Stellar Energy Generation and Solar Neutrinos

Half a century after he explained how the Sun shines, Hans Bethe addressed the problem of why we see too few of its neutrinos.

John N. Bahcall and Edwin E. Salpeter

How energy is produced in the Sun and other stars was a big puzzle when Hans Bethe was a teenager in the early 1920s. When Arthur Eddington wrote his long *Encyclopedia Britannica* article on stars in 1911, he could report only on energy release from gravitational contraction, a clearly inadequate source. By 1920, however, Francis Aston had shown experimentally that the mass of the helium atom is slightly less than that of four hydrogen atoms.

Modern measurements tell us that the alpha particle's mass is less than four times the proton mass by 26 MeV, about 7%. Soon after Aston's discovery, Eddington suggested that the Sun's energy source might be the conversion of hydrogen to helium. If that were so, he argued, the Sun could shine for a very long time.

But no one in the 1920s knew how this putative fusion process might work. In the following decade, however, quantum mechanics became well established and nuclear physics was greatly stimulated by the discoveries of the neutron and the positron. Much of the progress on nuclei was due to Hans. His contributions included several papers with his friend Rudolf Peierls on neutron–proton scattering and on the deuteron. George Gamow's elucidation of Coulomb-barrier penetration was a huge step. But perhaps the greatest advance during that period was "the Bethe bible," a set of three full issues (one each with Robert Bacher and Stanley Livingston as coauthor, and one alone) of *Reviews of Modern Physics* in 1936 and 1937. The three review articles summarized all that was known, or even surmised, about nuclear physics at the time.

How the Sun shines

Following up on a suggestion by Carl von Weizsäcker, Hans and Charles Critchfield, a graduate student of Gamow's, teamed up to calculate the rate at which two protons would fuse to form a deuteron with the emission of a positron and a neutrino.[1] They calculated a moderately accurate energy production rate for the p–p fusion chain as a function of temperature, but they did not have a detailed model of the solar interior from which to deduce realistic temperatures. Consequently, their inferred energy-production rate disagreed badly with the Sun's known luminosity. So Hans considered that work with Critchfield at the beginning of 1938 to be just an exercise rather than the start of a new science.

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But his gloom vanished promptly after he attended a conference in Washington, organized by Gamow in March 1938. There he heard of new estimates of solar interior temperatures that brought his calculations into encouraging agreement with the Sun's luminosity. Exploiting his intimate knowledge of nuclear physics, Hans examined all reactions that might lead from hydrogen to helium. Studying all the exothermic reactions between a proton and the various isotopes of carbon and nitrogen, he found a cyclic phenomenon: Starting with ^{12}C, you don't simply end up with ^{16}O. Mostly, you come back to ^{12}C plus the desired helium nucleus.

Hans was able to calculate not only the reaction rates for this "carbon-nitrogen-oxygen cycle" as a function of temperature, but also the abundance ratios for the various isotopes that served as catalytic intermediates in the cycle. He correctly deduced that the CNO cycle and the p–p chain would supply about equal amounts of energy at a temperature of about 16 million kelvin. With reactants having high Coulomb barriers produced by nuclear charges of six and seven, the CNO thermonuclear reactions were more temperature-sensitive than the p–p chain.

It was already known that the luminosity of main-sequence stars much more massive than the Sun is a steeper function of temperature than it is for less massive stars. Working out the details of the CNO cycle took Hans only two weeks. It was immediately clear to him that this conjectured chain of fusion and breakup reactions must play an important astrophysical role. He concluded that stars significantly heavier than the Sun would shine via the CNO cycle and that lighter stars would shine via fusion initiated by the p–p reaction.

The overly simplistic models of the solar interior in use before the 1938 Washington conference gave central temperatures that were too high. Unlike those very inaccurate estimates, the central temperature in the Bethe–Critchfield paper was high by only 20%. That overestimate didn't make much difference for p–p energy production, but it did for the temperature-sensitive CNO cycle. So Hans inferred, incorrectly, that the Sun shines primarily via the CNO reactions. But his result for more massive main-sequence stars fit the observations remarkably well. As for stars that have left the main sequence, he already knew that red giants were altogether different.

Characteristically, until the end of his life, Hans remained interested in the question of the relative importance of p–p and CNO energy generation in the Sun. At age 96, he sent a short handwritten note to one of us (Bahcall), commenting on, and offering congratulations for, the exploitation of solar-neutrino observations to set a 7% upper limit on the CNO contribution to solar energy generation (see PHYSICS TODAY, July 2002, page 13).

