ELSEVIER

Nuclear Physics B (Proc. Suppl.) 118 (2003) 77–86

NUCLEAR PHYSICS B
PROCEEDINGS
SUPPLEMENTS

www.elsevier.com/locate/npe

Solar Models: An Historical Overview

John N. Bahcall[a]*

[a]School of Natural Sciences, Institute for Advanced Study, Princeton, NJ, USA

I will summarize in four slides the 40 years of development of the standard solar model that is used to predict solar neutrino fluxes and then describe the current uncertainties in the predictions. I will dispel the misconception that the p-p neutrino flux is determined by the solar luminosity and present a related formula that gives, in terms of the p-p and ^{7}Be neutrino fluxes, the ratio of the rates of the two primary ways of terminating the p-p fusion chain. I will also attempt to explain why it took so long, about three and a half decades, to reach a consensus view that new physics is being learned from solar neutrino experiments. Finally, I close with a personal confession.

1. Introduction

I will follow in this text the content of my talk at Neutrino2002, which occurred in Munich, May 25-30, 2002.

I begin in Section 2, as I did in Munich, with a tribute to Ray Davis. In Section 3, I present a concise history of the development of the standard solar model that is used today to predict solar neutrino fluxes. This section is based upon four slides that I used to summarize the development and is broken up into four subsections, each one of which describes what was written on one of the four slides. I describe in Section 4 the currently-estimated uncertainties in the solar neutrino predictions[2], a critical issue for existing and future solar neutrino experiments. I show in Section 5 that the solar luminosity does not determine the p-p flux, although there are many claims in the literature that it does. I also present a formula that gives the ratio of the rates of the ^{3}He-^{3}He and the ^{3}He-^{4}He reactions as a function of the p-p and ^{7}Be neutrino fluxes. These reactions are the principal terminating fusion reactions of the p-p chain. In Section 6,I give my explanation of why it took so long for physicists to reach a consensus that new particle physics was being learned from solar neutrino experiments. I close with a personal confession in Section 7.

2. Ray Davis

Before I begin the discussion of the standard solar model, I would like to say a few words about Ray Davis, shown in Figure 1. The solar neutrino saga has been a community effort in which thousands of chemists, physicists, astronomers, and engineers have contributed in crucial ways to refining the nuclear physics, the astrophysics, and the detectors so that the subject could become a precision test of stellar evolution and, ultimately, of weak interaction theory.

However, Ray's role in the subject has been unique. Any historical summary, even of solar models, would be grossly incomplete if it did not emphasize the inspiration provided by Ray's experimental vision. Although Ray never was involved in solar model calculations, and has always maintained a healthy skepticism regarding their validity, his interest in performing a solar neutrino experiment was certainly the motivation for my entering and remaining in the subject. More importantly, for all of the formative years of the "solar neutrino problem", Ray inspired everyone who became involved with solar neutrinos by his conviction that valid and fundamental measurements could be made using solar neutrinos. We committed to a subject that did not attract main stream scientists because we believed in Ray's dream of measuring the solar neutrino flux.

*jnb@ias.edu

[2]Where contemporary numbers are required in this review, I use the results from the BP00 solar model, ApJ 555 (2001) 990, astro-ph/0010346.

doi:10.1016/S0920-5632(03)01306-9

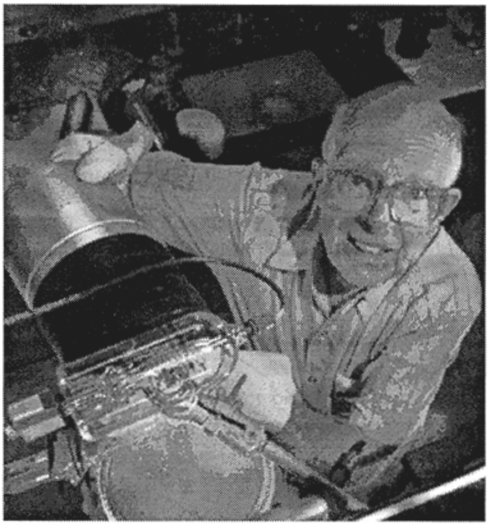

Figure 1. Ray Davis preparing to pour liquid nitrogen into a dewar on a vacuum system of the type used for gas purification and counter filling in the chlorine experiment. The glass object in the foreground with the wire coming out that blocks Ray's left hand is an ionization gauge used to measure the pressure in the vacuum system.

