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An Account of the Development of the Solar Neutrino Problem

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Introduction

This chapter is a summary of some of the ideas and events that have led to what has come to be known as the solar neutrino problem. The account given here is based upon recollections of many years past and therefore probably contains many inadvertent errors. We hope, nevertheless, that our memories of pleasant and exciting times may be of some interest to students of nuclear astrophysics and especially to friends of Willy Fowler. At every stage of the story described below, Willy provided encouragement, wise advice, and above all, unequaled enthusiasm and a sense of fun. He has stressed by example that the human aspects of science are at least as important as the strictly technical aspects.

Theory and observation depend upon each other for their significance in solar neutrino research. Without a well-defined predicted counting rate the observed number of captures per day loses most of its meaning. Similarly, the theoretical work derives its motivation from the possibility of observational tests. The calculations required for this problem are detailed, precise, and specific; they are not necessary in making the general comparisons with observations that are appropriate for most other work in stellar evolution research. This synergism between theory and observation in solar neutrino work can be contrasted with the situation in a number of other astronomical fields whose initial development occurred during the period described here. The discoveries of quasars, infrared sources, radio pulsars, x-ray sources, and interstellar molecules all had immediate and obvious significance independent of previous theoretical work. The interdependence of solar neutrino theory and observation has been clearly recognized by the funding agencies. Because of this interdependence, we have found it natural to describe the combined history of the subject as we remember it.

We adopt an unconventional format for this narrative. We list in chronological order the highlights of each year as we remember them. We make no attempt to be complete in reciting references or developments; this would be a complicated task for us to undertake now and inappropriate for the present volume. It would also deprive the story of whatever interest it may possess. Naturally enough, we concentrate on events in which we participated since we know these best. For further discussions and many additional references, the reader may wish to consult: Tombrello (1967), Kavanagh (1972), Rolfs and Trautvetter (1978), Barnes (1980), and Chapters 8 and 9 in this volume for a detailed account of the low-energy nuclear physics experiments; Reines (1967) for a description of some of the solar neutrino experiments that were not continued; and Bahcall and Sears (1972), Kuchowicz (1976), and Rood (1978) for summaries of a variety of nonstandard solar models.

Prior to 1962

It is interesting to note that the early literature on nuclear fusion as the basis of stellar energy production did not mention the possibility of testing the ideas by observing neutrinos. In the great papers by Bethe, neutrinos were not included explicitly in the nuclear reactions (see, e.g., Bethe 1939; Bethe and Critchfield 1938). When these works were written, the Fermi-Pauli theory of β decay was more than five years old. However, the principle of lepton conservation was not clearly articulated and one was not required to balance leptons as well as baryons. One of the earliest discussions of the Sun as a source of neutrinos was the stimulating review article by H. R. Crane (1948), a graduate student colleague of Willy's at Caltech in earlier days. Crane used geophysical evidence concerning the rate of heat production in the Earth to exclude neutrino absorption cross sections in the range 10^{-32} to 10^{-35} cm^2.

In the early 1950s, a radiochemical neutrino detector was developed at Brookhaven by R. Davis, based on the reaction $^{37}Cl(\nu,e^-)^{37}Ar$. This method was suggested by B. Pontecorvo (1946) when he was working at Chalk River, and was later studied independently by L. Alvarez (1949). Pontecorvo gave persuasive reasons why chlorine (or bromine) would be a good detector of neutrinos and why a reactor experiment with either of these detectors might be feasible (Pontecorvo presciently dismissed solar neutrinos as not sufficiently energetic).

The report by Alvarez is a remarkable combination of theoretical and experimental insights. It could easily serve as a model of how to write proposals to do experiments in basic physics-in this case, testing the theory of beta radioactivity with a chlorine detector near a pile. Alvarez made specific suggestions on the chemical procedures, expected neutrino capture cross sections, and estimated background effects. Alvarez stressed that ``. . .the most important experimental problems lie in the elimination of the various types of background'' (Alvarez 1949), a statement that applies equally well today. It is interesting to note that at the Irvine Solar Neutrino Conference (see Reines and Trimble 1972) Alvarez mentioned that even in 1949 he had considered using a chlorine detector for observing solar neutrinos.

Note that the Pontecorvo and Alvarez proposals to use chlorine as a detector for reactor neutrinos explicitly assumed that neutrinos and antineutrinos were equivalent. In 1948, the double-beta-decay experiment of Ed Fireman on ^{115}Sn indicated that neutrinos and antineutrinos could be Majorana particles. This experiment was discussed in Alvarez's proposal. The original experiment by Fireman was later shown to be invalid by a more specific search for neutrinoless double-beta-decay by Fireman, among others.

In the course of developing the detector, a 3800 l tank of CCl_4 was buried 19 ft below the sandy soil at Brookhaven in order to reduce the cosmic ray background. This experiment gave a crude upper limit to the solar neutrino flux if the Sun operated on the CNO cycle [the limit being 10^{14} neutrinos-cm^{-2}-s^{-1} (Davis 1955)]. In more modern terms, this amounts to an upper limit of about 40,000 SNU. A reviewer of Davis's paper made the following critical but amusing comment:

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

It was generally believed by astrophysicists in the 1950s that the Sun operates predominantly on the p-p chain and that the only significant production of neutrinos was from the proton-proton reaction that initiates this chain. These neutrinos have a maximum energy of only 0.4 MeV. Since the chlorine detector has a threshold of 0.86 MeV, it is incapable of detecting these p-p neutrinos. The only neutrinos expected to come from the Sun with sufficient energy to be absorbed by chlorine were those from ^{13}N and ^{15}O in the CNO cycle. Although observing neutrinos appeared to be hopeless, the topic was discussed by Davis with Alastair Cameron, Clyde Cowan, Willy Fowler, and Fred Reines, among others, at Gordon Conferences in the 1950s.

A dramatic event occurred in early 1958 that altered the picture completely. At the New York meeting of the American Physical Society, Holmgren and Johnston (1958) reported that the $^{3}He(\alpha,\gamma)^{7}Be$ cross section had been measured at the Naval Research Laboratory and was a thousand times higher than expected! The consequences of this result were pointed out immediately in two letters received by Davis from Willy Fowler and Al Cameron. They suggested, following Bethe's original discussion, that if ^{7}Be was produced, it could capture a proton yielding ^{8}B, which Fowler and Cameron stressed would promptly decay emitting energetic neutrinos. (Both Willy and Al remember that Willy first pointed out the importance of the Holmgren-Johnston experiment in an informal conversation.) The important question was the lifetime of ^{7}Be in the Sun, which would be determined by the total rate of electron capture and proton capture. Both Willy and Al were hopeful that the cross section for proton capture would be large and that the energetic ^{8}B neutrinos might be observed with a chlorine neutrino detector. A handwritten postscript at the bottom of Willy's letter expressed his usual optimism: ``It may be possible to use your results to calculate how many neutrinos are emitted by the Sun and thus determine a lower limit on the cross section for $^{7}Be(p,\gamma)^{8}B$ astrophysically!" Cameron reported his views in a contributed paper to the American Physical Society at the New York meeting (Cameron 1958a), in a summary in the *Annual Review of Nuclear Science* (Cameron 1958b), and in more detail in a supplement to his Chalk River report on stellar evolution, nuclear astrophysics, and nucleogenesis (Cameron 1958c). Willy discussed the implications of a large $^{3}He(\alpha,\gamma)^{7}Be$ cross section in a paper that contained a detailed quantitative treatment of the reaction networks (see Fowler 1958). Davis sent a reply to both letters that contained a calculation of expected rates. The expected capture rate for ^{8}B neutrinos was calculated using an expression obtained from Ed Kelley, a postdoctoral physicist at Brookhaven National Laboratory a few years earlier. If the cycle gave the full yield of ^{8}B ($4.3 \times 10^{10}/cm^2/s$), there would be 7.7 captures per day per 1000 gal of C_2Cl_4, that is, 3900 SNU! The crest of this wave of optimism was soon to pass, as will be described later, but for a time the wave rolled on and set the immediate course for the future.

At the time these letters were received, a 1000 gal C_2Cl_4 experiment was essentially completed at the reactor site of the Savannah River plant. These experiments were performed under 25 m water equivalent of cosmic ray shielding, and the background of 26 ^{37}Ar atoms per day could be explained by cosmic ray interactions. Clearly, it was necessary to move the chlorine detector to a mine if one wished to observe the solar neutrino signal. However, this could not be done immediately because the work at Savannah River consumed the entire experimental effort. Don Harmer and Davis were building a new 3000 gal experiment that was designed to distinguish between the four component neutrino theory (of Mayer, Telegdi and Preston) and the two component theory. They had been urged to build the larger 3000 gal experiment by W. Pauli in a letter to Maria Mayer.

The 1000 gal tank used in the initial experiment at Savannah River was taken to Brookhaven at the end of 1959. After some minor improvements were made, it was moved to the Barberton Limestone Mine (in Ohio) of the Columbia-Southern Chemical Co. This mine was 2300 fit deep and had an enormous excavated volume, nearly a square mile with 32 ft high ceilings. John Calvin and Davis completed the installation in July, 1960, and completed the first experiments in October. Immediately, they found that the solar neutrino capture rate was low, 3 ± 5 ^{37}Ar atoms/day (< 4000 SNU), but by late 1960 we expected a low flux of ^{8}B neutrinos!

The critical reaction $^{7}Be(p,\gamma)^{8}B$ was studied by Ralph Kavanagh (1960) and the low value (0.027 keV-b) that he found was very disappointing. Kavanagh measured the cross section at two energies, 800 and 1400 keV, by observing the energetic positrons from ^{8}B decay. The whole attitude of Davis (and others) on the possibility of observing solar neutrinos was greatly influenced by this measurement. It was abundantly clear that the detection of solar neutrinos was indeed a difficult problem. The last sentence in the review by Reines (1960) reflected the general view: ``The probability of a negative result even with detectors of thousands or possibly hundreds of thousands of gallons of C_2Cl_4 tends to dissuade experimentalists from making the attempt."

1962

Our collaboration began in 1962. Characteristically, it was initiated by Willy. He was the referee for the paper on beta decay in stellar interiors by Bahcall (1962a) where it was pointed out that electron capture rates in stars could be very different from the terrestrial values that had been used previously in most nuclear astrophysics calculations. Calculations of capture rates from continuum orbits were made including Coulomb effects and the exclusion principle. Willy described these calculations to Davis who then wrote to Bahcall (in February, 1962) asking about the rate of electron capture by ^{7}Be in the Sun. The results for the capture rate of ^{7}Be appeared in Bahcall (1962b). We have been asking each other questions ever since.

1963

The first calculation of the neutrino fluxes obtained from a detailed model of the Sun was presented by Bahcall, Fowler, Iben, and Sears (1963), who evaluated the expected ^{7}Be and ^{8}B neutrino fluxes with the aid of a quantitative model for an evolved Sun. These fluxes corresponded to a capture rate of only 0.01 captures per day in the 1000 gal tank experiment in the Barberton Mine (i.e., 5 SNU with the presently known values for only the ground-state transitions). This calculation did not provide any encouragement to build a larger experiment, because even 100,000 gal would only capture about one neutrino per day according to this estimate.

The collaboration on the calculation of the neutrino fluxes was typical of the strong interactions that characterized science (and partying) at the Kellogg Radiation Laboratory. The model was computed by Dick Sears using an energy-generation subroutine and opacity code originally developed by Icko Iben. The energy-generation routine was improved by Bahcall and Fowler. The neutrino fluxes were computed by hand by Bahcall from the detailed model results. Bahcall had come to Kellogg in the summer of 1962 with the idea of stimulating a collaboration that would make use of the new and more accurate values for the ^{7}Be production and destruction rates (Kavanagh 1960; Bahcall 1962b; and Parker and Kavanagh 1963). Willy was especially important in seeing that the work was actually done. Bahcall, Iben, and Sears were all research fellows in the Kellogg Laboratory, run by Charlie Lauritsen with the active assistance of Willy and Tommy Lauritsen. Iben and Sears were, along with most other astronomers and astrophysicists, more interested in studying evolved stars than in making models of the Sun. High quality measurements by Parker and Kavanagh (1963) of the reaction $^{3}\mathrm{He}(\alpha,\gamma)^{7}\mathrm{Be}$ gave a cross section about a factor of two lower than was deduced from the initial results of Holmgren and Johnston. In an important companion paper, Tombrello and Parker (1963) developed a theoretical model for this reaction which underlies our current understanding of the process. These papers provided an important step in determining more accurately the nuclear physics parameters in the p-p chain.

