarily experienced by human sense-perception, but simply that there is reliable evidence that some form of informative interaction with *x* has taken place. The value of sense-perception has been overrated anyway: sight, for instance, works only on a small part of the electromagnetic spectrum, which itself is the manifestation of only one of a number of fundamental forces in nature.

If we are to believe that Davis or Reines–Cowan really observed neutrinos, we must accept that they did so in a highly theory-laden manner. The path between source and detector in the Homestake experiment was dependent on theory as much as the Reines and Cowan experiment. In the former case, we have neutrinos going from the centre of the sun through the outer layers of the sun, then through space, then through hundreds of meters of rock, into a tank of C_2Cl_4, very occasionally reacting with a chlorine atom to produce a radioactive isotope of argon, which is extracted by bubbling helium through the tank, then collected atom by atom, measured, the data analyzed, etc. In the latter case, we have neutrinos coming from a reactor core, through the shielding into the cadmium chloride target, interacting with a proton to produce a neutron and positron, which interact with other particles, expelling gamma rays of a characteristic energy and temporal separation. So even though the existence of the neutrino became a scientific certainty with these experiments, belief in this entity must entail a belief in the theories that make the experiment work.

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[105] Ibid., p. 492.

In either case, it is clear that the burden of credibility rests on the theory. Scientists of course only use trusted theories in designing their observations, but theories change over time: Pauli postulated the neutrino and his postulate is *now* regarded universally as true, but for a time there were a number of possibilities about some of the qualities of the particle. For instance, it was not until Davis's work that it became clear that the neutrino was distinct from its own antiparticle.[106] Since then, further theoretical developments have revealed more than one sort of neutrino. Because we are scrutinizing experiments from a period of about 25 years, we must be sensitive to changes like these. Furthermore, it is necessary to properly consider the theoretical contexts in which the experiments were located (Pinch uses the notion of "evidential context" to refer to a similar idea). An easy mistake would be to impose later theoretical contexts onto earlier experiments, which would result in our defining any experiment involving what were *later* understood as neutrinos as a demonstration that they were real.

Shapere gives observation two aspects: perceptual and epistemic.[107] Even when it is perception-based, observation includes "an extra ingredient of focussed attention". An observer not only perceives data, but focuses attention on some aspect of the entity too. Epistemic observation relates to the evidential role of observation, where experience serves to provide evidence for existing beliefs. In the realm of entities imperceptible by human senses, says Shapere, the perceptual and the epistemic aspects have become disconnected and science has focussed on the latter.

Shapere's distinction is helpful, but we would prefer to make a different distinction, between *observation* and *detection*. Although observation and detection are often thought to be interchangeable, they are in fact different. One way of distinguishing the two terms is by linking them to different sorts of realism. Observation as we are defining it here is connected with "truth realism", which gives certainty about some specified quality of an entity. When we *observe,* we are looking for something *about* our entity, some quality over and above its mere existence. In contrast, "existence realism" is certainty about the *existence*

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[106] Majorana proposed in 1949 that the neutrino may be equivalent to its own antiparticle. Davis's first experiment was a test of this theory. See Pontecorvo (1983), p. 1098.

[107] Shapere (1982), pp. 507–508.

of something, a matter for "detection". In detection, we are concerned with whether something is actually there.

In grappling with these issues, it is helpful to learn from Peter Kosso's work on the nature of scientific observation. Kosso works on a similar basis to Shapere, but he thinks that Shapere and many others are wrong to talk of an observable/unobservable *dichotomy*, or even a one-dimensional spectrum between the "observable" and the "unobservable". Instead, Kosso points out that observability has many dimensions. He defines observation as the flow of information like Shapere, but Kosso points out that there are other tests of quality of observation than directness. A better test of observation is that it must tell us something *about* an entity rather than just being a report *of* the entity.[108] This is in line with our distinction between observation and detection. On the question of realism, Kosso again seeks to avoid a rigid dichotomy between reality and non-reality; instead, the question is reduced to degrees of reliability. The most important measure of reliability for Kosso is "independence in the interaction-information account", which means that the entity under test must provide information resolvable in more than one theoretical domain. Therefore a single experiment where the only theoretical domain is, say, beta-decay may be unreliable, but an experiment or a set of experiments which reach across different theoretical domains would have more assured reliability. Realism is not secure unless information about the entity in question is received via multiple theoretical channels.[109]

We are now ready to give a more precise statement of our own distinction between observation and detection:

> Entity *x* is shown to exist (it is *detected*) if information about *x* can be received by an appropriate receptor; the information may not be informative about any particular property of *x*.