Ray and I have written three articles on the history of solar neutrino research (in 1976, 1982, and 2000, see http://www.sns.ias.edu/ jnb under the menu item Solar Neutrinos/History). It is not feasible to present in a twenty minute talk a balanced account of these three articles with the appropriate acknowledgments of the important work of so many people. Therefore, I shall just describe some of the highlights regarding the standard solar model from a very personal view. I encourage the listeners who are interested in a more balanced presentation to look back at the earlier articles which provide references to critical work done by a large number of researchers.

3. The development of the "standard solar model" for neutrino predictions

I describe the development of the "standard solar model" for neutrino predictions in four subsections, covering the period 1962-1988 (Section 3.1), 1988-1995 (Section 3.2), 1995-1997 (Section 3.3), and 1998-2002 (Section 3.4).

3.1. 1962-1988

At the time Ray and I first began discussing the possibility of a solar neutrino experiment, in 1962, there were no solar model calculations of solar neutrino fluxes. Ray, who heard about some of my work on weak interactions from Willy Fowler, wrote and asked if I could calculate the rate of the ^{7}Be electron capture reaction in the Sun.

After I did the calculation and submitted the paper to Physical Review, I woke up to the obvious fact that we needed a detailed model of the Sun (the temperature, density, and composition profiles) in order to convert the result to a flux that Ray might consider measuring. I moved to Willy's laboratory at CalTech, where there were experts in stellar modeling who were working on stellar evolution. We used the codes of Dick Sears and Icko Iben, and a bit of nuclear fusion input that I provided, to calculate the first solar model prediction of solar neutrinos in 1962 – 1963.

The result was extremely disappointing to Ray and to me, since the event rate from neutrino capture by chlorine that I calculated from our first flux evaluation was too small by an order of magnitude to be measured in any chlorine detector that Ray thought would be feasible. The situation was reversed in late 1963, when I realized that the capture rate for ^{8}B neutrinos on chlorine would be increased by almost a factor of 20 over my earlier calculations because of transitions to the excited states of argon, most importantly the super-allowed transition from the ground state of ^{37}Cl to the isotopic analogue state at about 5 MeV excitation energy in ^{37}Ar. This increase in the predicted rate made the experiment appear feasible and Ray and I wrote a joint paper for Physical Review Letters proposing a practical chlorine experiment, a paper that was separated into two shorter papers to meet the space requirements.

During the period 1962 – 1968, the input data to the solar models were refined in a number of important ways as the result of the hard work of many people. The most significant changes were in the measured laboratory rate for the ^{3}He-^{3}He reaction (changed by a factor of 3.9), in the theoretically calculated rate for the $p-p$ reaction (changed by 7%), and the observed value of the heavy element to hydrogen ratio, Z/X (decreased by a factor of 2.5). Unfortunately, each of the individual corrections were in a direction that decreased the predicted flux.

Ray's first measurement was reported in PRL in 1968. Our accompanying best-estimate solar model prediction (made together with N. A. Bahcall and G. Shaviv) was about a factor of 2.5 times larger than Ray's upper limit. But the uncertainties in the model predictions were, in 1968, sufficiently large that I personally did not feel confident in concluding that the disagreement between prediction and measurement meant that something fundamental was really wrong.

As it turned out, the values of the stellar interior parameters used in 1968 are in reasonably good agreement with the values used today. However, the uncertainties are much better known now, after more than three decades of intense and precise studies and refinements by many different groups working all over the world.

The laboratory measurement of the ^{7}Be$(p,\gamma)^8$B cross section was a principal source of uncertainty in the 1962 prediction, remained a principal uncertainty in 1968, and is still today one of the two largest uncertainties in the solar neutrino predictions. Moreover, the best-estimate measured value for the cross section has decreased significantly since 1968 (see Figure 4).

As we shall see in the subsequent discussion, the only fundamentally new element that has been introduced in the theoretical calculations since 1968 is the effect of element diffusion in the sun (see 1988-1997 below).

During the period 1968 – 1988, very few people worked on topics related to solar neutrinos. There was only one solar neutrino detector, Ray's chlorine experiment. His measurement was lower than our prediction. I concentrated during these two long decades on refining the predictions and, most importantly, making the estimates of the uncertainties more formal and more robust.

We calculated the uncertainties by computing the partial derivatives of each of the fluxes with respect to each of the significant input parameters. In 1988, Roger Ulrich and I also did a Monte Carlo study of the uncertainties, which made use of the fluxes calculated from 1000 standard solar models. For each of the 1000 models, the value of each input parameter was drawn from a probability distribution that had the same mean and variance as was assigned to that parameter. The Monte Carlo results confirmed the conclusions reached using the partial derivatives. The uncertainty estimates made during this period are the basis for the uncertainties assigned in the current neutrino flux predictions and influence inferences regarding neutrino parameters (like Δm^2, $\tan^2\theta$) that are derived from analyses that make use of the solar model predictions.

