The appearance and disappearance of the 17-keV neutrino

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It is a fact of life in empirical science that experiments often give discordant results. This is nowhere better illustrated than in the recent history of experiments concerning the existence of a heavy, 17-keV neutrino. The 17-keV neutrino was first "discovered" by Simpson in 1985. The initial replications of the experiment all gave negative results, and suggestions were made that attempted to explain Simpson's result using accepted physics, without the need for a heavy neutrino. Subsequent positive results by Simpson and others led to further investigation. Several of these later experiments found evidence supporting that claim, whereas others found no evidence for such a particle. Some theorists attempted to explain away the result, and others tried to explain it and to incorporate it within existing theory without the need for a new particle, or to look for the further implications of such a particle, or to propose a new theory that would incorporate the new particle. The question of the existence of such a heavy neutrino remained for several years. Recently, doubt has been cast on the two most convincing positive experimental results, and errors have been found in those experiments. In addition, recent, extremely sensitive experiments have found no evidence for the 17-keV neutrino. The consensus is that it does not exist. The discord has been resolved by a combination of finding errors in one set of experiments and a preponderance of evidence.

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I. INTRODUCTION

It is a fact of life in empirical science that experiments often give discordant results. This is nowhere better illustrated than in the recent history of experiments concerning the existence of a heavy, 17-keV neutrino.[1] What makes this episode so intriguing is that the original positive claim and all subsequent positive claims were obtained in experiments using one type of apparatus, namely, those incorporating a solid-state detector, whereas the initial negative evidence resulted from experiments using another type of detector, a magnetic spectrometer.[2] This is an illustration of discordant results obtained using different types of apparatus. One might worry that the discord was due to some crucial difference between the types of apparatus or to different sources of background that might mimic or mask the signal.

The 17-keV neutrino was first "discovered" by Simpson in 1985. The initial replications of the experiment all gave negative results, and suggestions were made that attempted to explain Simpson's result using accepted physics, without the need for a heavy neutrino. Subsequent positive results by Simpson and others led to further investigation. Several of these later experiments found evidence supporting that claim, whereas others found no evidence for such a particle. Some theorists attempted to explain away the result; others tried to explain it and to incorporate it within existing theory without the need for a new particle, or to look for the further implications of such a particle, or to propose a new theory that would incorporate the new particle.[3] The question of the existence of such a heavy neutrino remained for several years. Recently, doubt has been cast on the two most convincing positive experimental results, and errors have been found in those experiments. In addition, recent, extremely sensitive experiments have found no evidence for the 17-keV neutrino. The consensus is that it does not exist. The discord has been resolved by a combination of finding errors in one set of experiments and a preponderance of evidence.

II. THE APPEARANCE

A. "The discovery"

The 17-keV neutrino was first reported in 1985 by Simpson (1985).[4] He had searched for a heavy neutrino

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[1] The units of mass are in keV/c^2, but physicists usually refer to the masses of particles in energy units such as keV. Physicists currently believe that the mass of the neutrino is zero, or very close to it.

[2] As we shall see, two of the initial negative results were, in fact, obtained with solid-state detectors. Simpson later argued that one of the experiments, Ohi *et al.* (1985), was incorrect, and that the other Datar *et al.* (1985), was inconclusive. There was also suggestive, although not conclusive, evidence from a third type of experiment—that detecting internal bremsstrahlung in electron capture (IBEC), a form of beta decay. This is sometimes referred to as internal or inner bremsstrahlung. Not all of the IBEC experiments gave positive results. As discussed later, one of the experiments that convinced the physics community that the 17-keV neutrino did not exist did, in fact, use a solid-state detector (Mortara *et al.*, 1993).

[3] In this paper I will not discuss the large amount of theoretical work on the 17-keV neutrino unless it impinges directly on the experiments or on the existence of such a particle.

[4] Although, as we shall see later, there is good reason to doubt the existence of the 17-keV neutrino, I shall speak of it as if it existed.

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by looking for a kink in the energy spectrum, or in the Kurie plot,[5] at an energy equal to the maximum allowed decay energy minus the mass of the heavy neutrino, in energy units. The fractional deviation in the Kurie plot value $\Delta K/K \sim R\,[1-M_2{}^2/(Q-E)^2]^{1/2}$, where M_2 is the mass of the heavy neutrino, R is the intensity of the second neutrino branch, Q is the total energy available for the transition, and E is the energy of the electron.[6] Simpson's result is shown in Fig. 1. A kink is clearly seen at an energy of 1.5 keV, corresponding to a 17-keV neutrino. "In summary, the β spectrum of tritium recorded in the present experiment is consistent with the emission of a heavy neutrino of mass about 17.1 keV and a mixing probability of about 3%" (Simpson, 1985, p. 1893).

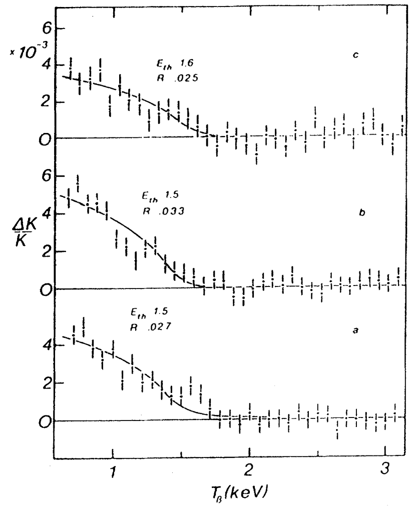

FIG. 1. Data of three runs presented as $\Delta K/K$ (the fractional change in the Kurie plot) as a function of the kinetic energy of the β particles. $E_{\rm th}$ is the threshold energy, the difference between the end-point energy and the mass of the heavy neutrino. A kink is clearly seen at $E_{\rm th}=1.5$ keV, or at a mass of 17.1 keV. Run (a) included active pileup rejection, whereas runs (b) and (c) did not. (c) was the same as (b) except that the detector was housed in a soundproof box. No difference is apparent. From Simpson (1985).