[108] Kosso (1988), p. 451.
[109] Ibid., p. 466.

> Some quality *y* about *x* is shown to be true (it is *observed*) if:
>
> (1) information about *x* can be received by an appropriate receptor;
>
> (2) that information is transmitted reliably;
>
> (3) the information is informative about *y*.

The first schema is similar to Shapere's requirement for the passage of uncorrupted information; the second schema shows what extra criteria need to be fulfilled for the interaction to qualify as an observation. These schemas might seem weaker than Shapere's, but if we recognize that there is no real distinction between direct and indirect observation, and that observation is different from detection, then we think that it is a workable extension.

But can our distinction really be defended? Obviously observation implies detection — and doesn't detection also imply observation? How can we receive information about an entity without it being information about some property? The problem is a serious one. The detection–observation distinction can only be maintained if we can register the presence of a property without measuring it precisely. A consideration of some older neutrino experiments shows that this is possible: some experiments were certain about the presence of a quantity (the momentum of an entity) without having any certainty about its magnitude. These may be regarded as successful at detection but unsuccessful at observation. More generally, having reliability as a requirement for observation but not detection gives us a possible way out: information about some property may be present but lack sufficient reliability to enable us to infer anything about it for sure. Reliability here is a property of information transfer that could be assessed a number of ways, and Kosso's approach is helpful: an information-transfer process could score variously on his various "dimensions": immediacy, directness, amount of interpretation, or independence.[110] It is possible that an experiment is quite reliable in some dimension, to indicate the presence of something, but not reliable enough overall to give us true observation.

[110] Ibid., part 3.

4.2. Who first observed neutrinos?

Informed by the general discussion in the previous section, we now turn to the question of who first observed (anything about) neutrinos. It is widely assumed that the first people to prove that neutrinos were real were Reines and Cowan, with their 1953 liquid scintillator, which showed the characteristic signals produced by neutrino interactions. Davis's technique in the Homestake experiment was to collect an easily manipulable product of a neutrino interaction. When describing the history of the neutrino, commentators almost invariably see these two experiments as the first conclusive proof of the existence of the neutrino.[111] There is a philosophical debate about how far these experiments could be regarded as "direct detection". It takes a loose definition of "direct" to include the Davis experiment as such, and under such a definition experiments that took place in the 1930s and 1940s could also be regarded as direct detection.

In Reines and Cowan's scintillation counter the information about the neutrino events was received by means of two pulses of the right magnitude separated by the right amount of time. The pulses were said to be "characteristic" of a neutrino interaction, which means that they agreed with what theory predicts about the interactions going on in the target material. So the information was received appropriately and it was reliable. But the same can be said about a beta-decay reaction in which the energy of the neutrino is deduced from the observed energies of the other particles involved. Reines and Cowan's apparatus returned results that were compatible with theories of neutrino production in the reactor, so it was observing the energy of the neutrinos well. Since the theory of neutrino-production in reactor cores is independent of the theory of inverse beta-decay, their observation of the core scores high on Kosso's criterion of independence.

When it comes to the observation of *neutrinos*, however, there is no clear-cut distinction between the original beta-decay experiments and Reines and Cowan's experiments. In the latter, the reaction in the cadmium chloride could only be initiated by neutrinos of certain energy, so the specific property of the neutrinos that was being resolved was their energy. It is probably fair to say that all of these experiments *detected*

[111] See, for example: Trigg (1975), p. 191; Galison (1997), p. 460; Pinch (1986), pp. 50–54; Solomey (1997), p. 64.

neutrinos, one way or another. But when was the line between detection and observation crossed? It could be argued that even some of the experiments described in Crane's 1948 review fulfill all the requirements for observation, though they have typically been ignored in historical studies of neutrino detection. In Crane and Halpern's ^{38}Cl experiment of 1938, the information obtained was about an interaction. The data took the form of clusters of bubbles that revealed the recoil momentum of a nucleus, showing it to recoil with more momentum than the emission of the electron alone would allow. So this experiment was certainly detection, but could it also be observation? Was the information about the momentum reliable? The momentum could not be precisely assessed without data on the angle of emission, which suggests that it was not sufficiently reliable. It could, however, be argued that reliable information was present in as much as the data allowed the momentum of the neutrino to be placed between some maximum and minimum.