3.2. 1988-1995

In the period 1990–1994, F. Rogers and J. Iglesias of the Livermore National Laboratory published their detailed and improved calculations of stellar radiative opacities and equation of state. Now almost universally used by stellar modelers, this fundamental work resolved a number of long standing discrepancies between observations and predictions of stellar models.

In the same 1988 RMP paper in which we presented the Monte Carlo study of the uncertainties, Roger Ulrich and I also made comparisons between the predictions of our standard solar model–constructed to predict solar neutrinos–and the then existing helioseismological data on p-mode oscillations. The agreement was reasonably impressive: the model predictions and the measured frequency splittings agreed to about 0.5%. But, we suspected that there was something missing in the solar models.

During the period 1990 – 1995, my colleagues and I made successively better approximations at including element diffusion in the solar model calculations. First, we derived an approximate analytic description which was included in the solar models (after some significant coding struggles) and later we made use of a precise computer subroutine that calculated the diffusion numerically. This work was done with S. Basu, A. Loeb, M. Pinsonneault, and A. Thoule.

3.3. 1995-1997

In 1995, Steve Tomczyk and his colleagues presented the first observations of the solar p-mode oscillations that included modes that sampled well both the intermediate solar interior and the deep interior. These observations determined precise observational values for the sound speed over essentially the entire solar interior.

We were in a wonderful position to make use of these precise sound speeds. In 1995, Marc Pinsonneault and I had just published a systematic study of improved solar models that incorporated the new opacity and equation of state calculations from the Livermore group and, most importantly, we had succeeded in including helium and heavy element diffusion in our standard solar model.

Together with Sarbani Basu and Joergen Christensen-Dalsgaard, we showed that the helioseismologically measured sound speeds were in excellent agreement throughout the Sun with the values calculated from our previously constructed standard solar model. As shown in Figure 2, the agreement averaged better than 0.1% r.m.s. in the solar interior. We made a simple scaling argument between accuracy in predicting sound speeds and accuracy in predicting neutrino fluxes. The concluding sentence in the Abstract of our PRL paper was:

"Standard solar models predict the structure of the Sun more accurately than is required for applications involving solar neutrinos."

This result was published in the January 1997 issue of PRL, but I had earlier presented at Neutrino '96 in Helsinki (June 1996) the same conclusion based upon somewhat less precise helioseismological data. Since many of you were present also at Helsinki, you may be interested in the precise form of the statement made in the printed proceedings:

"Helioseismology, as summarized in Figure 2 [a comparison of measured and calculated sound speeds], has effectively shown that the solar neutrino problems cannot be ascribed to errors in the temperature profile of the Sun."

So, from the astronomical perspective, we have known for six years that new physics was required to resolve the discrepancy between the standard predictions of the solar model and electroweak theory. Even prior to the existence of this helioseismological evidence, it had become clear that one could not fit the data for all the solar neutrino experiments by simply rescaling standard

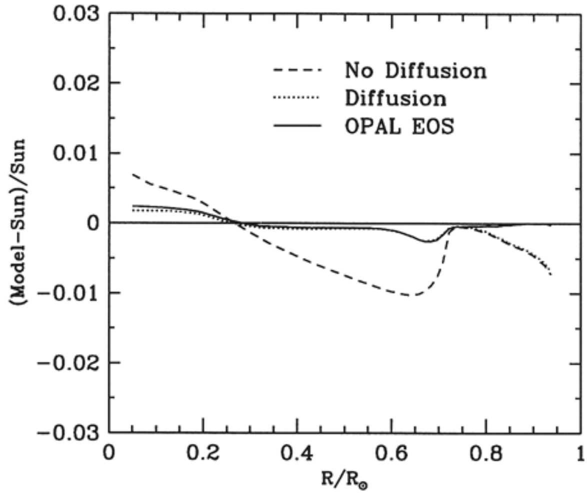

Figure 2. Comparison of measured and calculated sound speeds. This figure from astro-ph/9610250, PRL **178**,171 1997 compares the sound speeds calculated with the standard solar model, BP95, with the helioseismologically determined sound speeds. The dashed curve represents the results from a solar model that does not include element diffusion. Much better agreement is obtained when element diffusion is included, as indicated by the dotted curve. The solid line represents a model in which both element diffusion and the refined OPAL equation of state are included.

predictions of neutrino fluxes.