Davis had been studying for some time the idea of carrying out a large scale solar neutrino experiment with ^{37}Cl. There was not much enthusiasm among astronomers for what was viewed as an expensive experiment and not too much reason to hope that an observation could be performed that would actually detect solar neutrinos.

Even though the prospects for observing solar neutrinos looked dim, Davis was eager to build a 100,000 gal experiment. He has often been asked why this particular size was chosen and the reasons may be of some interest. First, the size was picked to be a hundred times larger than the Barberton experiment because expansion by a factor of 100 appeared feasible. Davis felt that a tank this size could be processed in a reasonable time and trusted that it could be made sufficiently leakproof. The latter specification was necessary in order to prevent the inward leakage of atmospheric argon. The total volume of argon had to be kept small to permit using a proportional counter with a small internal volume (say, 0.5 cm^3). The cosmic ray background was an important consideration requiring a room large enough to contain a 100,000 gal tank at least 4000 ft below the surface. Davis did not know if a suitable mine existed and if so, whether it could be used for a scientific experiment. In early 1963, Blair Munhofen and Davis started looking for deep mines in the United States, even though the theoretical and funding prospects were dismal. James E. Hill of the Bureau of Mines recommended two: the Homestake Gold Mine and the Anaconda Copper Mine. Visits to these mines convinced the Brookhaven scientists that the rock at Homestake at the 4850 ft level would permit the opening up of a cavity large enough to hold the 100,000 gal tank, whereas the rock at the Anaconda would allow only a 14 ft diameter hole at their 4200 ft level. The Anaconda Copper Company was eager for their mine to be used and quoted a very reasonable cost for providing a concrete lined cylindrical hole. However, the Homestake Company estimated a very high cost for a suitable excavation, so it was decided to look for other mines. The Sunshine Silver Mine (Willy loved these names) in Kellogg, Idaho, was considered and their management expressed interest in the project. The depth at the Sunshine Mine was 5400 ft, the rock strength was satisfactory, and their cost estimate reasonable. It seemed that the Sunshine Mine was a suitable location. Thus, although there was no approval for a larger project, there was at least one place where a 10^5 gal experiment could be carried out.

The planning for a solar-neutrino experiment became a practical exercise after Bahcall showed that the expected capture rate for ^{8}B neutrinos was about twenty times larger than previously calculated due to transitions to excited states of ^{37}Ar (especially the superallowed transition from the ground state of ^{37}Cl to the $T = 3/2$ state of ^{37}Ar at about 5 MeV excitation in ^{37}Ar). The idea of considering transitions to excited states was stimulated by a question by Ben Mottelson in a seminar Bahcall gave during the summer of 1963 at the Niels Bohr Institute in Copenhagen.

Our combined results suggesting the feasibility of a 10^5 gal ^{37}Cl experiment were first presented in November of 1963 at an international conference on stellar evolution organized by B. Stromgrem and A. G. W. Cameron at the Institute for Space Studies in New York [the proceedings appeared much later, see Bahcall and Davis (1966)]. It is indicative of the then (and perhaps still prevailing) preference by astronomers for studies of more exotic stages of stellar evolution that neither the possibility described by Bahcall and Davis of a solar neutrino experiment nor the solar models of Sears (1966) were mentioned in the conference summary.

Shortly after this conference, Bahcall visited Brookhaven National Laboratory to describe in a physics department seminar his new results on neutrino capture cross sections for ^{37}Cl, and to join Davis in a crucial discussion with Maurice Goldhaber regarding the desirability of Brookhaven requesting funds to carry out a full scale solar neutrino experiment. Accounts of this meeting have already been published (see Goldhaber 1967; Bahcall 1967). Bahcall and Davis both remember being very worried about the meeting because Goldhaber was the director of Brookhaven and was known to be skeptical of the ability of astronomers to say anything correct about anything interesting. We planned to discuss two points. First, we hoped that the nuclear aspects of the new absorption cross sections might intrigue the director (both Goldhaber and his wife Trudy are distinguished nuclear physicists). Second, we tried to stress that a failure of the theory to predict the correct capture rate in a solar neutrino experiment would be the most scientifically interesting result possible because it would confirm his (Goldhaber's) conviction that astrophysicists did not really know what they were talking about. At the present time, we do not remember what, if anything, Maurice agreed to in this initial meeting. However, in a later published account, Goldhaber agreed publicly with the idea that we had not understood what we were talking about (see Reines and Trimble 1972, pp. d-1 and d-2). Nevertheless, Charlie Lauritsen, who was both one of America's most distinguished scientific statesmen and a good friend of Dick Dodson, then the chairman of the Chemistry Department at Brookhaven (and a research fellow in Kellogg in 1940), was successful in mobilizing important support.

The realization that neutrino capture to the analog state in ^{37}Ar greatly increased the total capture rate made an enormous difference in Davis's view of a 100,000 gal experiment. It seemed to him that the analog state was a beautiful new concept in the present context that should appeal to nuclear physicists. Moreover, the total expected capture rate was increased to about 4 to 9 per day, making the experiment seem more reasonable. Since only the neutrinos from ^{8}B were sufficiently energetic to feed the analog state, the experiment was very sensitive to the magnitude of the flux of ^{8}B neutrinos. The reactions producing ^{8}B were in turn very sensitive to the interior temperatures of the Sun and these temperatures depended (in the models) upon the solar composition and calculated opacities. Thus, the chlorine experiment could be considered as a way of measuring the central temperature of the Sun, which Davis felt should appeal to astrophysicists (Bahcall 1964a set an upper limit of 2×10^{7} K for the central temperature of the Sun using the Barberton results and claimed that a measurement of the ^{8}B solar neutrino flux accurate to 50% would determine the central temperature of the Sun to 10%). The importance of the ^{8}B flux for the chlorine experiment also made clear that the ^{7}Be(p,γ)^{8}B and ^{3}He(^{3}He,2p)^{4}He cross sections had to be remeasured to obtain values of comparable accuracy to the ^{3}He(α,γ)^{7}Be cross section measured by Parker and Kavanagh.

1964

A large fraction of the most important theoretical ideas and suggestions for experiments were first described in this year. We published companion papers in the March issue of *Physical Review Letters* (Bahcall 1964a, Davis 1964). These two articles were originally intended to be one paper but we could not squeeze all we wanted to say into one (letter-sized) article, so we divided the subject into theoretical and experimental aspects of the proposed solar neutrino experiment. Willy urged us to publish these papers to present our plans to the scientific community; he felt this was an essential step toward funding the project. Bahcall's paper described both the calibration of the suggested detector (i.e., the neutrino absorption cross sections) and the neutrino fluxes (from Bahcall, Fowler, Iben, and Sears 1963, and the very important discussion of uncertainties in the values of the calculated fluxes by Sears 1964). This permitted him to make the first quantitative prediction of the rate expected for the ^{37}Cl experiment: (40 ± 20) SNU, where we have used the acronym SNU for 10^{-36} capture per target atom per second. Davis reported the results of a pilot experiment that used 1000 gal of perchloroethylene. The measurements were carried out in the limestone mine of the Pittsburgh Plate Glass Company of Barberton, Ohio, at a depth of 1800 m water equivalent. The limit reported from the 1000 gal experiment was 300 SNU. Davis showed that a tank containing 100,000 gal of perchloroethylene would permit detection of the predicted capture rate and that the expected backgrounds would be small.

There was a flurry of activity and discussion in the United States following these analyses. In one of his letters to Bahcall from this period, Davis mentioned the sometimes unappreciated side benefits of publicity (an article in *Time* had just appeared) in finding a suitable mine for the experiment and procuring a satisfactory tank for the liquid detector: ``. . . these tank people take us more seriously after the article in *Time*."

Pontecorvo told us many years later (at a conference in Leningrad) that he reported on our two papers at a special seminar in the Soviet Union about this time. According to his account, he was the only person present who expressed the opinion that it would be a successful experiment.

Reines and Kropp (1964) reported in April an upper limit on the flux of ^8B neutrinos that was obtained from an experiment designed for other purposes. Their limit referred to elastic scattering of electrons by ^8B neutrinos. The upper limit was equivalent to 1000 SNU. Bahcall (1964b) then showed that the electron-scattering experiment proposed by Reines and Kropp could determine the direction of the incoming neutrinos to better than ten degrees.

Fred Reines and his associates were engaged in building a large scintillation counter system for observing cosmic-ray produced neutrinos. They were working in the deepest mine in the world (10,000 ft), the famous East Rand Proprietary Gold Mine near Johannesburg. Fred's team also built a 4000 gal scintillation counter in this location in order to observe solar neutrinos from ^8B decay (see Reines 1967), an impressive achievement.

A more detailed description of the nuclear physics calculations for the ^{37}Cl experiment was published in Physical Review (Bahcall 1964c). This paper contains also an extensive discussion of possible nuclear physics measurements that could reduce the uncertainties in the calculation of the neutrino absorption cross sections. Figure 1 of this paper is particularly interesting from a historical point of view because it shows that only a few nuclear states were then known in the mass-37 quartet (^{37}Cl, ^{37}Ar, ^{37}K, and ^{37}Ca); by now, hundreds of states are known. Bahcall predicted the existence of a particle-stable ^{37}Ca isotope that would decay by positron emission in the order of 130 ms.

Initially, it was surprisingly difficult to interest any experimental group, with the appropriate facilities, in searching for ^{37}Ca. Vigorous attempts by both of us and Willy were unsuccessful (and discouraging) until Charlie Barnes proposed during a discussion in the coffee room at Kellogg Laboratory, that ^{37}Ca could be best studied experimentally by searching for the delayed protons emitted by the highly excited states of ^{37}K that would be produced by ^{37}Ca positron decay. It was pointed out (Bahcall and Barnes 1964) that the matrix elements from the ground state of ^{37}Ca to the excited states of ^{37}K that are proton emitters are essentially the same matrix elements that are most important for calculating the capture cross section for ^8B neutrinos. Barnes's suggestion of searching for delayed protons provided the experimental twist that stimulated the experiments leading to the present secure estimate of the cross section.

July 1964 was a crucial month for the funding of the 100,000 gal experiment and Willy played an important role in obtaining approval for the project. On July 27, Dick Dodson wrote to Willy describing the budget considerations that were then underway in Washington and requested an authoritative statement from him regarding the importance of doing the experiment. Dick posed the problem in a way that says a lot about the climate of opinion at the time. He wrote, ``I suppose one can reduce, somewhat crudely, the question we need to answer to: why spend a substantial sum trying to measure something which is calculated with great confidence by nuclear astrophysicists - and who cares about confirming the central temperature of the sun anyway?'' Willy answered on July 31 with a department chairman's dream letter. He wrote, ``The Brookhaven solar neutrino experiment has my enthusiastic support.... The observation of solar neutrinos and the detection of the flux at the earth is crucial to further progress in nuclear astrophysics and to related efforts in thermonuclear research and the space sciences.'' Willy went on to describe the relation of the solar neutrino experiment to ``. . . a diverse set of terrestrial experiments and calculations which are of considerable practical importance.'' This formal letter was typed and addressed to ``Dr. Dodson.'' In an accompanying handwritten note addressed to his old friend ``Dick,'' Willy offered to supply further material if it were required.

Two systematic studies of uncertainties in the prediction of solar neutrino fluxes were also carried out during this year. Sears (1964) published a study of the effects of various uncertainties on the solar-model calculations. This was a very significant article that strongly influenced thinking regarding a new experiment. The first sentence of Sears's article contains an interesting apologetic disclaimer to his astronomical colleagues: ``Theoretical models of the internal structure of the Sun are no longer at the frontier of the theory of stellar structure and evolution.'' He concluded that the flux of ^{8}B neutrinos could be estimated to within a factor of two, the primary uncertainty being the initial homogeneous solar composition. Sears calculated a ^{8}B flux of $(3 \pm 1) \times 10^{7}$ cm^{2} s^{-1}. Pochoda and Reeves (1964) also published the results of a calculation of the neutrino fluxes from a solar model constructed by Martin Schwarzschild and Pochoda. In a note added in proof, they pointed out that when Bahcall's neutrino absorption cross sections were used, the capture rate corresponded to 38 SNU. In their excellent article, Pochoda and Reeves also noted that the calculated increase in solar luminosity with time (from the initial main sequence stage to the present) would have deep effects on the history of the solar system, a topic that was much discussed some ten or so years later. A detailed study of the termination of the proton-proton chain was performed by Parker, Bahcall, and Fowler (1964), who investigated a variety of deuterium and helium-burning reactions. Several skeletons in the nuclear closet were unearthed during the course of this later work, the most important being the systematic uncertainties in the then available data on the $^{3}He(^{3}He, 2p)^{4}He$ cross section.