It all depends on how one judges reliability and, again, Kosso's "dimensions" may help us in gaining more clarity. The Crane–Halpern experiment scores badly on immediacy because the data is not perceivable by human senses, and also on directness because the chain between the neutrino and the observer is long. That much is perhaps the same for all neutrino experiments, but the Crane–Halpern experiment also scores very poorly on independence — the neutrino is detected by the same type of process that prompts the postulation of its existence. On the amount of interpretation it scores rather better, because it requires very little interpretation to assume that something is carrying off the momentum. But all in all, we can see that the Crane–Halpern counts as observation of neutrinos only as much as the original beta-decay observations. Sherwin's 1948 experiment is probably a better candidate for observation, because of the striking fit obtained for the 180° emissions. Even though the data fell apart for the other angles, it seems that Sherwin had fleetingly achieved good observation. In short, there is no black-and-white dichotomy between observation and non-observation. Rather, there is a multi-dimensional continuum of observability, along all of the dimensions of observability specified by Kosso.

5. Summary

We have followed the curious and multi-faceted history of solar neutrino detection, in which chlorine-37's ability to interact with neutrinos played a crucial role. Chlorine provided scientists with the pleasing prospect of being able to detect the elusive neutrinos directly, but when that promise was finally fulfilled in the Homestake experiment it revealed a serious scientific puzzle, namely the solar neutrino problem. The controversy over the solar neutrino problem has now died down, but the character of the resolution of the controversy remains somewhat puzzling: it is in line with neither social constructivism nor simple-minded objectivism. In another direction, a careful consideration of the philosophy of observation reveals that neither the Homestake experiment nor any other single experiment constituted a completely unequivocal advance in making neutrinos observable.

Appendix: Neutrino oscillations

The originator of the idea of neutrino oscillations was Bruno Pontecorvo. Cosmic-ray interactions revealed the prolific production of a type of particles with rather unusual and seemingly contradictory properties. They were named "strange" mesons and included the kaon (K), and the lambda (Λ) and sigma (Σ) particles. There were two main reasons why these particles were considered to be so unusual: they are only produced in pairs, and they have unexpectedly long lifetimes. Pontecorvo found a way to solve the conundrum by proposing a new quantum number, "strangeness", which obeys a conservation law: "whenever a nuclear reaction or decay occurs, the sum of the strangeness quantum numbers before the process must equal the sum of the strangeness quantum numbers after the process." Therefore, "the production of strange particles in pairs is explained by assigning $S = +1$ to one of the particles, $S = -1$ to the other and $S = 0$ to all non-strange particles"[112] However, strangeness is not conserved in weak interactions: "this explained why the particles were copiously produced in pairs by the strong interaction, in which strangeness is conserved", whereas the decay of a strange particle into non-strange ones were by the weak interaction, as it involved an overall change of strangeness.[113]

Considering the interactions of *K*-mesons, Pontecorvo postulated that the K^0 and anti-K^0 were quantum-mechanical superpositions of short-lived and long-lived particles, K^0_S and K^0_L; K^0 and anti-K^0 had definite masses and K^0_S and K^0_L had definite lifetimes. The most significant aspect of this theory for our present purposes was the

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[112] Serway and Beichner (2000), p. 1524.

prediction of interference and oscillation between K^0_S and K^0_L particles. Given that the masses and lifetimes of the K^0_S and K^0_L mesons differ, the probability of observing a K^0 meson or an anti-K^0 meson varies with time, where "the frequency of the beat oscillations depends on the mass difference between the K^0_S and the K^0_L". The phenomenon of beats in this context is similar to those "that occur when two waves closely spaced in frequency combine to produce a noise that pulsates between loud and soft"[114] Essentially, the K^0_S and K^0_L particles can be thought of as mass eigenstates, particular combinations of which constitute the K^0 and anti-K^0 mesons at various points in time. This means that the K^0 and anti-K^0 mesons are more of a fluid concept than simply solid, unchanging entities. Pontecorvo began searching for other elementary particle systems which might echo the behaviour found in *K*-mesons, and suspected that neutrinos might undergo oscillations. As explained by a scientist working on the SOUDAN2 experiment, which measures the atmospheric neutrino flavour ratio, "each neutrino would consist of a mixture of matter waves; any difference in rest mass would give rise to 'beats' between them. The result would be that the nature of the mixture would change as the waves propagate", meaning that neutrinos could change flavour.[115] Pontecorvo continued to ponder the issue of neutrino oscillations and in 1967 published a paper considering the consequences of lepton number nonconservation on neutrinos.[116]