Why did it take so long? In Section 6, I will try to answer the question: Why were some physicists unconvinced by the astronomical evidence that solar neutrino oscillations occurred?

3.4. 1998-2002

You have heard beautiful talks at this conference by A. Hallin and M. Smy on the awesome achievements of the SNO and Super-Kamiokande collaborations. These experiments have confirmed directly the calculated solar model flux of ^{8}B neutrinos, provided there is not a large component of sterile neutrinos in the incident flux.

In units of $10^6\ \mathrm{cm^{-2}s^{-1}}$, the standard solar model prediction for the flux, ϕ, of rare ^{8}B neutrinos is

$$\phi(\mathrm{BP00}) = 5.05^{+1.0}_{-0.8}. \quad (1)$$

In June 2001, the SNO collaboration announced that the combined result from their initial CC measurement and the Super-Kamiokande $\nu - e$ scattering measurement implied a flux of ^{8}B active neutrinos equal to

$$\phi(\mathrm{SNO\ CC} + \mathrm{SK}) \ = \ 5.44 \pm 0.99. \tag{2}$$

The agreement between the best-estimate calculated value given in Eq. (1) and the best-estimated measured value given in Eq. (2) is 0.3σ.

The recent SNO NC measurement implies an even closer agreement between the best-estimates. Assuming an undistorted ^{8}B neutrino spectrum (a very good approximation), the SNO collaboration finds

$$\phi(\mathrm{NC}) \ = \ 5.09 \pm 0.64. \tag{3}$$

The agreement between the best-estimates given in Eq. (1) and Eq. (3) is embarrassingly small, 0.03σ, but obviously accidental. The quoted errors, theoretical and experimental, are real and relatively large.

We shall now discuss uncertainties in the predictions of the solar neutrino fluxes.

4. Uncertainties in the solar model predictions

I will begin the discussion of uncertainties with a brief introduction in Section 4.1 that emphasizes the importance of robust and well-defined estimates of the errors. Then I will describe in Section 4.2 the most important sources of uncertainties in the contemporary predictions.

4.1. Skepticism

From the very beginning of solar neutrino research, the uncertainties in the solar model predictions have been a central issue. If, as many physicists initially believed, the astronomical predictions were not quantitatively reliable, then there was no real "solar neutrino problem."

Even Bruno Pontecorvo, in his prophetic paper "Neutrino Experiments and the Problem of Conservation of Lepton Charge", Soviet Physics JETP, 26, 984 (1968), expressed the view that the uncertainties in the solar model calculations were so large as to prevent a useful comparison with solar neutrino experiments, Here is what Bruno said:

"From the point of view of detection possibilities, an ideal object is the sun...Unfortunately, the weight of the various thermonuclear reactions in the sun, and the central temperature of the sun, are insufficiently well known in order to allow a useful comparison of expected and observed solar neutrinos, from the point of view of this article."

This comment by Bruno Pontecorvo is indicative of the skepticism about solar model predictions that existed among many physicists. In an effort to assess the validity of this skepticism, I spent much of the period 1968-2002 investigating the robustness of the solar model predictions of neutrino fluxes. Even after the experimental confirmation of the predicted active ^{8}B neutrino flux, the uncertainties in the other solar neutrino fluxes remain an important ingredient in the determination of neutrino parameters (Δm^2, $\tan^2\theta$) from global analyses of solar neutrino experiments.

I want to summarize for you now the current best estimates for the uncertainties in the solar neutrino predictions.

4.2. Currently estimated uncertainties in predicted neutrino fluxes

I will first present in Section 4.2.1 the current values for the total and the partial uncertainties in the flux predictions. Then I will describe in Section 4.2.2 and Section 4.2.3, respectively, the very different histories for the determination of the cross sections for the ^{7}Be(p,γ)^{8}B reaction and the ^{37}Cl(ν, e^-)^{37}Ar.

4.2.1. Total and fractional uncertainties

Figure 3 shows the calculated values for the principal (p-p) solar neutrino fluxes and their estimated uncertainties. The p-p and pep neutrino fluxes are predicted with a calculated uncertainty of only $\pm1\%$ and $\pm1.5\%$, respectively. The ^{7}Be neutrino flux is predicted with an uncertainty of $\pm10\%$ and the important ^{8}B neutrino flux, which is measured by Super-Kamiokande and SNO, is predicted with an error of about 20%. The fluxes from CNO reactions, especially ^{13}N and ^{15}O neutrino fluxes, are predicted with less precision than the fluxes from the p-p reactions. I have not