Various aspects of the subject of solar neutrino astronomy were reviewed at the Second Texas Symposium on Relativistic Astrophysics in the middle of December (see the talks in the proceedings by Bahcall 1969a; Davis et al. 1969; and Reines 1969).

Plans to build the solar neutrino experiment in the Sunshine Silver Mine collapsed about a month before the 1964 Texas Conference. Funds were apparently available, but suddenly there was no suitable mine. During the conference, Blair Munhofen returned to the Homestake Mine and asked them to reconsider the project. They quickly reviewed the costs and presented Brookhaven with a very favorable estimate for excavation, $125,000. Homestake provided a detailed design of the rock cavity and was anxious to begin work in the spring of 1965. We were of course very pleased with their plans and their cooperative attitude. As a consequence of the larger facilities, it was necessary to have the tank fabricators, who were also pleased with the larger facilities, rebid for constructing the tank.

Following the Second Texas Conference, G. T. Zatsepin and A. E. Chudakov of the Lebedev Institute of Moscow visited Brookhaven and learned of the detailed plans for the Homestake experiments. These Soviet scientists were very interested in establishing a program of neutrino astronomy in the Soviet Union. They were developing a chlorine experiment and also large scintillation counters. During their visit, Zatsepin gave us a curve showing the calculated cosmic ray muon background for the chlorine experiment as a function of depth underground. It was both gratifying and useful to have an independent calculation of this important parameter (see O. Ryajskaya and G. Zatsepin 1965). The visit of Zatsepin and Chudakov was the first of a number of valuable discussions with these outstanding scientists about problems and developments in neutrino astronomy.

The discovery of ^{37}Ca was reported in companion *Physical Review Letters* in late December. Hardy and Verrall (1964) and Reeder, Poskanzer, and Esterlund (1964) reported independent experiments detecting the delayed protons following the positron decay of ^{37}Ca; the measured lifetime (170 ms) was in satisfactory agreement with the predicted (130 ms) value. Thus, in this one year the issue of the enhanced ^{8}B cross section was raised and settled. Bahcall remembers the phone call from Poskanzer (which appropriately enough came during another Kellogg coffee hour) notifying him of the detection of ^{37}Ca decay with approximately the predicted lifetime as the most exciting and satisfying single moment of his professional career.

In the enthusiasm of the moment, we discussed other possible experiments. Bahcall (1964d) proposed, near the end of the year, a program of neutrino spectroscopy of the solar interior that was to be carried out with a variety of targets. In a remarkable example of accidental prophecy, he suggested, ``If no neutrinos are observed in the Davis-Harmer experiment, it will be even more desirable to try to observe the low-energy (p-p and ^{7}Be) neutrinos." The use of ^{7}Li was also advocated here for the first time.

1965-1967

This period was relatively quiet on the theoretical front. Most astrophysicists were concerned with quasars and other problems that have come to be called high energy astrophysics. If they took notice of solar neutrinos at all, theorists appeared to be waiting for the observations to confirm the predictions. A few independent solar models were published by Ezer and Cameron (1965, 1966) and by Weymann and Sears (1965). It is indicative of the mood of the time among astrophysicists that Weymann and Sears did not calculate neutrino fluxes from their improved solar model. The neutrino fluxes calculated by Ezer and Cameron (1965) for a particular solar model correspond to a capture rate of 15 SNU, using the then available estimates for neutrino absorption cross sections (Bahcall 1964c).

One new idea is worth noting, mainly because of its simplicity. Bahcall (1966) pointed out that the capture rate in SNU could be calculated accurately without the aid of solar models if the Sun were assumed to shine by the CNO cycle. Each conversion of four protons to an alpha particle results, in this case, in the production of one ^{13}N and one ^{15}O neutrino. At the time, Bahcall was concerned that the prediction (of order 30 SNU) from the CNO hypothesis agreed, within the errors, with the prediction obtained from detailed solar models that showed that the proton-proton chain was dominant. He recalculated neutrino fluxes from detailed printouts of solar models generously made available by Ezer and Cameron; Iben, Weymann, and Sears; and Sears, using a somewhat improved computer routine for calculating neutrino fluxes. His result was a capture rate that lay between 15 and 60 SNU, with a best estimate of 30 SNU. It appeared therefore, at the time, that even neutrino observations with a ^{37}Cl detector might not be able to distinguish between the CNO cycle and the p-p chain.

There were a number of important laboratory experiments that measured the low-energy behavior of nuclear reaction processes occurring in the proton-proton chain (see the discussion in Kavanagh 1972). Willy played a major role in encouraging and supporting all of these experiments.

Parker (1966) remeasured, in a classic experiment performed at Brookhaven, the crucial ^{7}Be(p,γ)^{8}B production cross section. Davis prepared the ^{7}Be source for this experiment by a new technique, and Parker employed in his experiment a method that was superior to the earlier procedure of Kavanagh; the new method involved flipping the target, after exposure to the beam, in front of a silicon detector that observed alphas following the decay of ^{8}B. Parker's method also permitted the measurement of the number of ^{7}Li atoms on the target by a d-p reaction on ^{7}Li, which also produced alphas. Both of us were in constant touch with Parker during the long series of tests involved in this experiment since the predicted capture rate for the ^{37}Cl experiment depends almost linearly on the rate of this reaction. Parker's value was about a factor of two greater than obtained previously in Kavanagh's (1960) pioneering study, an encouraging result for the would-be solar neutrino experimentalist.

Much of the experimental and theoretical work during this period concentrated on the ^{3}He(^{3}He,2p)^{4}He reaction (following the realization by Parker et al. (1964) that it was highly uncertain). This work eventually led to improved measurements of the low-energy cross section factor, which led to a predicted neutrino flux from ^{8}B decays that was reduced by a factor of two from the original (1963 and 1964) results. Kavanagh (1972) summarized the experiments by Bacher and Tombrello (unpublished), Dwarakanath, Winkler, and others that ultimately yielded a cross section factor five times larger than the value recommended by Fowler (1954) on the basis of less accurate experiments; the 1954 value was used in the early calculations of solar-neutrino fluxes. Shaviv, Bahcall, and Fowler (1967) stressed that the value of the low-energy cross section factor for this reaction was the ``major nuclear physics uncertainty'' in the prediction of the neutrino capture rate. They computed solar models with values of this parameter that differed by as much as a factor of 50!

Iben, Kalata, and Schwartz (1967) computed the decay rate of ^{7}Be due to the capture of bound electrons, and showed that the bound electron capture rate increases the total electron capture rate in the Sun by about 20%, reducing the predicted proton capture rate by about the same percentage.

Kuzmin (1965) raised again the question of the possible role in the proton-proton chain of the reaction ^{3}He(p,e^{+})^{4}He. This reaction is potentially very important because it emits energetic neutrinos (maximum energy = 18.6 MeV) that have a large cross section for absorption by ^{37}Cl (Bahcall 1964a). Moreover, the rate of this reaction is not as temperature sensitive as are the rates of the reactions that produce ^{8}B neutrinos. The possibility of an additional source of high-energy neutrinos was stimulating to the experimentalists, but the careful analysis of Werntz and Brennan (1967) showed that the cross section for proton capture by ^{3}He in the Sun was so low that the neutrinos produced would be too rare to be observed.

In a short but important paper, Kuzmin (1966) pointed out the advantages of a radiochemical solar neutrino experiment with ^{71}Ga as the detector (see also, Kuzmin and Zatsepin 1966). He stressed the importance of the low threshold which permits the detection of the fundamental proton-proton neutrinos, the large ground-state cross section, the experimentally convenient half life (11.4 days), and the relatively large K-capture energy (12 keV). He wisely did not discuss the availability or expense of the required large amount of gallium, the main problem today in carrying out a solar neutrino experiment with this detector.

The experimental solar neutrino effort was devoted during these two years to building the 100,000 gal chlorine detector. Already by the end of 1964, the Homestake Mining Company had agreed to let Brookhaven build the detector in their mine. Excavation of the rock cavity that they designed for the installation was started in May, 1965 and completed in about two months. It was an exciting day for Blair Munhofen and Davis when they were first shown the 30 × 60 ft room with a 32 ft ceiling. They were brought into the room and immediately started looking around with miner's lamps. Suddenly, the lights were turned on and they could see the enormous room with its walls covered with chain-link fencing, the concrete floor with pedestals for the tank supports, and the monorail for the lifting hoist 32 fit above. The Homestake people were very pleased with the room and delighted that it also pleased the Brookhaven experimenters.

Building of the tank by the Chicago Bridge and Iron Company (CBI) was started in the summer of 1965. Don Harmer, Blair Munhofen, and Ray Davis had visited their plant in Salt Lake City to check the steel plate for the surface alpha emission rate. The alpha yield of ^{37}Ar from perchloroethylene had been measured by dissolving ^{222}Rn in the liquid as an alpha source. Based on these measurements, limits were set on the total acceptable alpha emission rate from the tank walls and in the liquid itself. There was great concern about natural alphas producing ^{37}Ar in the detector by the sequential reactions ^{35}Cl(α,p)^{38}Ar and ^{37}Cl(p,n)^{37}Ar. If sulfur were present in the liquid, ^{37}Ar could also be produced by the ^{34}S(α,n)^{37}Ar reaction. The sulfur reaction was studied with ^{222}Rn dissolved in carbon disulfide (see Davis 1969). After selecting the steel, CBI fabricated the tank parts and shipped them to Homestake. All parts were designed to fit the shaft hoist and mine tunnels. CBI personnel said later that they ordinarily would not have been interested in building a small, rather conventional tank such as was required for the neutrino experiment but they were intrigued by the aims of the project and the unusual location. Another critical feature of the tank and pumping system is that it had to be absolutely leakproof to prevent the inward leakage of atmospheric argon. CBI engineers were experienced in making vacuum leak tests on large vessels with helium, using a mass spectrometer as a detector. They had built many space chambers and large Dewars for NASA. After the tank was complete, it was evacuated and leak tested. The two 500 gal/min liquid pumps were canned rotor pumps, designed and built especially for the experiment by the Chempump Company. Canned rotor pumps have the armature and impeller in a single, sealed can containing the liquid being pumped. This design avoids using a shaft seal, and can therefore be made permanently sealed and leakproof. The liquid is circulated by these pumps through a set of eductors that are used to force helium through the liquid. A system of 40 eductors provides vigorous agitation and a thorough mixing of the helium purge gas with the liquid. The eductor system was designed at Brookhaven and tested in the Brookhaven swimming pool.

During the period when the tank was being designed, Davis received a letter from B. Kuchowicz of the University of Warsaw suggesting that a ^{64}Cu neutrino source could be used to test the neutrino capture cross section calculations. This isotope has a half life of 12.5 h and is too short-lived to be suitable for a practical test. However, his letter prompted the Brookhaven group to modify the internal piping to allow a reentrant well to be installed in the center of the tank. This design feature may yet be very useful. In 1965, Bernard Manowitz of Brookhaven suggested that ^{l52}Eu be used as a calibration source. He pointed out that its long half life and the fact that europium is used as a reactor control rod made it an attractive and available source. The possibility of using an intense neutrino source to test the calculated neutrino capture cross sections and the chemical extraction and counting procedures has been discussed many times since.

The vessel was completed in the summer of 1966. The final step was to thoroughly clean the inside of the tank by shot blasting and scrubbing with solvent. In order to insure that the alpha emission rate from the inside walls of the tank was below the acceptable levels, selected areas of the tank were checked with a windowless proportional alpha counter that could cover an area 60 × 180 cm. Next, the cover flange was installed and the filling began. Ten railroad tank cars of perchloroethylene were brought one by one to the head of the shaft. Dutch Stoenner of Brookhaven had previously checked the alpha content of the perchloroethylene in samples from each tank car before it left the Frontier Chemical Company's plant in Wichita, Kansas. The liquid was transported to the 100,000 gal tank nearly a mile underground by a set of three 650 gal tank cars designed to fit the hoist and mine rail system. The work was completed in five weeks with the aid of the Homestake hoistman and five Brookhavenites. Then, the processing system was installed and a long series of preliminary purges were carried out to remove dissolved air and reduce the amount of atmospheric argon present to less than a few tenths of a cm^3. Once this was accomplished, it seemed clear that the detector would indeed work as planned and a sensitive measurement of the solar neutrino flux could be made.