Hans's great 1939 paper was a landmark achievement that showed how stars shine.[2] It set the agenda for nuclear astrophysics for the next half century. Hans was awarded

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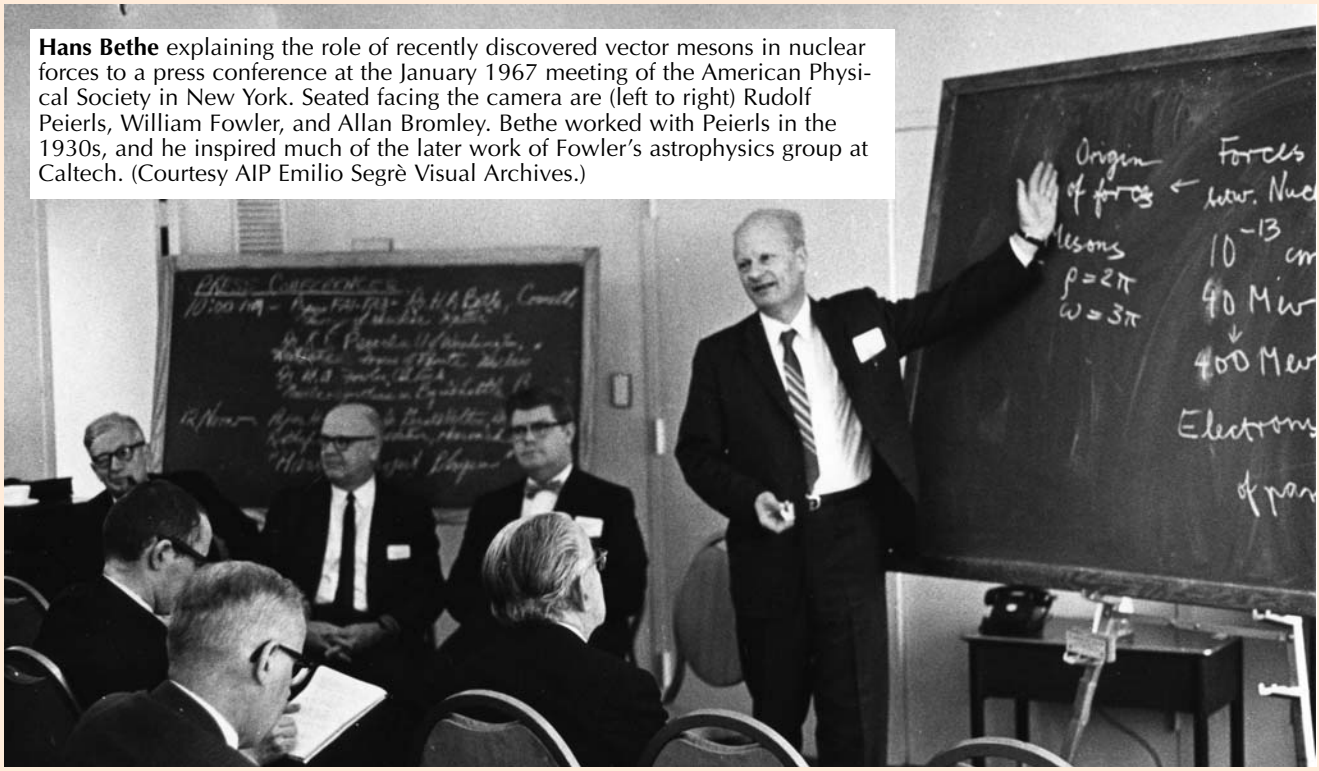


Hans Bethe explaining the role of recently discovered vector mesons in nuclear forces to a press conference at the January 1967 meeting of the American Physical Society in New York. Seated facing the camera are (left to right) Rudolf Peierls, William Fowler, and Allan Bromley. Bethe worked with Peierls in the 1930s, and he inspired much of the later work of Fowler's astrophysics group at Caltech. (Courtesy AIP Emilio Segrè Visual Archives.)

a New York Academy of Sciences prize for that work even before its publication. But his Nobel Prize did not come until 1967. The delay may have been due, in part, to the difficulty of deciding which of his many important contributions to recognize.