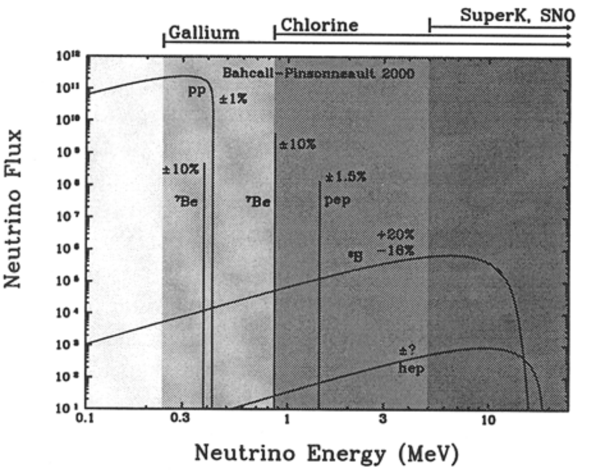

Figure 3. Solar neutrino spectrum with currently estimated uncertainties.

shown the CNO fluxes in Figure 3 since these fluxes are not expected to play a discernible role in any of the planned or in progress solar neutrino experiments.

Table 1 shows how much each of the principal sources of uncertainty contribute to the total present-day uncertainty in the calculation of the ^{8}B and ^{7}Be solar neutrino fluxes. The largest uncertainty in the prediction of the ^{8}B neutrino flux is caused by the estimated error in the laboratory measurement of the low energy cross section for the ^{7}Be(p,γ)^{8}B reaction (This statement was also true in 1962, 1964, 1968, ...). The largest uncertainty in the prediction of the ^{7}Be neutrino flux is due to the quoted error in the measurement of the low energy rate for the ^{3}He + ^{4}He reaction. In addition, there are a number of other sources of uncertainty, all of which contribute more or less comparably to the total uncertainty in the prediction of the ^{7}Be and the ^{8}B neutrino fluxes.

4.2.2. The saga of the ^{7}Be(p,γ)^{8}B cross section

Figure 4 shows, as a function of the date of publication, the measured values for the low energy cross section of the crucial reaction ^{7}Be(p,γ)^{8}B. I have only shown here the direct measurements of this reaction; there are also indirect measure-

Table 1
Fractional uncertainties in the Predicted ^{8}B and ^{7}Be Solar Neutrino Fluxes (BP00). The table presents the fractional uncertainties in the calculated ^{8}B and ^{7}Be neutrino fluxes, due to the different factors listed in the column labeled Source. The first four rows refer to the low energy cross section factors for different fusion reactions. The last four rows refer to the heavy element to hydrogen ratio, Z/X (Composition), the radiative opacity, a multiplicative constant in the expression for the diffusion rate of heavy elements and helium, and the total solar optical luminosity.

Source	^{8}B	^{7}Be
p-p	0.04	0.02
^{3}He+^{3}He	0.02	0.02
^{3}He+^{4}He	0.08	0.08
p + ^{7}Be	$^{+0.14}_{-0.07}$	0.00
Composition	0.08	0.03
Opacity	0.05	0.03
Diffusion	0.04	0.02
Luminosity	0.03	0.01

ments that yield similar results.

The encouraging aspect of Figure 4 is that the huge uncertainty that existed between 1960 and 1980, of order a factor of two, has been much reduced in the following two decades. In the BP00 calculations, we adopted as the best-estimate the Adelberger et al. [RMP, 70, 1265 (1998), astro-ph/9805121] consensus value for the cross section factor of the ^{7}Be(p,γ)^{8}B reaction, $S_{17}(0) = 19(1+^{+0.14}_{-0.07})$ eV-b (the 1σ error given here is one-third the Adelberger et al. 3σ estimate). This value is indicated in the figure by arrow next to "Standard."

Several refined experiments are in progress or are planned to measure more accurately the low energy cross section factor for the ^{7}Be(p,γ)^{8}B reaction or the p(^{7}Be,γ)^{8}B reaction. Also, there are a number of related reactions that are being studied in order to give somewhat more indirect information about the low energy cross section. The goal of all these experiments is to reduce the combined systematic and statistical errors to below 5%, so that $S_{17}(0)$ is no longer a dominant

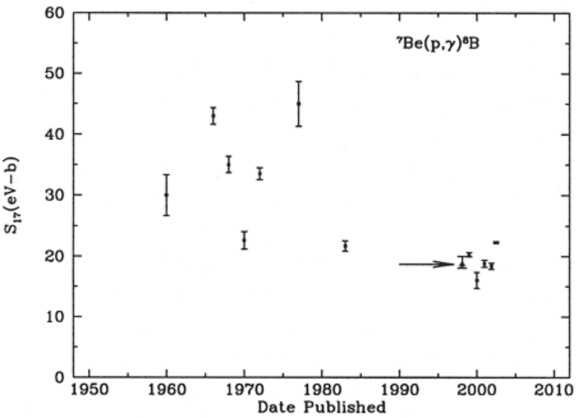

Figure 4. ^{7}Be(p,γ)^{8}B. The figure shows the measured values as a function of date of publication for the low-energy cross section factor for the ^{7}Be(p,γ)^{8}B reaction. The arrow points to the currently standard value, recommended by Adelberger et al., that is used in the BP00 calculations.