In the course of removing atmospheric argon from the tank, a relatively small sample of argon of about 6 cm^3 was finally obtained and brought back to Brookhaven. There was little interest in this sample because it was too large to put in the small counters. However, Dick Dodson suggested that the sample should be measured somehow, as it was the first sample from the tank. It was measured in a relatively large counter and surprisingly, the rate was 6 counts/min! The high level of activity was explained by the presence of ^{85}Kr in the sample from atmospheric krypton dissolved in the huge volume of perchloroethylene. The krypton was easily removed by gas chromatography. Dutch Stoenner showed Davis how to set up a simple gas chromatograph with a charcoal column, and this became an essential step in the gas purification procedure, not only to remove ^{85}Kr but also ^{222}Rn. This part of the story would be grossly incomplete without at least some mention of some of the other people who helped to make it happen. During the design and building phase, Don Harmer spent a year at Brookhaven helping on the solar neutrino experiment while on leave from Georgia Tech. Kenneth Hoffman, a young but very experienced engineer, provided guidance through a number of problems. The Homestake Mining Company and the Chicago Bridge and Iron Company provided excellent cooperation throughout the building phase; the Homestake company has continued to be an active partner in the operation of the detector they helped build. The Homestake people that have been directly connected with the project are the mine supervisors Donald Delicate and Joel Waterland, the research and planning engineer Albert Gilles, and James Dunn of the public relations office. During the construction phase, Jim Dunn and Don Howe devoted four issues of the company magazine, *Sharp Bits*, to the project. All of these men contributed in an important way to the solar neutrino experiment by their enthusiastic support, valuable advice, and direct help.

Meanwhile, an active program was underway to develop direct counting detectors for observing the energetic neutrinos from ^{8}B decay in the Sun. One of these experiments, the 4000 l scintillation counter designed to detect solar neutrinos by elastic scattering, has already been mentioned. A second detector used ^{7}Li as a neutrino absorber in the form of a half ton of lithium metal (see Reines and Woods 1965). A third detector was built by Tom Jenkins and his associates at Case Western Reserve. This experiment contained 2000 l of D_2O and was designed to be operated as a Cerenkov detector for the electrons produced by neutrino capture by deuterium. All of these detectors were built (see Reines 1967) but were eventually abandoned after the chlorine experiment showed that the ^{8}B flux was low.

1968

The last systematic theoretical calculation of the solar neutrino fluxes to appear before the first experimental results were obtained was a detailed paper on the uncertainties in the predicted rate by Bahcall and Shaviv (1968). These authors varied all of the parameters within the limits that were then believed to be a plausible range of uncertainties and obtained a predicted capture rate (assuming the uncertainties combined as statistical errors) that lay between 8 SNU and 29 SNU. The lower values calculated in this paper were primarily the result of the much increased estimate for the cross-section factor for the ^{3}He-^{3}He reaction, as discussed above. In a short note, Bahcall, Bahcall, Fowler, and Shaviv (1968) pointed out that the cross-section factor for the ^{3}He(α,γ)^{7}Be reaction was also highly uncertain and derived predicted capture rates that lay between 7 and 49 SNU. In a statement that could have been written today, these authors closed their paper with the following exhortation: ``We urge that additional low-energy cross section measurements be made for the reactions ^{3}He(α,γ)^{7}Be and ^{7}Be(p,γ)^{8}B in order to reduce the large uncertainty in the predictions for the neutrino experiments designed to test the theory of nuclear energy generation in stars." The major results for both the observations and the revised theoretical estimates were presented again in a pair of papers in *Physical Review Letters* (Davis, Harmer, and Hoffman 1968; Bahcall, Bahcall, and Shaviv 1968). It is surprising to us, and perhaps more than a little disappointing, to realize that there has been very little quantitative change in either the observations or the standard theory since these papers appeared, despite a dozen years of reexamination and continuous effort to improve details (see Figs. 1-3).

The first results for the search for solar neutrinos by Davis, Harmer, and Hoffman (1968) yielded an upper limit of 3 SNU, based on the results of the initial two runs. The operating experimental system was described in this paper as well as various tests and limits on the backgrounds, the recovery efficiency, and the counting efficiency.

The accompanying theoretical paper by Bahcall, Bahcall, and Shaviv (1968) gave a most probable rate of (7.5 ± 3) SNU, with specified assumptions regarding the uncertainties in various parameters. Despite the subsequent careful examination of dozens of effects and parameters, documented in many highly detailed papers, the best estimate for the capture rate predicted by the current standard solar model has never fallen outside this range (although there have been many fluctuations up and down within the quoted range). When these papers appeared, Ed Salpeter wrote an incisive review of the experimental and theoretical results in *Comments on Nuclear and Particle Physics* (Salpeter 1968). His review contained the wise and felicitously worded summary statement: ``Thus, at the present time, we neither have a positive identification of solar neutrinos nor the morbid satisfaction of predicting a scandal in stellar evolution theory!"

It is instructive to compare the 1968 calculation with the earlier results from 1963 and 1964 (see Bahcall et al. 1963, Sears 1964; and Bahcall 1964a). The 1968 calculation took account of the larger cross-section factor for the ^{3}He-^{3}He reaction discussed above (a reduction factor of about 0.6); a more detailed calculation of the proton-proton reaction rate by Bahcall and May (1968) who also made use of a more accurate measurement of the lifetime of the neutron by Christensen et al. (1967) (all of which resulted in a reduction factor of about 0.7); and an improved determination by Lambert and Warner (1968a,b) of the heavy element to hydrogen ratio on the surface of the Sun (a reduction factor of 0.5 using their new value of $Z/X = 0.02$). These three changes were all in the same direction and resulted in a net reduction factor of $0.6 \times 0.7 \times 0.5 = 0.2$, that is, a reduction from about 40 SNU to an estimated 7 or 8 SNU.

Another discussion of the first experimental results was given by Iben (1968), who constructed a large number of solar models in order to illustrate the parameter dependence of the neutrino fluxes. Iben used the primordial helium abundance by mass, *Y*. as a composition parameter to be varied in obtaining consistency with the results of the solar neutrino experiment. He deduced an upper limit of $Y = 0.16$ for the primordial helium abundance by demanding consistency of the neutrino results with standard solar models and the accepted values of the nuclear parameters. His inferences differed from the parallel investigation of Bahcall and Shaviv (1968) who chose the photospheric ratio of heavy element to hydrogen abundance to be their composition parameter (determined by observation) and found values of the helium abundance consistent with other astronomical determinations (albeit in conflict with the solar neutrino observations for the best-guess model parameters).

Ezer and Cameron (1968) made the first serious proposal of a nonstandard solar model that would be consistent with the observed upper limit on the neutrino capture rate. They suggested that the Sun was thoroughly mixed, fresh hydrogen being continually brought into the central regions. This process would permit the proton-proton chain to proceed at a lower central temperature than in standard models and could reduce the predicted capture rate in extreme cases to one-fourth the value calculated for the standard models. Arguments were immediately given against the likelihood of maintaining such extreme mixing (see Bahcall, Bahcall, and Ulrich 1968; Shaviv and Salpeter 1968), but the idea was an important one because it was the forerunner of many related suggestions.

G. Zatsepin, one of the earliest and most influential enthusiasts for solar neutrino experiments, organized an international conference on neutrino physics and astrophysics in Moscow in September, 1968. Bahcall (1966b), Davis (1969), and Kuzmin and Zatsepin (1969) all wrote summaries of various aspects of the solar neutrino problem for this conference. The Moscow meeting was an occasion for discussing informally what to do next, given the recently discovered discrepancy between theory and observation. Davis gave a detailed account of the 100,000 gal experiment including the design of the detector, the tests of the recovery efficiency, the counting procedure, and the first observational results (capture rate less than 3 SNU). During the conference, a number of young Soviet physicists asked many questions about the details of the design. It was clear that Zatsepin's group was actively engaged in building a chlorine detector in the Soviet Union. Bahcall was not able to attend because his first child was born only nine days after the conference concluded (Bahcall's paper, including the jokes, was read to the conference somewhat uncomfortably by Davis). In lieu of being able to express an informal opinion in person, Bahcall wrote in his manuscript:

> It seems to me most likely that nature has been nasty to us and that some of the experimentally-measured parameters, S_{17}, S_{11}, Z, and perhaps others . . .are different than we originally believed. I feel especially uncertain about the extrapolated value for S_{17}. I think, however,

Davis will ultimately measure (provided a lower sensitivity is possible) a capture rate between 1 SNU and 3 SNU; otherwise there will be a serious conflict with the theory of stellar interiors.

Kuzmin and Zatsepin (1969) expressed very similar attitudes, stressing the need to remove the uncertainties in the experimental values of S_{17} and S_{34} before conclusions could be drawn regarding the possible astrophysical importance of the discrepancy between the predicted capture rate and the observed limit. They drew special attention to the broad spread in values of S_{17} that resulted when this cross section factor was determined in different ways (cf. Kavanagh 1960; Parker 1968; and Tombrello 1965).

The conference proceedings also reflect some of the fun we have had with our subject and our colleagues. When asked by A. Wolfendale the cost of the experiment, Davis replied: ``Ten minutes time on commercial television ($600,000)." Also, the text of a carefully drafted and detailed bet between the late Jon Mathews (Professor of Theoretical Physics, Caltech) and Bahcall was shown; Bahcall agreed to pay Mathews two dollars if an upper limit of less than 1 SNU was established in the ^{37}Cl experiment. It should be obvious from the above financial data that, at least in this subject, we have always valued experiments more highly than theory.

1969-1977

This period was devoted largely, both theoretically and experimentally, to the reexamination and validation of inferences whose basic outline had been established in the previous five years. For example, in response to the urgings of Bahcall, Bahcall, Fowler, and Shaviv (1968), Kavanagh and his collaborators at Kellogg remeasured the ^{7}Be(p,γ)^{8}B cross section in great detail and with improved precision down to a proton energy of 164 keV, extending and confirming Parker's earlier results and thereby greatly increasing our confidence in the low energy extrapolation of the rate of this crucial reaction. Shortly afterward, Dwarakanath (1974) returned to Kellogg and, in a real tour de force, managed to push the ^{3}He-^{3}He cross section measurements down to 33 keV (0.1 nb), showing that the earlier extrapolation continued smoothly to low energies and that there was no evidence for a low lying threshold resonance.

There were also many suggestions of possible solutions to the solar neutrino problem, none of which has been accepted generally and nearly all of which were either ad hoc or were discredited by further analysis, or both. It is possible that the correct solution is one of the suggestions that were made during this period, but if so it is still very difficult for us to guess which one this might be. Hence, we will content ourselves here with simply recalling some of the more interesting (or, in some cases, more amusing) proposals [for discussions of many of these nonstandard models, see Bahcall and Sears (1972) or Rood (1978)].

Proposals made during this period include: turbulent diffusion of ^{3}He (Schatzman 1969); neutrino oscillations (Gribov and Pontecorvo 1969; Wolfenstein 1978); an overabundance of ^{3}He in the present Sun (Kocharov and Starbunov 1970); the effect of a magnetic field (Abraham and Iben 1971; Bahcall and Ulrich 1971; Bartenwerfer 1973; and Parker 1974); a secular instability such that the presently observed solar luminosity does not equal the current energy-generation rate (Fowler 1968, 1972; Sheldon 1969); quark catalysis (Libby and Thomas 1969; Salpeter 1970); a very low heavy element abundance in the solar interior (Bahcall and Ulrich 1971); an appreciable magnetic moment for the neutrino (Cisneros 1971); an instability of the Sun that makes now a special time (Fowler 1972; Dilke and Gough 1972); neutrino decay (Bahcall, Cabibbo and Yahil 1972); a low-energy resonance in the ^{3}He-^{3}He reaction (Fowler 1972; Fetisov and Kopysov 1972); rapid rotation of the solar interior (Demarque, Mengel, and Sweigert 1973; Roxburgh 1974; and Rood and Ulrich 1974); rotation plus magnetic fields (Snell, Wheeler, and Wilson 1976); a burned-out Sun with a helium core (Prentice 1973); a half-solar mass core of large heavy element abundance that survived the big bang and subsequently accreted another half solar mass at the time of the formation of the solar system (Hoyle 1975); a departure from the Maxwellian distribution (Clayton et al. 1975); a fractionation of the primordial hydrogen and helium (Wheeler and Cameron 1975); accretion onto a black hole in the center of the Sun (Clayton, Newman, and Talbot 1975); and multiplicative mass creation (Maeder 1977).

This list of published suggestions is certainly incomplete; we have not attempted a full literature search. In any event, it does show that during the period under consideration many astronomers and physicists thought seriously about the solar neutrino problem, a situation in marked contrast to what occurred in the first few active years of the subject (1963-1966).