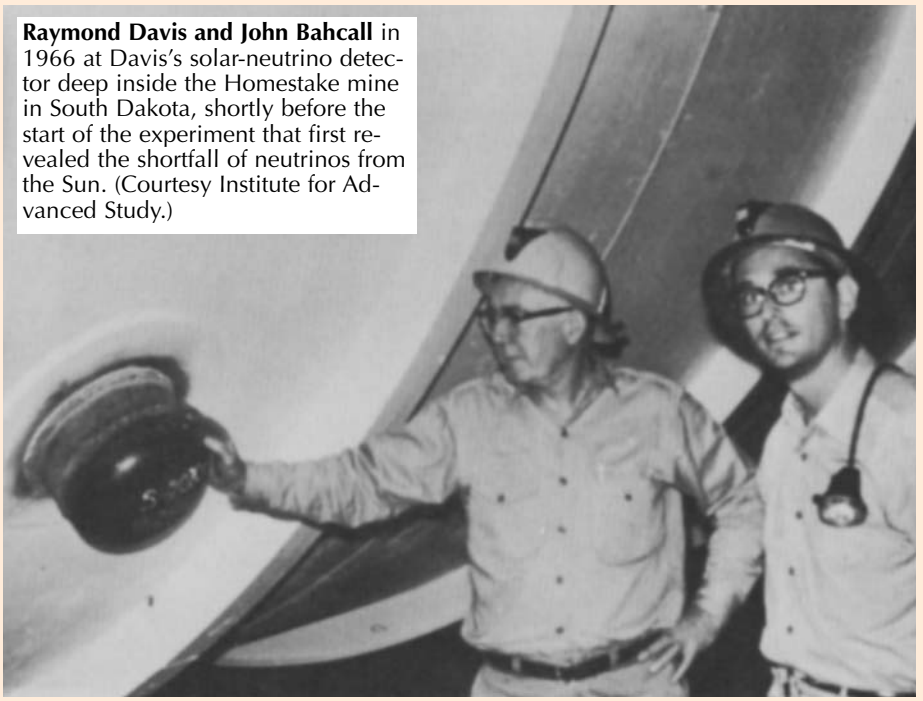
Two interesting aspects of the 1939 paper are not well known. First, given his important role in solar-neutrino studies in the 1980s and 1990s, it is amusing that Hans did not include a neutrino in any of the paper's nuclear reaction equations. For example, he left out the emerging neutrino in

$$p + p \rightarrow {}^2H + e^+ + \nu.$$

The explanation is simple. In 1934, Hans and Peierls, invoking dimensional arguments, had set an upper limit of 10^{-44} cm^2 on neutrino absorption cross sections.[3] Not unreasonably, they concluded that it would be "impossible to observe processes of this kind with the neutrinos created in nuclear transformation." The following year, Hans concluded from the absence of observed ionization by neutrinos that any neutrino magnetic moment must be much less than that of the electron.[4]

Second, Hans discussed the reactions ${}^2H + {}^2H$ and ${}^4He + {}^4He$ in the 1939 paper, but not the analogous reaction ${}^3He + {}^3He$. The ${}^3He$ reaction is, in fact, the dominant way of completing hydrogen burning to helium in modern solar models. So we asked Hans why he hadn't considered it in his comprehensive survey of nuclear fusion in main-sequence stars. Characteristically, he answered immediately: "I didn't think of it." Hans didn't waste time tooting his own horn.

Hans's 1939 paper laid the conceptual foundation for solving the energy-production problem in main-sequence stars. He explained, in outline, how the Sun shines. But he always wanted to make the theory more quantitative. Hans brought a few things up to date on the CNO cycle in a 1940 paper, but there was still a fair bit of unfinished



Raymond Davis and John Bahcall in 1966 at Davis's solar-neutrino detector deep inside the Homestake mine in South Dakota, shortly before the start of the experiment that first revealed the shortfall of neutrinos from the Sun. (Courtesy Institute for Advanced Study.)

Bethe and his wife Rose in 1995

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business on both the p–p chain and the CNO cycle. Then, with the coming of war, astrophysics had to wait.

After the war

After his work directing the theory group at Los Alamos during the war, Hans returned to Cornell in 1945. In his reminiscences, Hans said modestly that he did not return to astrophysics in earnest until 1978, when he and Gerald Brown started to work on supernovae. That was indeed a major effort, which Gerry describes in his article on page 62 of this issue. But Hans also had a tremendous, although indirect, impact on astrophysics in the 1950s and 1960s, partly by energizing William Fowler's group at Caltech. That stimulation involved many youngsters, including the two of us, who worked on subtopics of stellar energy generation—including the unfinished business of the p–p chain and the CNO cycle.

One example of Hans's prowess in making things clear-cut and simple was the effective-range theory[5] of nuclear scattering he invented in 1949. This new analysis technique soon enabled others to put the p–p reaction rate on a more secure footing. Hans's encouragement went beyond proton burning in main-sequence stars to another topic in which he had first become interested at the 1938 Washington meeting—that is, energy generation and the production of heavier elements in evolved stars consisting mainly of helium.

Besides his involvement in Fowler's endeavors, Hans had other indirect influences on astrophysics. Already in the 1940s, together with Robert Marshak, he had written some definitive papers on white dwarf stars. That work stimulated interest in transitional evolutionary stages preceding the white-dwarf stage, especially in central stars of planetary nebulae, where energy losses to neutrinos are important. Papers on the relevant neutrino reactions were written in the 1960s by Giles Beaudet, Hong-yi Chiu, Vahe Petrosian, and Hubert Reeves, all of whom had been Cornell graduate students at one stage or another.