The Homestake results emerged the following year, and shortly afterwards Pontecorvo and Vladimir Gribov published a paper proposing neutrino oscillations as a possible explanation for the deficit in the observed neutrino flux: "it is shown that lepton nonconservation might lead to a decrease in the number of detectable solar neutrinos at the earth surface, because of $\nu_e \leftrightarrow \nu_\mu$ oscillations, similar to $K^0 \leftrightarrow$ anti-K^0 oscillations".[117] Gribov and Pontecorvo noted that lepton nonconservation "would be in the right direction if the necessity should definitely arise of accounting for unexpectedly small values of detected solar neutrinos" since it "leads to virtual or real transitions between . . . neutrino states". The oscillations would have the effect of "decreasing the number of detectable solar neutrinos with respect to the number

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[113] Franklin (2000), p. 284.
[114] Brown (1997). When waves with different wavelengths but similar frequencies are superposed and interfere with each other they are periodically in and out of phase. There exists between them a "temporal alternation between constructive and destructive interference" and "the amplitude and therefore the intensity of the resultant sound vary in time". This is the phenomenon of beating, which can be defined as "the periodic variation in intensity at a given point due to the superposition of two waves having slightly different frequencies". See Serway and Beichner (2000), pp. 564–566.
[115] "The Oxford Neutrino Experiment Home Page", http://www.physics.ox.ac.uk/Neutrino (accessed initially on 9 January 2004, most recently on 6 January 2007). See also Brown (1997).
[116] Franklin (2000), p. 286.
[117] Gribov and Pontecorvo [1969] (1994), p. 274.

expected theoretically under the assumption that lepton charges are strictly conserved".[118]

Although the Pontecorvo–Gribov paper seems a very significant work in retrospect, the next ten years yielded few further developments along this line. This "decade of silence" on the subject of neutrino oscillations probably reflected the necessity of working through the issues of the experimental technique and the implications of "the more formal theoretical assumptions which are contained in the complicated mixture of astrophysics, nuclear physics, neutrino physics and chemistry used to predict the events in Davis' tank", before jumping onto an entirely new idea.[119] When other avenues of questioning had been exhausted, the neutrino oscillation hypothesis began to seem more attractive, although the idea of "changing fundamental physics as a result of an astrophysics experiment" was met with some trepidation by many physicists.[120] As one of the scientists interviewed by Pinch put it: "every year, or every few years, somebody suggests some change in physics, the laws of physics as we know them . . . which are designed to explain puzzling astronomical facts. Roughly once every century one such explanation turns out to be correct."[121] Bahcall and Davis recall, in their 2000 paper:

> Only one year after the first (1968) chlorine results were published, Vladimir Gribov and Bruno Pontecorvo proposed that the explanation of the solar neutrino problem was that neutrinos oscillated between the state in which they were created and a more difficult to detect state. This explanation, which is the consensus view today, was widely disbelieved by nearly all of the particle physicists we talked to in those days. In the form in which solar neutrino oscillations were originally proposed by Gribov and Pontecorvo, the process required that the mixing angles between neutrino states be much larger than the quark mixing angles, something which most theoretical physicists believed, at that time, was unlikely. Ironically, a flood of particle theory papers explained, more or less "naturally", the large neutrino mixing angle that was decisively demonstrated thirty years later in the SuperKamiokande atmospheric neutrino experiment.[122]

A paper by Lincoln Wolfenstein in 1978 brought neutrino oscillations fully to the fore. He specified conditions under which neutrino oscillations in vacuum could occur: (i) at least one flavour of neutrino should have non-zero mass; (ii) the masses of the different flavours of neutrinos should not all be equal; (iii) lepton number must not be conserved, "so that the different neutrino types as defined by the weak charged current are mixtures of the mass eigenstates."[123] The work of S. P. Mikheyev and A. Y. Smirnov in 1985 made a further advance, predicting the MSW effect named after them and Wolfenstein, in which the presence of ordinary matter can enhance neutrino

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[118] Ibid., p. 273.
[119] Pinch (1986), pp. 156–157.
[120] Ibid., p. 194.
[121] Unnamed physicist, quoted in ibid., p. 193.
[122] Bahcall and Davis (2000), p. 4 (online version).
[123] Wolfenstein [1978] (1994), p. 294.

oscillations. Thus "the change of flavour of solar neutrinos may be spectacularly enhanced in the presence of solar matter".[124] This was applied to the solar neutrino problem to show that oscillations could indeed explain the observed neutrino deficit.

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[124] Messiah [1986] (1994), p. 315.

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