A valuable conference on the solar neutrino problem was organized by Fred Reines at the Irvine campus of the University of California in February, 1972 (Reines and Trimble 1972; Trimble and Reines 1973). The emphasis of this conference was on the experimental aspects of the problem and there was a thorough examination of the details of the chlorine experiment. The prospects for future experiments were also discussed. The first day of the meeting was held in the conference room of President Nixon's Western White House; this was an unusual touch provided by Fred's always active imagination and skill with arrangements. Shortly after this conference, two interesting theoretical suggestions were published: Willy's speculation (Fowler 1972) that there might be a resonance in the 3H-^{3}He reaction; and Al Cameron's analysis of the effect of a sudden mixing of the solar interior on the ^{8}B neutrino flux (Cameron 1973). Another important idea that may have been stimulated by this conference was the proposal by Luis Alvarez that the chlorine detection system could be tested by using an intense radioactive source of ^{65}Zn (Alvarez 1973). He pointed out that a strong monoenergetic source of neutrinos could be prepared by neutron activation of ^{64}Zn. (We are currently discussing a detailed plan to carry out this experiment.)

We next review briefly a few events that occurred during this period in which we were personally involved. We will depart from a strictly chronological order to more logically group some related developments.

The SNU was first introduced by Bahcall (1969c) in a paper that argued that another solar neutrino experiment (preferably ^{7}Li) was needed to decide if the discrepancy between theory and observation was due to a deficiency in our astrophysical understanding or to an unknown phenomenon affecting neutrinos in transit from the Sun.

Gribov and Pontecorvo (1969) suggested that a factor of two discrepancy between theory and observation might be due to oscillations between electron- and muon-neutrino states. They presented the relevant equations for this two-component system, expanding upon the earlier, less formal discussion of Pontecorvo (1968). Gribov and Pontecorvo discussed two kinds of averages of the neutrino fluxes: an average over the emitting region of the Sun and an average (suggested by I. Pomeranchuk) over the time of reception.

Bahcall and Frautschi (1969) pointed out that another average, over the broad spectrum of neutrino energies produced by the Sun, was more important and that some of the variations discussed by Gribov and Pontecorvo would not occur. Bahcall and Frautschi recommended new solar neutrino experiments that were designed to detect p-p or p-e-p neutrinos for which the uncertainties in the predicted fluxes arising from astrophysical considerations are minimal. They argued that such experiments would be sensitive to neutrino masses of order 10^{-6} eV and hence, could discriminate between different elementary-particle explanations of neutrino mixing.

In a more contemporary context, Bilenky and Pontecorvo (1978) have argued that ``. . . neutrino mixing is much more natural solution than any other that has been proposed either in terms of elementary particle physics or astrophysics." They also state: ``From the elementary particle physics point of view, lepton mixing is a reasonably likely and quite attractive hypothesis." They go on to note that: ``. . . neutrino oscillations were not invented ad hoc, for the sake of explaining the result of the experiment of Davis et al."

We would love to test the oscillation hypothesis with a gallium experiment.

The sensitivity of the solar-neutrino fluxes to small changes in opacity, the equation of state, and nuclear cross sections, solar age, and heavy-element abundance was the subject of a detailed study by Bahcall, Bahcall, and Ulrich (1969). The convenient formulae given in this paper for the dependences of the predicted capture rate on various quantities have been used often by us and others to make quick estimates of the possible importance of various uncertainties in parameters and/or of some proposed solutions to the solar neutrino problem. (This paper was the first in a long series of happy collaborations involving J. Bahcall and Roger Ulrich that is continuing even today.) Somewhat similar results were obtained by Torres-Peimbert, Simpson, and Ulrich (1969) using the Berkeley stellar-evolution program. These authors stressed that the primordial heavy element abundance of the Sun, Z, must be less than 0.02 in order for there to be any hope of obtaining with standard models a neutrino capture rate that was not in obvious disagreement with the observed upper limit.

Detailed studies of the effects of various changes in composition, as well as the importance of magnetic fields and turbulent diffusion, were considered also by Abraham and Iben (1971) and Bahcall and Ulrich (1971). The neutrino fluxes calculated by all the active groups using different stellar evolution computer codes were shown by Bahcall and Ulrich to give consistent answers when proper account was taken of the different choices of parameters. New radiative opacities calculated by the Los Alamos group were used by Bahcall, Huebner, Magee, Merts, and Ulrich (1973) to obtain a standard model that predicted a rather low neutrino capture rate, 5.5 SNU. This paper was the first one on solar neutrinos where the calculators of opacities (here, Huebner, Magee, and Merts), whose results had long been recognized as central to the whole subject, were coauthors of a paper specifically on the solar neutrino problem.

Various corrections to the stellar opacity were discussed by Watson (1969a, 1969b, 1969c), including an increased iron abundance. These corrections, among others, were included in the models of Bahcall and Ulrich (1970), who obtained a best estimate of 7.8 SNU.

In December, 1970, we both attended the Symposium on Relativistic Astrophysics in Austin, Texas. The main excitement at this symposium was related to the possibility that Joe Weber had detected pulses of gravitational radiation from distant sources. Searches for radio and microwave signals coincident with Weber pulses were described in detail in a number of interesting talks. During one of these talks, it occurred to us that the ^{37}Cl solar neutrino detector could provide a useful limit on the amount of neutrino energy reaching the Earth that might be associated with Weber pulses. Afterwards, we retreated to a table at a nearby coffee shop and were able to derive quickly a strong limit on the ratio of neutrino energy flux to gravitational energy flux (0.1% for 10 MeV neutrinos, see Bahcall and Davis 1971). The signal to noise ratio of the ^{37}Cl detector is so large (for energy fluxes comparable to those claimed in Weber's gravity wave experiments) that one does not need to perform coincidence experiments (which are necessary in the radio and microwave regions).

We also collaborated with John Evans in a similar study of a reported possible detection of antineutrinos from a stellar collapse by the University of Pennsylvania group (Lande et al. 1974). The absence of detectable neutrinos associated with the January 4 antineutrino event was shown (Evans, Davis, and Bahcall 1974) to be difficult to reconcile with the suggestion that a collapsed star had been detected. We continue to believe that solar neutrino experiments are good detectors for collapsing stars and that their use for this purpose complements the more specific experiments that are being carried out with gravitational wave detectors.

The close connection that has existed between theory and observation (or, perhaps more correctly, between cocktail-hour suggestions and observation) is illustrated by the genesis of the proposal (Bahcall, Cabibbo, and Yahil 1972) that neutrinos may be unstable (i.e., may decay to some other particles). This idea was considered because Davis told Bahcall in a telephone conversation (and in a subsequent memorandum) in November, 1971 of the latest run that he was counting where no counts (either background or signal) had been observed in the counter for two months. This result suggested that the production rate in the tank might well be shown to be zero when counters with sufficiently low background rates were generally available. This possibility made it natural to consider various Lagrangians for which neutrino decay was allowed. The idea of neutrino decay as an explanation for a low neutrino rate has somewhat fallen out of favor in recent years because it appears that a finite production rate in the tank may have been observed (the November, 1971 result was a statistical fluctuation).

There were a number of experimental developments in this period that ultimately increased dramatically the sensitivity of the experiment. Of course, the detector size was already fixed and the chemical recovery was nearly quantitative. The only possibility of increasing the sensitivity of the Homestake detector was to somehow reduce backgrounds. The background counting rate was 10 counts per month in the ^{37}Ar region (full width at half maximum) and a nearly zero background rate was needed for a really substantial improvement in sensitivity.

The crucial suggestion that led to a dramatic improvement was made by Gordon Garmire. After a seminar at Caltech, we went for a swim at the campus pool. Lounging around the side of the pool, we started talking to Gordon. He pointed out that x-ray astronomers had developed pulse rise-time techniques for proportional counters that enabled them to observe and characterize x-ray events in the presence of a high flux of cosmic rays. He suggested that this same technique could be used for characterizing ^{37}Ar decay events in the small Brookhaven counters. When Davis first asked the Brookhaven electronics engineers if this might be possible, they pointed out that their amplifiers were not fast enough to be used for this purpose with the small counters. However, about a year later, they developed amplifiers and pulse stretchers with sufficient speed to be applicable. The first working system was developed at Brookhaven by Robert L. Chase, Veljko Radeka, and Lee C. Rogers, and was used in run number 18 (cf. Fig. 3) in late 1970. This improvement reduced the background counting rate for events simulating ^{37}Ar to one event per month.

With this reduction in counter background, the background ^{37}Ar produced in the tank became an important consideration. The production of ^{37}Ar by fast neutrons from the surrounding rock wall was very small, approximately 0.04 ^{37}Ar atoms per day. This background effect was easily eliminated by flooding the rock cavity with water. The water was added for run number 21 (cf. Fig. 3) in the summer of 1971. The water shield has remained in place since that time, except for a six month period in 1975 when it was removed in order to inspect the tank for corrosion and to give it a new coat of paint. (The water shield has been converted to an active water Cerenkov particle detector by Ken Lande's group from the University of Pennsylvania. Their 250 ton detector is being used currently to search for baryon decay, to observe cosmic rays, and to try to detect neutrinos from collapsing stars.)

The cosmic ray background was known to be small, but by now we had also learned that the solar neutrino signal was very low. Evaluating the cosmic ray background was a difficult problem. It required measurements of the ^{37}Ar production rate by cosmic ray muons as a function of depth and a valid extrapolation of these measured rates to the full depth of the chlorine detector. Measurements were performed at depths from 30 to 108 kg/cm^2. Arnold Wolfendale and E. C. M. Young from Durham analyzed the results to give the extrapolated ^{37}Ar production rate in the detector at 440 kg/cm^2 (Wolfendale et al. 1972). Later, an independent analysis was made by George Cassidy of the University of Utah; he obtained similar results (Cassidy 1973).

In recent years, Ed Fireman has developed an independent technique for determining the muon background by using ^{39}K as a target and detecting events in which ^{37}Ar and a neutron and a proton are produced. These important measurements are being carried out in the Homestake Mine.

Still another method of scaling the background effect with depth makes use of a radiochemical neutron detector based upon the $^{40}Ca((\alpha,n)^{37}Ar$ reaction. The Brookhaven group is currently using a 2000 l tank of calcium nitrate solution exposed at various levels in the mine to measure the neutron production rate by fast muons. In reporting the results of the chlorine solar neutrino detector, a cosmic-ray background of 0.08 ^{37}Ar atoms/day is used in all of the recent analyses; this is the value resulting from the original studies by Wolfendale, Young, and Cassidy, based on the measurements made with perchloroethylene.

The ^{37}Ar counting system was moved to the Homestake Mine in 1977. Moving the system underground did not reduce counter background as much as was hoped. However, the underground muon flux is negligibly small in the counters, and this permitted the measurement of the environmental gamma-ray background effect on the counters. These measurements of the gamma-ray background may eventually lead to further reduction of the counter backgrounds.

There have been worries expressed by physicists and astronomers that there could be something wrong with the radiochemical procedures used for extracting a few tens of atoms of ^{37}Ar from a large volume of perchloroethylene, a typical concentration of one atom in 10,000 l. Some individuals speculated that the ^{37}Ar produced by neutrino captures ends up in a chemical state that is nonvolatile and thus, is not removed by a helium purge. Some specific suggestions were advanced by Kenneth Jacobs; he proposed molecule-ions and radiation induced polymerization traps (Jacobs 1973).

Although these suggestions were not based upon sound chemistry, we felt that an experiment should be performed to test these unlikely possibilities. To this end, an experiment was performed with ^{36}Cl-labeled perchloroethylene. This isotope decays by β-emission to produce ^{36}Ar. The dynamics of the decay process is essentially identical to a neutrino capture and electron emission. In an experiment performed by Herman Vera-Ruiz, John Evans, and Ray Davis, it was found that the yield of ^{36}Ar recovered from perchloroethylene by a helium purge was quantitative. This experiment and the other argon efficiency tests made with the 100,000 gal tank show that ^{37}Ar is recovered with high efficiency.

The Soviet solar neutrino project has developed into a major program under the leadership of G. T. Zatsepin. We first learned of the magnitude of their effort at a lunch table discussion during the Neutrino 1974 conference at Balatonfured, Hungary. A group of interested Americans, Fred Reines, Ken Lande, John Bahcall, and Ray Davis, asked to hear about the Soviet plans. Answers were provided by Ya Chudakov, A. Pomanski, V. A. Kuzmin, and B. Pontecorvo. They outlined their plans to dig a 4 km long tunnel under a mountain in the Caucasus to contain a number of neutrino detectors, including a chlorine detector about five times larger than the Homestake experiment and a 1000 ton scintillation detector for observing collapsing stars. (This ambitious program is well advanced at the present time. The Neutrino 1977 conference was held on site and at that time the tunnel was about 1.7 km deep. At the Neutrino 1980 conference, the Soviet group indicated that a 50 ton gallium experiment would be operating in 1983!)