The missing solar neutrinos

Starting in the late 1960s, Raymond Davis and coworkers reported that their neutrino detector, deep inside a South Dakota gold mine, recorded solar neutrinos at only about ⅓ the rate predicted by a rather precise solar model from which one would have expected better. Davis's radiochemical detector, a large vat of chlorine-rich fluid, was sensitive only to electron neutrinos—the only neutrino type produced by p–p or CNO reactions in the solar core.

Addressing the mystery of the missing solar neutrinos in 1986, the 80-year-old Hans wrote an influential paper that explained the Mikheyev-Smirnov-Wolfenstein (MSW) effect in language with which nuclear, atomic, and molecular physicists would be familiar.[6] In a Soviet journal, Stanislav Mikheyev and Alexei Smirnov had recently made the important suggestion that if neutrinos oscillate between different types, as Lincoln Wolfenstein and others had suggested, the metamorphosis of solar neutrinos might be resonantly amplified by matter effects in the Sun.

Using Wolfenstein's formalism, Mikheyev and Smirnov showed that resonant neutrino conversion by matter outside the solar core could elegantly explain the discrepancy between Davis's observations and the solar model. The MSW effect would convert a large fraction of the electron neutrinos produced in the solar core to neutrino types invisible to Davis's detector.

Hans's explanation of the MSW effect—in terms of avoided crossings of nearly degenerate energy levels—appeared before the Mikheyev–Smirnov paper was published in English translation. The fact that he considered the MSW idea in particular, and new neutrino physics in general, worth exploring as a possible solution of the long-standing astrophysical problem energized a number of nuclear and particle physicists. Some of them went on to do extraordinarily beautiful and important solar-neutrino experiments. It suddenly became more fashionable to discuss

ways in which physics beyond the standard model of particle theory could affect neutrino propagation from the interior of the Sun to detectors on Earth.

In 1990 Hans and one of us (Bahcall) demonstrated that if one compared the Davis results with newer data from a water-Cherenkov detector that had some limited sensitivity to other neutrino types, one had to conclude that the overall observational picture of the solar-neutrino deficit "requires new physics."[7] The case was made even clearer in 2001 by first results from the Sudbury Neutrino Observatory (SNO) in Ontario, whose heavy-water core could record neutrinos of all three types with equal sensitivity. Therefore SNO could confront the solar model in detail, irrespective of neutrino metamorphoses on the journey from the solar core to the terrestrial detector. The SNO experiment confirmed that the Sun's total output of neutrinos of all types was in excellent agreement with the solar-model prediction for the production of electron neutrinos in the Sun's core.

After the beautiful SNO result, Hans never wavered from his conviction, expressed in the 1990 paper, that physics beyond the standard model of particle theory was required to solve the solar neutrino problem. The standard model assumes, for simplicity, that all neutrino types are massless and therefore cannot metamorphose into one another. In 1993, with new gallium radiochemical detectors also reporting solar-neutrino shortfalls, Hans undertook a detailed Monte Carlo simulation of solar-model uncertainties.[8] The simulation showed unambiguously that without neutrino oscillation no plausible tweaking of the solar model was consistent with all the solar-neutrino data.

Before 1996, Hans often expressed the hope that he would learn the result of the SNO experiment in time for his 90th birthday. In fact, the results did not come until June 2001, when Hans was almost 95. Arthur McDonald, leader of the SNO effort, phoned Hans a few days before the public announcement to tell him that—although he couldn't reveal the result yet—he knew that we would be pleased to see the data. The result, when it was posted on the Web, was a strong confirmation of the solar model that had its beginnings with Hans's 1939 paper.

Collaborating with Hans was an honor and an enormous pleasure for both of us. He was a wonderfully enthusiastic coworker, with tremendous insight and mastery of an extraordinary range of physics. He was particularly skilled at making effective approximations.

We admired Hans as much for his personal qualities of decency, friendliness, honesty, and dedication to moral principles as for his greatness as a scientist. He used to advise his protégés: "Never work on a problem for which you do not have an unfair advantage." That was, in general, good advice. But for Hans himself, it was hardly a limitation.

Those of us who had the good fortune of working with John Bahcall were particularly saddened by his sudden death shortly after he and I had finished this article. I always enjoyed writing papers with John. He brought refreshing originality to the beginning of the work, but then always ended up with meticulous quantitative detail. I will miss John, as will all the youngsters whom he has inspired. —*E. E. S.*

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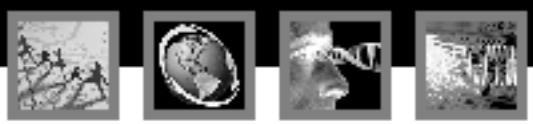

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