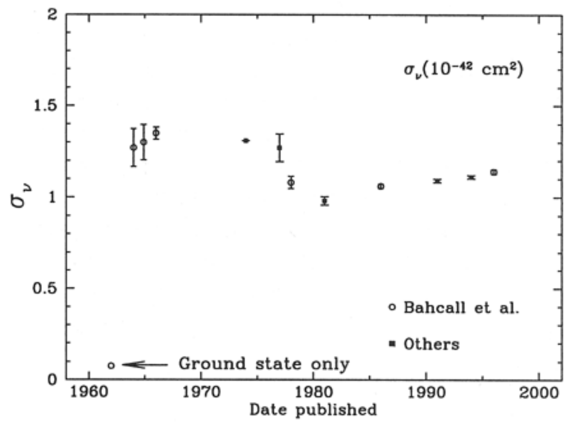

Figure 5. ^{37}Cl(ν_e,e^-)^{37}Ar. The figure shows, for an undistorted ^{8}B solar neutrino spectrum, the calculated values for the cross section ^{37}Cl(ν_e,e^-)^{37}Ar as a function of date of publication.

source of uncertainty in the prediction of the ^{8}B solar neutrino flux (cf. Table 1 above).

To the best of my knowledge, the preliminary data from all of the existing experiments are consistent with the currently standard value of $S_{17}(0)$ quoted above. In order to avoid the confusion that would be created by introducing numbers in the literature that are changed frequently, I prefer not to revise the "standard" estimate of $S_{17}(0)$ (and the ^{8}B solar neutrino flux) until the in-progress experiments on ^{7}Be(p,γ)^{8}B and related reactions are completed.

4.2.3. The ^{37}Cl(ν_e, e^-)^{37}Ar cross section

In the early days of solar neutrino astronomy, the cross sections for neutrino absorption by chlorine, ^{37}Cl(ν_e, e^-)^{37}Ar, were an important source of uncertainty. For comparison with Figure 4, I show in Figure 5 the calculated values of the absorption cross section for ^{8}B neutrinos incident on ^{37}Cl. The first calculation I made (in 1962) was too small, because I did not consider transitions to excited states. The calculation I made in 1964 was quickly confirmed by measurements made (by Poskanzer et al.) on the predicted decay: $^{37}\text{Ca} \rightarrow {}^{37}\text{K} + e^+ + \nu_e$, which is the isotopic analogue of the neutrino capture reaction. A series of subsequent refined measurements and calculations reduced the estimated error in the neutrino cross section to where it is no longer one of the largest sources of uncertainty in the calculation of the predicted capture rate in the chlorine solar neutrino experiment (although the uncertainty still plays some role in the global determination of solar neutrino oscillation parameters).

We shall now discuss the uncertainty in the calculated p-p neutrino flux.

5. Is the flux of p-p neutrinos determined by the solar luminosity?

The predicted p-p neutrino flux is NOT determined by the solar luminosity independent of details of the solar model. There are many statements in the literature that make the opposite claim, namely, that one can calculate the p-p solar neutrino flux without using a detailed solar model[3]. These claims are wrong.

Usually, I have managed to keep quiet about this question because it seemed to be only of academic importance. But, now that p-p solar neutrino experiments are being seriously developed, it is important to consider what the p-p flux really can tell us. We shall see that the p-p neutrino flux cannot be obtained simply from the observed solar photon luminosity, but instead is determined by the temperature, density, and composition profiles of the present-day solar interior.

5.1. The CNO cycle does not produce p-p neutrinos

The simplest way to see that the p-p neutrino flux is not determined by the solar luminosity is to recall the results of Hans Bethe in his epochal paper on main-sequence nuclear fusion reactions. Hans concluded, using a crude stellar model, that the Sun was powered by the now familiar nuclear reactions that make up the CNO cycle. If the Sun shines by the CNO reactions, then the p-p neutrino flux is essentially zero. Q.E.D.