A summary of the solar neutrino problem, as we saw it after 15 years of work in collaboration with many colleagues, was published in *Science* (Bahcall and Davis 1976). The origin of this paper is somewhat unusual: it was originally solicited by the editor of the British journal, *The New Scientist*, but the manuscript we produced was rejected by the editor as unsuitable for his readership. We then submitted the manuscript to the editor of *Science* who graciously accepted it.

1978 to present

Our efforts, along with those of many colleagues, have concentrated in recent years on bringing about a new solar neutrino experiment. The most promising targets at present appear to be ^{7}Li, ^{37}Cl (enlarged experiment), ^{71}Ga, ^{81}Br, ^{115}In, and electron-neutrino scattering. The current status of the subject and discussions of each of these targets are summarized in the remarks by the various speakers whose talks are recorded in the *Proceedings of the Informal Conference on the Status and Future of Solar Neutrino Research* (Friedlander 1978), which took place at Brookhaven National Laboratory in January, 1978. This meeting was an occasion for examining the present status of the subject and for informally discussing what ought to be done next. Davis (1978) opened the conference by describing the technical details of the ^{37}Cl experiment, including tests which showed that any argon produced in the tank was extracted, as well as an explanation of the counting and data analysis techniques. The experimental rate reported was 2.2 ± 0.3 SNU. There was much lively discussion following this and the other talks. The theoretical calibration of each of the possible new targets was discussed by Bahcall at the conference (Bahcall 1978a), and more completely in a detailed paper (Bahcall 1978b) that gives the best estimates of the neutrino absorption cross sections for the various targets, the estimated uncertainties in the cross sections, and an analysis of what can be learned about astrophysics (or physics) by using each target. In some cases, experiments that had been the subject of much previous work were dropped as a result of Bahcall's analysis, which showed that for several otherwise useful neutrino targets, the absorption cross sections were inherently uncertain. Willy skillfully guided a panel discussion on the final day among several of the participants; the discussion showed remarkable unanimity in supporting a new experiment that would be sensitive primarily to neutrinos from the proton-proton reaction.

The important story of the proposed ^{205}Tl experiment has been told in a complete and dramatic manner by Mel Freedman (1978), who suggested and worked out the observational details of this clever detection scheme in collaboration with his colleagues at Argonne (see Freedman et al. 1976). The basic idea is to use ^{205}Tl as a measure of the average neutrino flux from p-p reactions (or almost equivalently, the solar luminosity) for the past ten million years. The ^{205}Tl must be obtained from geological deposits. A recent reinvestigation of this suggested experiment by Rowley, Cleveland, Davis, Hampel, and Kirsten (1980) has confirmed Freedman's analysis that background effects are expected to be small and that this detector could provide in principle otherwise inaccessible information regarding the average luminosity of the Sun over timescales that some theorists (including Willy) have speculated may be relevant to the internal history of the Sun and to the solar neutrino problem. The principal difficulty with the proposed experiment is that Bahcall (1978a,b) has shown that the neutrino absorption cross sections cannot be calculated accurately for this target and are essentially unknowable to the desired precision (a factor of two or better).

The basic reason that a new experiment is required is to establish whether the origin of the present discrepancy between theory and observation (with the ^{37}Cl detector) is due to errors in our understanding of astrophysics (stellar models) or physics (e.g., properties of neutrinos). Detectors such as ^{71}Ga and ^{115}In (Raghavan 1976) that are primarily sensitive to p-p neutrinos are preferable for this purpose. If the Sun is currently producing its average luminosity by virtue of nuclear fusion reactions in its interior, then the flux of p-p neutrinos can be calculated essentially from the observed optical luminosity of the Sun. One need only assume for this calculation that the branches involving ^{7}Be are relatively rare (less than, or of order of 10%), an assumption that can be justified on the basis of either stellar models or the ^{37}Cl experiment.

A modular experiment that uses ^{71}Ga as a detector is currently underway. It involves a collaboration between individuals at Brookhaven National Laboratory, the Institute for Advanced Study, the University of Pennsylvania, the Max Planck Institute for Nuclear Physics at Heidelberg, and the Weizman Institute in Rehovot. The rationale and procedures for this project were summarized in an article in *Physical Review Letters* (Bahcall et al. 1978) and in the proposal submitted to the *Max Planck Gesellschaft zur Forderung der Wissenschaften* (September, 1978). The extraction procedure has been tested successfully in the spring of 1980 on a 1.3 ton sample of gallium by Bruce Cleveland, Israel Dostrovsky, Gerhart Friedlander, and Davis; the procedures for counting ^{71}Ge efficiently have been developed by the group at the Max Planck Institute for Nuclear Physics in Heidelberg under the leadership of Til Kirsten and Wolfgang Hampel. This experiment could be completed in three or four years if support were forthcoming from the U.S. Department of Energy to supply, together with the Max Planck Institutes, the required amount of gallium (about 50 tons total).

The next stage in the gallium experiment is to use a ^{65}Zn source to calibrate the detector throughput and the neutrino absorption cross sections, in a manner first proposed by Luis Alvarez (1973) for the ^{37}Cl detector. About 10 tons of gallium are required for this intermediate step. The recent suggestions that neutrinos may have been observed to oscillate on scales observable in the laboratory (see, e.g., Barger et al. 1980; Reines et al. 1980) make this experiment of special interest. Only the 1.343 MeV neutrino line of ^{65}Zn (from electron capture) contributes significantly to the observable neutrino capture rate (Bahcall 1978b). If oscillations do occur at the suggested level (neutrino masses of order 1 eV), then the oscillation parameters could be determined by using the effectively monoenergetic ^{65}Zn neutrinos and varying the source-absorber distance.

Other experiments are also being actively investigated at present. These include the following targets: ^{7}Li (K. Rowley, S. Hurst, S. D. Kramer, R. Davis, A. M. Bakich, L. S. Peak); ^{115}In (R. S. Raghavan and M. Deutsch); and ^{81}Br (S. Hurst, Bahcall and Davis, following the recent important experiment on the beta-decay rate of the 190 keV excited state of ^{81}Kr by Benett et al. 1981).

Retrospective

It is instructive to look back over the history of this subject to see how the observational and theoretical values have changed with time. This is shown in Figs. 1 and 2.

Figure 1 is an overall pictorial history of the subject as it looked in 1970 (when this drawing was originally used by Davis in a public lecture). A few of the major events are indicated on the figure at the period corresponding to the time they occurred. It is interesting to note that the only change that would have to be made to bring it up to date ten years later is to lower the experimental upper limit by about a factor of two.

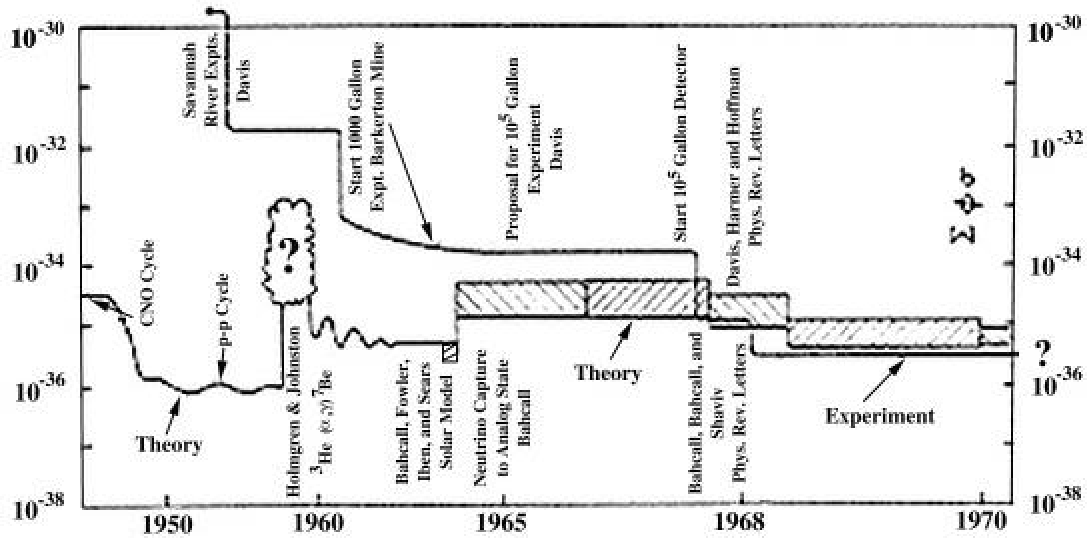

Figure 1. some of the principal events in the development of the solar neutrino problem. The experimental upper limit is indicated by the thin black curve and the range of theoretical values (after 1964) by the cross-hatched region. The units are captures per target atom per second (10^{-36} capture/target atom/s = 1 SNU).

Figure 2 shows all the published values in which we have participated, with the exception of the observational limits obtained in 1955 and 1964 (These earliest upper limits of 4000 and 160 SNU would not fit conveniently in Fig. 2, which unlike Fig. 1, has linear scales).

A few remarks need to be made about the theoretical error bars in Fig. 2. These uncertainties are more ``experimental'' than ``theoretical'' since the basic theory has not changed since 1964. What have changed are the best estimates for many different input parameters (see the earlier discussion under 1968). The error bars shown in Fig. 2 for the theoretical points were taken in all cases from the original papers (see caption to Fig. 2), and represent the range of capture rates that were obtained from standard solar models when the various nuclear and atomic parameters were allowed to vary over the range conventionally regarded as acceptable at the time the calculations were made. A number of detailed theoretical studies and improvements have been introduced into the stellar model calculations over the past fifteen years, at great expense in personal effort and computing time, but these theoretical refinements have had only relatively minor effects on the calculated capture rates compared to the rather large changes produced by new measurements of experimental parameters. The various ups and downs in the best estimate theoretical values since 1968 represent the largely statistical variations in the uncertainties in the many input parameters. The current theoretical estimate is (7.5 ± 1.5) SNU, where the quoted uncertainty takes account of known uncertainties in opacities, primordial chemical composition, and nuclear reaction parameters (Bahcall et al. 1980).

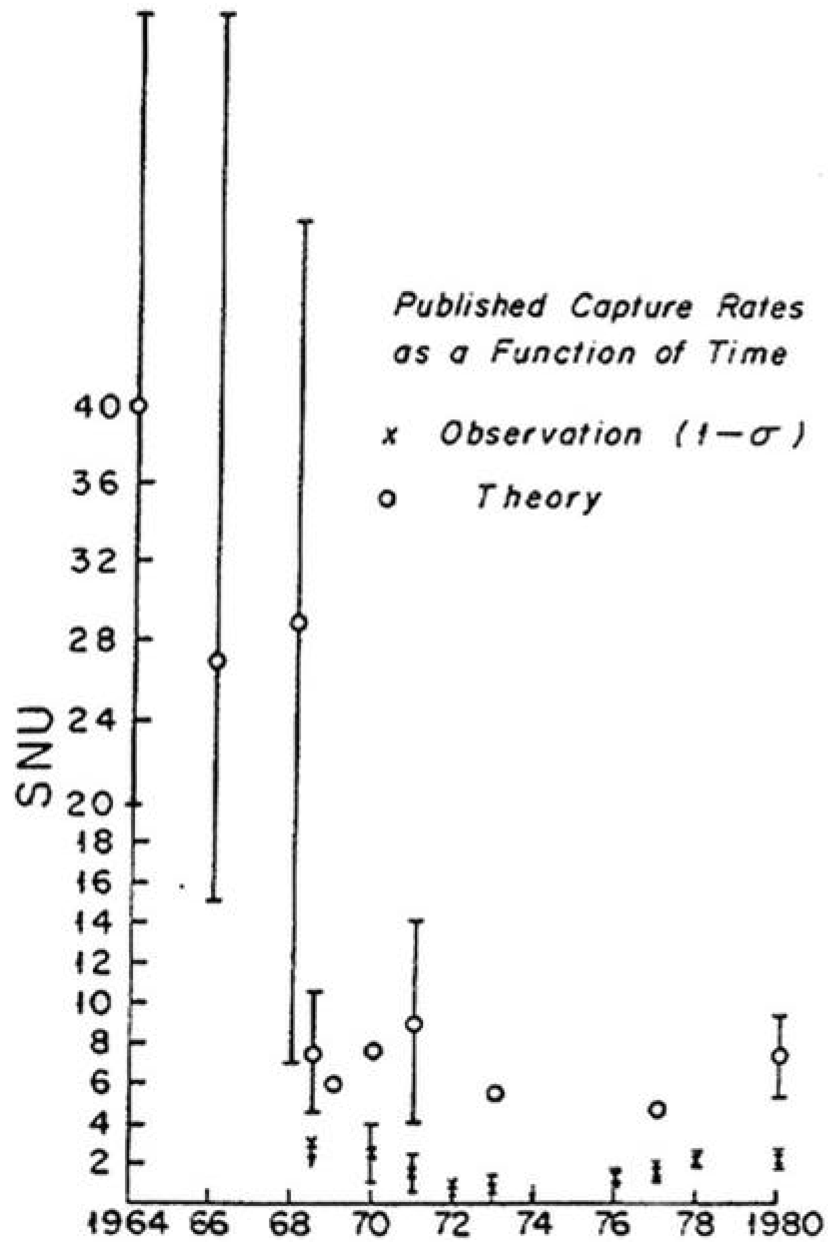

Figure 2. Published values of the predicted and observed neutrino capture rates from 1964 to 1980. The observational results are from Davis, Harmer, and Hoffman (1968); Davis (1970); Davis, Rogers, and Radeka (1971); Davis (1971); Davis, Evans, Radeka, and Rogers (1972); Davis and Evans (1973); Davis and Evans (1974); Davis and Evans (1976); Rowley, Cleveland, Davis, and Evans (1977); Davis (1978); and Rowley, Cleveland, Davis, Hampel, and Kirsten (1980). The theoretical values are from Bahcall (1964a); Bahcall (1966); Bahcall and Shaviv (1968); Bahcall, Bahcall, Fowler, and Shaviv (1968); Bahcall, Bahcall, and Shaviv (1968); Bahcall (1969b); Bahcall and Ulrich (1970); Bahcall and Ulrich (1971); Bahcall, Huebner, Magee, Merts, and Ulrich (1973); Bahcall (1977); and Bahcall, Huebner, Lubow, Magee, Merts, Parker, Rozsnyai, Ulrich, and Argo (1980). Similar results by other authors are mentioned in the text.

The procedures for analyzing the data have evolved with time; the techniques are discussed fully in the report by Davis (1978). All of the published capture rates prior to 1977 were described in the original papers (see caption to Fig. 2) as one-standard-deviation upper limits. The sensitivity of the experiment has greatly improved with time as experience has been gained with the operating system and the extremely low count rates. The measurement of both the rise time (as first suggested by Gordon Garmire) and the pulse height of the proportional-counter events allows one to discriminate strongly against noise pulses. Measuring the rise time was introduced in run 18 (1970);

it greatly reduced the number of background events. Bruce Cleveland has developed a maximum likelihood method of analyzing the data that utilizes the time of occurrence of all the events detected in the counters; this procedure is unbiased and gives a best estimate and uncertainty for both the background and the ^{37}Ar production rate. Using this method, it has been possible to establish that the ^{37}Ar production rate in the tank, although small, is actually not zero. Another way of demonstrating this fact is to use all of the events from the different runs and show that they collectively decay with the lifetime of ^{37}Ar; the resulting cumulative decay curve is a dramatic and convincing way of seeing that the experiment is actually detecting ^{37}Ar (see Davis 1978). The present best estimate for the production rate is 2.2 ± 0.4 SNU (Rowley et al. 1980).

It appears from Fig. 2 that the published estimates for the capture rate were at a minimum during 1972-4. This effect is due almost entirely to the change in the method of analyzing the data (see Davis 1978); all of the later points include the earlier data as well. In order to check this interpretation, Bruce Cleveland has reanalyzed the data using his maximum likelihood method. For the data available in 1972, Cleveland finds 1.3 ± 1 SNU (compared to the earlier published value of 0.2 SNU) and for the 1974 data Cleveland finds 2.0 ± 0.4 SNU (compared to the earlier published value of 1.3 SNU). The main difference between the present analyses and the earlier calculations is due to the fact that the statistical uncertainty for a very small number of events is now properly taken into account.

All of the high quality data presently available are shown in Fig. 3, which has been assembled by Cleveland and Davis. The average production rate of ^{37}Ar in the tank from all these data is 2.2 ± 0.4 SNU.

The current difference between theory and observation using the best available estimates for the parameters is about a factor of three. Experiments to remeasure at low energies and with the most modern techniques [see Rolfs and Trautvetter (1978) and Barnes (1981)] the cross section factors for the ^{3}He-^{3}He, ^{3}He-^{4}He, and ^{7}Be-p reactions are urgently needed. Experiments are underway to remeasure the second of these reactions, which is being studied by Claus Rolfs and his associates in Germany and also by an enthusiastic crew at the Kellogg Laboratories. Of the total 7.8 SNU predicted by the current best estimate model, 6.3 SNU is from the ^{7}Be(p,γ)^{8}B reaction, last studied in detail in 1969 by Ralph Kavanagh and his associates in an unpublished investigation (see Kavanagh 1972). It is worth stressing again that the entire difference between the theoretical and observational values in Fig. 2 is due to neutrinos from ^{8}B produced in the ^{7}Be(p,γ) reaction. The total capture rate also depends sensitively upon the ^{3}He(α,γ)^{7}Be reaction, approximately as the (cross section factor)$^{0.8}$.

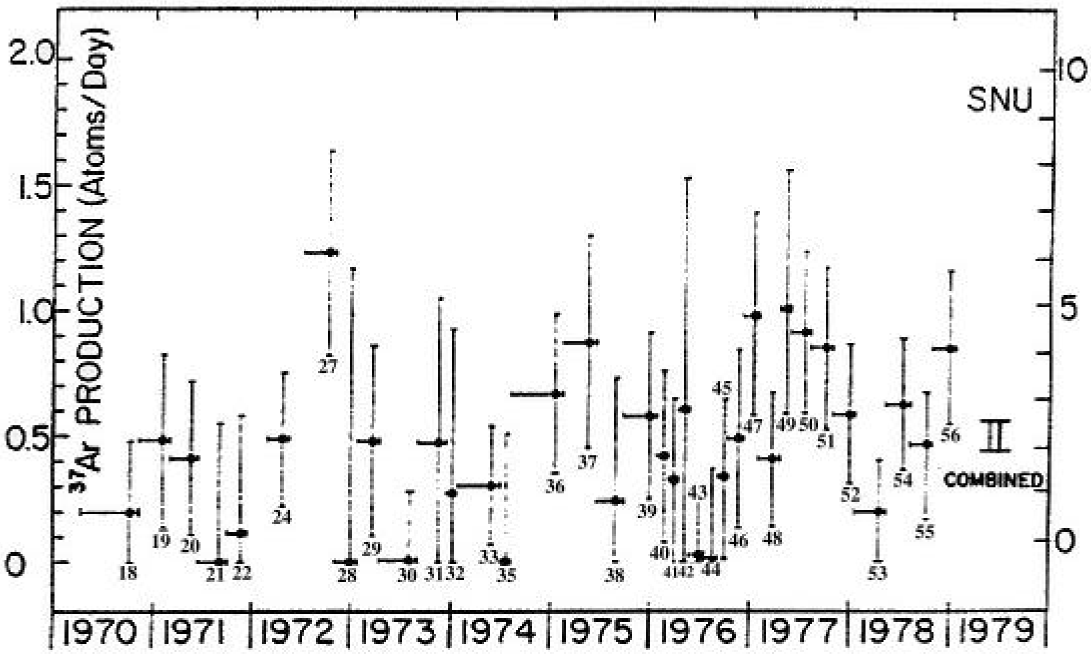

Figure 3. Summary of ^{37}Ar production rates for individual experimental runs, 1970-9.

It would not be surprising if Willy once again used his exceptional powers of persuasion to see that the above experiments were repeated expeditiously. After all, he has been telling all of us for many years what we ought to be doing; we have profited scientifically by his advice and have had fun in the process.

In conclusion, we believe that, whatever the solution of the solar neutrino problem turns out to be, the combined efforts of many people (chemists, nuclear physicists, astrophysicists, geophysicists, and elementary particle physicists) over the past two decades will ultimately result in a greater understanding of both the solar interior and the limitations of our present knowledge. In the interim, many parameters have been determined more accurately and many theoretical possibilities have been rendered implausible. Future solar neutrino experiments must delineate more clearly what is the missing element in our present understanding and even whether it is primarily in the realm of physics or astrophysics.

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References

Abraham, Z., and Iben, I., Jr. 1971, *Astrophys. J.*, **170**, 157.
Alvarez, L W. 1949, University of California Radiation Laboratory Re UCRL-328.
- 1973, Physics Notes, Mem. No. 767, Lawrence Radiation Laboratory (March 23).
Bahcall, J. N. 1962a, *Phys. Rev.*, **126**, 1143.
- 1962b, *Phys. Rev.*, **128**, 1297.
- 1964a, *Phys. Rev. Lett.*, **12**, 300.
- 1964b, *Phys. Rev. B*, **136**, 1164.

- 1964c, *Phys. Rev. B*, **135**, 137.
- 1964d, *Phys. Lett.*, **13**, 332.
- 1966, *Phys. Rev. Lett.*, **17**, 398.
- 1967, *Proceedings of the Second International Conference on High-E Physics and Nuclear Structure*, ed. G. Alexander, 232,
- l969a, *Quasars and High-Energy Astronomy*, eds. K. N. Douglas, I. Robinson, A. Schild, E. L. Schucking, J. A. Wheeler, and N. J. (New York: Gordon and Breach), p. 321.
- 1969b, *Proceedings of the International Conference on Neutrino Phy Astrophysics* (Moscow), **2**, 133 (USSR: F. I. Academy Science).
- 1969c, *Phys. Rev. Lett.*, **23**, 251.
- 1977, *Astrophys. J. Lett.*, **216**, L115.
- 1978a, *Proceedings of the Informal Conference on Status and Future Neutrino Research*, Brookhaven National Laboratory 50879, **1**, 223.
- 1978b, *Rev. Mod. Phys.*, **50**, 881.

Bahcall J. N., Bahcall, N. A., Fowler, W. A., and Shaviv, G. 1968, *Phys. Lett.*, **26B**, 359.

Bahcall, J. N., Bahcall, N. A., and Shaviv, G. 1968, *Phys. Rev. Lett* **20**, 1209.

Bahcall, J. N., Bahcall, N. A., and Ulrich, R. K. 1968, *Astrophys. L* **2**, 91.

- I969, *Astrophys. J.*, **156**, 559.

Bahcall, J. N., and Barnes, C. A. 1964, *Phys. Lett.*, **12**, 48.

Bahcall, J. N., Cabibbo, N., and Yahil, A. 1972, *Phys. Rev. Lett.*, **28**, 316.

Bahcall, J. N., Cleveland, B. T., Davis, R., Jr., Dostrovsky, I., Evans, J. C., Jr., Frati, W., Friedlander, G., Lande, K., Rowley, Stoenner, R. W., and Weneser, J. 1978, *Phys. Rev. Lett.*, **40**, 1351.

Bahcall, J. N., and Davis, R. Jr. 1966, *Stellar Evolution*, ed. R. F. Stein and A. G. W. Cameron (New York: Plenum Press), p. 241.

- 1971, *Phys. Rev. Lett.*, **26**, 662.
- 1976, *Science*, **191**, 264.

Bahcall, J. N., Fowler, W. A., Iben, I., and Sears, R. L. 1963, *Astrophys. J.*, **137**, 344.

Bahcall, J. N., and Frautschi, S. C. 1969, *Phys. Lett.*, **29B**, 623.

Bahcall, J. N., Huebner, W. F., Lubow, S. H., Magee, N. H., Jr., Mer Parker, P. D., Rozsnyai, B., Ulrich, R. K., and Argo, M. F. 1980, *Lett.*, **45**, 945.

Bahcall, J. N., Huebner, W. F., McGee, N. H., Merts, A. L., and Ulri 1973, *Astrophys. J.*, **184**, 1.

Bahcall, J. N., and May, R. M. 1968, *Astrophys. J. Lett.*, **152**, L17.

Bahcall, J. N., and Sears, R. L. 1972, *Ann. Rev. Astron. Astrophys.*, **10**, 25.

Bahcall, J. N., and Shaviv, G. 1968, *Astrophys. J.*, **153**, 113.

Bahcall, J. N., and Ulrich, R. K. 1970, *Astrophys. J. Lett.*, **160**, L57.

- 1971, *Astrophys. J.*, **170**, 593.