Based upon the results of detailed stellar model calculations, we now believe that stars slightly heavier than the Sun shine primarily by the CNO reactions, whereas these reactions are responsible for only about 1.5% of the solar luminosity.

5.2. Why the confusion?

What is the origin of the confusion? Why have so many people erroneously claimed that the solar luminosity determines the p-p neutrino flux?

I think the most important reason is that the solar luminosity does determine the total solar neutrino flux, just not the p-p flux[4] . The basic reaction by which the Sun and other main sequence stars shine is

$$4p \rightarrow {}^4\mathrm{He} + 2e^+ + 2\nu_e. \tag{4}$$

Lepton conservation guarantees that two neutrinos are produced every time four protons are burned to form an alpha-particle. However, lepton conservation does not guarantee that two *p-p* neutrinos are produced. Nuclear fusion may produce, for example, one p-p neutrino and one ^{7}Be neutrino.

If a ^{3}He ion fuses with an ambient alpha-particle, $^3\mathrm{He}(\alpha,\gamma)^7\mathrm{Be}$, before the reaction $^3\mathrm{He}(^3\mathrm{He},2p)^4\mathrm{He}$ can occur, then one p-p neutrino and one ^{7}Be or ^{8}B neutrino will be produced as four protons are burned. If instead the $^3\mathrm{He}(^3\mathrm{He},2p)^4\mathrm{He}$ reaction occurs first, then two p-p neutrinos are produced. Even if we could ignore the possibility of CNO fusion reactions, the solar luminosity would not determine the p-p neutrino flux.

The solar luminosity does determine the *maximum* possible flux of p-p neutrinos, $\phi_{\mathrm{Max}}(\mathrm{p-p})$, via the relationship:

$$\begin{aligned}\phi_{\mathrm{Max}}(\mathrm{p-p}) &= \frac{2L_\odot}{4\pi(A.U.)^2 E_{\mathrm{p-p}}} \\ &= 6.51\times 10^{10}\,\mathrm{cm^{-2}s^{-1}},\end{aligned} \tag{5}$$

where $L_\odot$ is the solar luminosity (in photons), A.U. is the average Earth-Sun distance, and $E_{\mathrm{p-p}}$ is the energy released (26.197 MeV) to the star when the p-p chain is terminated by the ^{3}He-^{3}He reaction and only p-p neutrinos are produced. The flux of p-p neutrinos in the standard solar model happens to be $0.91\phi_{\mathrm{Max}}(\mathrm{p-p})$. But, a detailed solar model is required to determine where the p-p flux lies between 0 and 1 times $\phi_{\mathrm{Max}}(\mathrm{p-p})$.

5.3. Using p-p and ^{7}Be neutrinos to probe details of solar fusion

Is there any way of probing the solar interior and determining experimentally which terminating reaction of the p-p chain, ^{3}He-^{3}He or ^{3}He-^{4}He, is faster in the solar interior and by how much? Yes, there is a way. Solar neutrino experiments can do just that.

The ratio R of the rate of ^{3}He-^{3}He reactions to the rate of ^{3}He-^{4}He reactions averaged over the Sun can be expressed in terms of the p-p and ^{7}Be

[3]One of the most important recent experimental papers on solar neutrinos states in the Introduction: "If we exclude exotic hypotheses, the rate of the p-p reaction is directly related to the solar luminosity..."

[4]Even this statement is only correct if one neglects the difference in neutrino energy loss between different ways of burning four protons to make an alpha-particle.

neutrino fluxes by the following simple relation[5]:

$$R \equiv \frac{<^3\mathrm{He}+^4\mathrm{He}>}{<^3\mathrm{He}+^3\mathrm{He}>} = \frac{2\phi(^7\mathrm{Be})}{\phi(\mathrm{pp}) - \phi(^7\mathrm{Be})}. \quad (6)$$

The standard solar model predicts $R = 0.174$. One of the reasons why it is so important to measure accurately the total p-p and ^{7}Be neutrino flux is in order to test this detailed prediction of standard solar models. The value of R reflects the competition between the two primary ways of terminating the p-p chain and hence is a critical probe of solar fusion.

6. Why did it take so long?

In the introduction to this talk, I said that I would address the question of why it took so long, about 35 years, to convince many physicists that solar neutrino research was revealing something new about neutrinos. I will now do my best to explain why the process from discovery to consensus required more than three decades.