Barger, V., Whisnat, K, and Phillips, R. J. N. 1980, *Phys. Rev. Lett*

45, 2084.
Barnes, C. A. 1981, ``Laboratory Approaches to Nuclear Astrophysics, three lectures in *Progress in Particle and Nuclear Physics*, ed. Sir Denys Wilkinson (Oxford: Pergamon Press), to be published.
Bartenwerfer, D. 1973, *Astron. Astrophys.*, **25**, 455.
Benett, C. 1981, to be published.
Bethe, H. A. 1939, *Phys. Rev.*, **55**, 434.
Bethe, H. A., and Critchfield, L. 1938, *Phys. Rev.*, **54**, 248.
Bilenky, S. M., and Pontecorvo, B. 1978, *Phys. Reports*, **41**, 225.
Burbidge, E. M., Burbidge, G. R., Fowler, W. A., and Hoyle, F. 1957, *Phys.*, **29**, 547.
Cameron, A. G. W. 1958a, *Bull. Am. Phys. Soc. II*, **3**, 227.
- 1958b, *Ann. Rev. Nucl. Sci.*, **8**, 299.
- 1958c, *Chalk River Report* CRL-41, 2nd ed., unpublished.
- 1973, *Explosive Nucleosynthesis* ed. D. N. Schramm and W. D. Arnett (Austin: University of Texas Press).
- 1973, *Rev. Geophys. Space Phys.*, **11**, 505.
Cassidy, G. A. 1973, *Proceedings of the Thirteenth International Con Cosmic Rays*, Denver, vol. 13, p. 1958.
Christensen, C. J., Nielsen, A., Bahnsen, A., Brown, W. K., and Rust 1967, *Phys. Lett.*, **26B**, 11.
Cisneros, A. 1971, *Space Sci.*, **10**, 87.
Clayton, D. D., Dwek, E., Newman, M. J., and Talbot, R. J. 1975, *Astrophys. J.*, **201**, 489.
Crane, H. R. 1948, *Rev. Mod. Phys.*, **20**, 278.
Davis, R., Jr. 1955, *Phys. Rev.*, **97**, 766.
- 1964, *Phys. Rev. Lett.,* **12**, 303.
- 1969, *Proceedings of the International Conference on Neutrino Phys Astrophysics* (Moscow), **2**, 99 (USSR: F. I. Academy Science).
- 1970, *Hungaricae*, Suppl. 4, **29**, 371.
- 1971, *Acad. Naz. Lincei*, **157**, 59.
- 1978, *Proceedings of the Informal Conference on the Status and Fut Solar Neutrino Research*, ed. G. Friedlander, Brookhaven National L 50879, **1**, 1.
Davis, R., Jr., and Evans, J. C. 1973, *Proceedings of the Thirteenth International Cosmic Ray Conference*, Denver, vol. 3, 2001.
- 1974, *Proceedings of International Conference on Particle Accelera Nuclear Reactions in Space*, Leningrad, August, 91.
- 1976, *Proceedings International Seminar on Active Processes on the* Laboratory 1984, p. 84.
Davis, R., Jr., Evans, J. C., Radeka, V., and Rogers, L. C. 1972, in *'72*, **1**, 5, ed. A. Frenkel and G. Marx (OMDK - Technoinform, Budapest, June, 1972, Europhysics Conference).
Davis, R., Jr., Harmer, D. S., and Hoffman, K. C. 1968, *Phys. Rev. L* **20**, 1205.
Davis, R., Jr., Harmer, D. S., and Neely, F. 1969, in *Quasars and Hi Astronomy*, ed. K. N. Douglas, I. Robinson, S. Schild, E. L Schucki Wheeler, and N. J. Woolf (New York: Gordon and Breach), p. 287.
Davis, R., Jr., Rogers, C., and Rodeka, V. 1971, *Bull. Am. Phys. Soc*

16, 631.
Demarque, P., Mengel, J. G., and Sweigart, A. V. 1973, *Mon. Not. R.* *Soc.*, **165**, 19.
Dilke, F. W. W., and Gough, D. O. 1972, *Nature*, **240**, 262.
Dwarakanath, M. R. 1974, *Phys. Rec. C*, **9**, 805.
Evans, J. C., Davis, R., Jr., and Bahcall, J. N. 1974, *Nature*, **251**, 486.
Ezer, D., and Cameron, A. G. W. 1965, *Can. J. Phys.*, **43**, 1497.
- 1966, *Can. J. Phys.*, **44**, 593.
- 1968, *Astrophys. Lett.*, **1**, 177.
Fetisov, V. N., and Kopysov, Y. S. 1972, *Phys. Lett. B*, **40**, 602.
Fowler, W. A. 1954, *Mém. Soc. Roy. Sci. Liège*, **14**, 88.
- 1958, *Astrophys. J.*, **127**, 551.
- 1968, *Contemporary Physics: Trieste Symposium 1968*, vol. 1 (Vienna: International Atomic Energy Agency), p. 359.
- 1972, *Nature*, **238**, 24.
Freedman, M. S., Stevens, C. M., Horwitz, E. P., Fuchs, L. H., Lemer Goodman, L. S., Childs, W. J., and Hessler, J. 1976, *Science*, **193**, 1117.
Freedman, M. S. 1978, *Proceedings of the Informal Conference on the Future of Solar Neutrino Research*, Brookhaven National Laboratory p. 313.
Friedlander, G. 1978, ed. *Proceedings of the Informal Conference on and Future of Solar Neutrino Research*, Brookhaven National Laborat 50879, vols. 1 and 2.
Goldhaber, M. 1967, *Proceedings of the Second International Conferen High-Energy Physics and Nuclear Structure*, ed. G. Alexander, Amste p. 475.
Gribov, V., and Pontecorvo, B. 1969, *Phys. Lett.*, **28B**, 493.
Hardy, J. C., and Verrall, R I. 1964, *Phys. Rev. Lett.*, **13**, 764.
Holmgren, H. P., and Johnston, R. 1958, *Bull. Am. Phys. Soc. II*, **3**, 26.
Hoyle, F. 1975, *Astrophys. J. Lett.*, **197**, L127.
Iben, I., Jr. 1968, *Phys. Rev. Lett.*, **21**, 1208.
Iben, I., Jr., Kalata, K, and Schwartz, J. 1967, *Astrophys. J.*, **150**, 1001.
Jacobs, K. 1973, *Nature*, **256**, 560.
Kavanagh, R. W. 1960, *Nucl. Phys.*, **15**, 411.
- 1972, *Cosmology, Fusion, and Other Matters*, ed. F. Reines (Boulder: Colorado Associated University Press), p. 169.
Kocharov, G. E., and Starbunov, Yu. N. 1970, *Acta Phys. Acad. Sci. H* Suppl. to Vol. 29 (Proceedings of the Eleventh International Confe Cosmic Rays, Budapest), **4**, 353.
Kuchowicz, B. 1976, *Rep. Prog. Phys.*, **39**, 291.
Kuzmin. V. A. 1966, *Soc. Phys. JETP*, **22**, 1051.
Kuzmin, V. A., and Zatsepin, G. T. 1966, *Proceedings of the internat Conference on Cosmic Rays*, London, Vol. 2, 1023.

- 1969, *Proceedings of the International Conference on Neutrino Phys and Astrophysics* (Moscow), vol. 2, (USSR: F. I. Academy Science),
Lambert, D. L, and Warner, B. 1968a, *Mon. Not. R. Astron. Sci.*, **138**, 181.
- 1968b, *Mon. Not. R. Astron. Sci.*, **138**, 213.
Lande, K., Bozoki, G., Frati, W., Lee, C. K., Fenyes, E., and Saaved 1974, *Nature*, **251**, 485.
Libby, L. M., and Thomas, F. J. 1969, , *Nature*, **222**, 1238.
Maeder, A. 1977, *Astr. Astrophys.*, **56**, 359.
Parker, E. N. 1974, *Astrophys. Space Sci.*, **31**, 261.
Parker, P. D. 1966, *Phys. Rev.*, **150**, 851.
- 1968, *Astrophys. J. Lett.*, **153**, L85.
Parker, P. D., Bahcall, J. N., and Fowler, W. A. 1964, *Astrophys. J.* **139**, 602.
Parker, P. D., and Kavanagh, R. W. 1963, *Phys. Rev.*, **131**, 2578.
Pochoda, P., and Reeves, H. 1964, *Planet. Space Sci.*, **12**, 119.
Pontecorvo, B. 1946, Chalk River Laboratory Report PD-205.
- 1968, *Sov. Phys. JETP*, **26**, 984.
Prentice, A. J. R. 1973, *Mon. Not. R. Astron. Sci.*, **163**, 331.
Raghavan, R. S. 1976, *Phys. Rev. Lett.*, **37**, 259.
Reeder, P. L, Poskanzer, A. M., and Esterlund, R. A. 1964, *Phys. Rev. Lett.*, **13**, 767.
Reines, F. 1960, *Ann. Rec. Nucl. Sci.*, **10**, 25.
- 1967, *Proc. R. Soc. Ser. A*, **301**, 159.
- 1969, *Quasars and High-Energy Astronomy*, ed. K. N. Douglas, I. Robinson, A. Schild, E. L. Schucking, J. A. Wheeler, and N. J. (New York: Gordon and Breach), p. 295.
Reines, F., and Kropp, W. R. 1964, *Phys. Rev. Lett.*, **12**, 457.
Reines, F., Sobel, H., and Pasierb, E. 1980, *Phys. Rev. Lett.*, **45**, 1307.
Reines, F., and Trimble, V. 1972, *Proceedings of the Solar Neutrino Conference*, University of California, Irvine.
Reines, F., and Woods, R. M., Jr. 1965, *Phys. Rev. Lett.*, **14**, 201.
Rolfs, C., and Trautvetter, H. P. 1978, *Ann. Rev. Nucl. Part. Sci.*, **28**, 115.
Rood, R T. 1978, *Proceedings of the Brookhaven Solar Neutrino Confer* **1**, 175.
Rood, R. T., and Ulrich, R. K. 1974, *Nature*, **252**, 366.
Rowley, J. K, Cleveland, B. T., Davis, R., Jr., and Evans, J. C. 197 *Proceedings of the international Conference on Neutrino Physics and Neutrino Astrophysics* (Neutrino '77, Elbrus, USSR), **1**, 15.
Rowley, J. K, Cleveland, B. T., Davis, R., Jr., Hampel, W., and Kirs 1980, Brookhaven National Laboratory Report 27190, to be published
Roxburgh, I. W. 1974, *Nature*, **248**, 209.
Ryajskaya, O., and Zatsepin, G. 1965, *Proceedings of the Internation Conference on Cosmic Rays*, London, 987.
Salpeter, E. E. 1968, *Comments Nucl. Part. Phys.* **II** 97.
- 1970, *Nature*, **225**, 165.

Schatzman, E. 1969, *Astrophys. Lett.*, **3**, 139.
Sears, R. L. 1964, *Astrophys. J.*, **140**, 477.
- 1966, *Stellar Evolution*, ed. R. F. Stein and A. G. W. Cameron (New York: Plenum Press), p. 245.
Shaviv, G., Bahcall, J. N., and Fowler, W. A. 1967, *Astrophys. J.*, **150**, 725.
Shaviv, G., and Salpeter, E. E. 1968, *Phys. Rev. Lett.*, **21**, 1602.
Sheldon, W. R. 1969, *Nature*, **221**, 650.
Snell, R. L, Wheeler, J. C., and Wilson, J. R. 1976, *Astrophys. Lett* **17**, 157.
Tombrello, T. A. 1965, *Nucl. Phys.*, **71**, 459.
- 1967, *Nuclear Research with Low Energy Accelerators*, ed. J. B. Marion and D. M. Palter (New York: Academic Press), p. 19
Tombrello, T. A., and Parker, P. D. 1963, *Phys. Rev.*, **131**, 2582.
Torres-Peimbert, S., Ulrich, R. K, and Simpson, E. 1969, *Astrophys.* **155**, 957.
Trimble, V., and Reines, F. 1973, *Rev. Mod. Phys.*, **45**, 1.
Watson, W. D. 1969a, *Astrophys. J.*, **157**, 375.
- 1969b, *Astrophys. J.*, **158**, 303.
- 1969c, *Astrophys. J.*, **158**, 303.
Werntz, C., and Brennan, J. G. 1967, *Phys. Rev.*, **157**, 759.
Weymann, R., and Sears, R. L. 1965, *Astrophys. J.*, **142**, 174.
Wheeler, J. C., and Cameron, A. G. W. 1975, *Astrophys. J.*, **196**, 601.
Wolfendale, A. F., Young, E. C. M., and Davis, R., Jr. 1972, *Nature Sci.*, **238**, 1301.
Wolfenstein, L. 1978, *Phys. Rev. D*, **17**, 2365.