In the early years, after the very rapid progress between 1964 to 1968, there were many, many things that had to be looked at very carefully to see if there could be something important that had been left out of the standard solar models. The values of all of the (large number of) important input parameters were remeasured or recalculated more accurately, a variety of imaginative "non-standard solar models" were examined critically, and possible instabilities in the solar interior were investigated. It took about 20 years, 1968-1988, for the collective efforts of many nuclear physicists, atomic physicists, astronomers, and astrophysicists to provide a thoroughly explored basis for the standard model calculations that allowed robust estimates of the uncertainties in the solar model predictions. Even after this long struggle with details was mostly complete, it was still necessary to develop codes that could include the refinement of element diffusion (which took until 1995). And, presumably, there are still today even further refinements that are appropriate and necessary to make to obtain a still more accurate description of the region in which solar fusion takes place.

My impression is that nearly all particle physicists remained blissfully unaware of, or indifferent to, the decades of efforts to make the solar neutrino predictions more robust. Why? Why did many (but not all) particle physicists not take the "solar neutrino problem" seriously?

I think that there were three reasons it took so long for particle physicists to acknowledge that new physics was being revealed in solar neutrino research. First, the Sun is an unfamiliar accelerator. Particle physicists, and most other physicists too, were skeptical of what astronomers and astrophysicists could learn about an environment that they could neither visit nor manipulate. These physicists often had only a newspaper-level understanding of the observational phenomena that stellar models reproduced and the constraints they met. Second, physicists who heard my talks, or heard other talks on solar neutrinos, were most impressed by the fact that the ^{8}B solar neutrino flux depended on the 25th power of the central temperature, $\phi(^8\mathrm{B}) \propto T^{25}$. This dependence seemed too sensitive to allow an accurate prediction (an objection which was answered experimentally only by the helioseismological measurements in 1995 and their successful comparison in 1996-1997 with standard solar model predictions, see Section 3.3). Third, the simplest interpretation of the discrepancy between observed and predicted solar neutrino event rates, vacuum neutrino oscillations, suggested large mixing angles for the neutrinos. It was widely (but not universally) agreed among particle theorists that mixing angles in the lepton sector would be small in analogy with the mixing angles in the quark sector. The most popular view of particle theorists over most of the history of solar neutrino research has been that since quarks and leptons are probably in the same multiplets, they should have mixing angles of comparable size. This objection to new solar neutrino physics was removed only when Mikheyev and Smirnov built upon the earlier work of Wolfenstein to describe the magic of the MSW effect. Ironically, the small mixing angle (SMA) MSW solution persuaded a significant number of physicists that there might be new

[5] More precisely, $\phi(^7\mathrm{Be})$ should be replaced by the sum of the ^{7}Be and ^{8}B neutrino fluxes in the denominator of Eq. (6).

physics being revealed by solar neutrino experiments, although today we know that only large mixing angles solutions are good fits to all the available solar and reactor neutrino data.

I think that the spirit with which many particle physicists regarded solar neutrino research is best expressed by a quotation from the introduction of a 1990 paper written by H. Georgi and M. Luke [Nucl. Phys. B, 347, 1 (1990)]. They began their article as follows:

"Most likely, the solar neutrino problem has nothing to do with particle physics. It is a great triumph that astrophysicists are able to predict the number of ^{8}B neutrinos to within a factor of 2 or 3..."

Professor Georgi generously allowed me to quote from his paper and asked only that I emphasize that it was the dependence on the 25th power of the temperature that maintained his skepticism even after the invention of the MSW effect.

7. A personal confession

I close this talk with a confession. This is the first time in 40 years of giving talks about solar neutrinos that it seems to me that the people in the audience are more confident of the solar neutrino predictions than I am.

More than 99.99% of the predicted solar neutrino flux is below 5 MeV. We do not yet have any direct energy measurements of the flux of this dominant component of the Sun's neutrino spectrum. The formal error on the predicted p-p neutrino flux is only $\pm 1\%$, but I think realistically it would take another 3 to 10 years of theoretical and experimental research to make sure that we have got everything (including the uncertainties) as correct as we can at this level of precision. We have to search hard for uncertainties at a level that is an order of magnitude smaller than the uncertainties that are significant for interpreting existing experiments. Moreover, it seems to me that there could still be surprises in the neutrino physics. Simple neutrino scenarios fit well the existing data, which–with the exception of the chlorine and gallium radiochemical experiments–all detect only solar neutrinos with energies above 5 MeV. Perhaps these higher energy data have not yet revealed the full richness of the weak interaction phenomena.

Although we certainly have reason for the celebratory remarks that have been made at this conference, I often wonder whether Nature has some beautiful tricks that are still hidden from us.

I am grateful to many colleagues, who are also close friends, with whom I have collaborated on this subject over the past 40 years. We have had a lot of fun together. This work was partially supported by an NSF grant No. PHY0070928.