Simpson had been using the apparatus for some time.[7] In 1981 he had attempted to measure, or to set an upper limit on, the mass of the neutrino (to be correct, the mass of the electron antineutrino) by a precise measurement of the end-point energy of the beta-decay spectrum of tritium.[8] If the neutrino had mass, then the measured end-point energy would be lower than that predicted by an amount equal to the mass of the neutrino. In addition, the shape of the energy spectrum near the end point was sensitive to the mass of the neutrino. "The precision measurement of the β spectrum of tritium near its end point seems to offer the best chance of determining, or putting a useful limit on, the mass m_ν of the electron antineutrino" (Simpson, 1981a, p. 649). Earlier measurements on tritium had been made with magnetic spectrometers, whereas Simpson used a different type of experimental apparatus, in which the tritium was implanted in a Si(Li) x-ray detector, a solid-state device. Although such an apparatus had worse energy resolution than did the magnetic spectrometers (300 eV as opposed to 50 eV), Simpson felt that that disadvantage could, to a large extent, be circumvented. In addition, source effects and final-state interactions would be different in the two types of experiment. "Clearly, it would be nice to have an experiment different enough from the above [magnetic spectrometers], yet accurate enough to check on the present upper limit on m_ν" (p. 649).

Simpson devoted considerable effort to both the calibration of the apparatus and the details of data recording and analysis.[9] Two of the key elements of the measurement were the energy calibration and the energy resolution. The energy was calibrated using x rays of known energy from copper, molybdenum, and silver. The calibration, as well as the stability of the entire recording apparatus, was constantly monitored. Beta-decay spectrum data, as well as those data plus calibration data, were recorded with the use of a slotted wheel, an x-ray chopper. This allowed x rays from the copper-molybdenum calibration source to strike the detector when the slots were open. When the slots were closed, the calibration x rays were excluded. The signal from the detector was routed to different halves of the same multichannel analyzer, depending on whether or not the slots were open. Thus one should observe only the beta-decay spectrum when the slots were closed, and that spectrum with the x-ray calibration lines superimposed, when the slots were open. This is seen in Fig. 2. The energy resolution was determined at the same time using both copper and molybdenum x rays, and in separate experiments using x rays from iron and silver.

In Simpson's earlier low-mass-neutrino search, the energy resolution and calibration near the end-point energy of 18.6 keV had been crucial. In the heavy-neutrino

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[5] In a normal beta-decay spectrum the quantity $K=\{N(E)/[f(Z,E)(E^2-1)^{1/2}E]\}^{1/2}$ is a linear function of E, the energy of the electron. A plot of that quantity as a function of E, the energy of the decay electron, is called a Kurie plot.

[6] This neglects the effects of experimental energy resolution.

[7] Simpson reported, "The decay of tritium has been followed with this detector over a period of four years and the half-life has been determined to be 12.35±0.03 yr, in very good agreement with published values" (1985, p. 1891).

[8] Simpson was searching for a low-mass neutrino with a mass of the order of tens of eV.

[9] Although I shall discuss the details of Simpson's calibration and data analysis here, I shall not, in general, discuss these issues for subsequent experiments unless questions have been raised concerning those details.

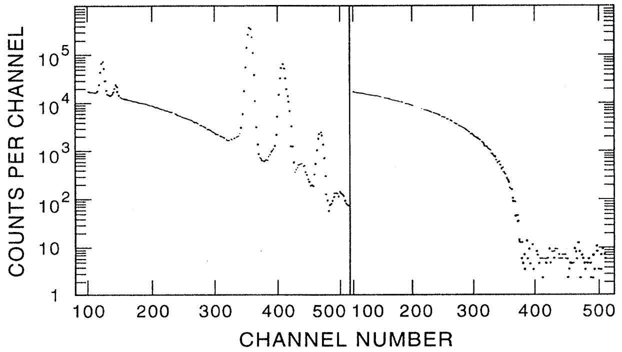

FIG. 2. Logarithmic display of a typical spectrum in the multichannel analyzer. The x rays shown on the left side are those of Cu, Mo, and Ag. The ability of the chopper system to eliminate the x rays is clear. From Simpson (1981a).

search, one had to worry about these factors at low energy, approximately 1.5 keV. “Because of the difficulty of energy calibrating an x-ray detector below about 6 keV, the calibration was established in the following way. The x-rays from Cu and Br, and the Mo K_α were used to determine a linear calibration (with a typical rms deviation of 6 eV). The precision pulsar was then used to measure the pulse-height response over the whole ADC [analog-to-digital converter] range. This was combined with the x-ray calibration to determine a calibration over the whole energy range” (Simpson, 1985, p. 1891).

Another possible problem was pileup, a spectral distortion due to the chance occurrence of two nearly simultaneous β decays. “In one run a pile-up rejection signal from the amplifier was used to veto piled up pulses, and in two others this was not done in order to check that the rejection process did not create an artifact in the spectrum” (Simpson, 1985, p. 1891; see Fig. 1. For further details of the experiment and its analysis, see Simpson, 1981a). The results of his first search were, “The measurement implies a mass <65 eV with 95% confidence and a best value of 20 eV, which is however only 0.2 standard deviations from zero mass” (Simpson, 1981a, p. 649).

Simpson subsequently became aware of theoretical work (McKellar, 1980; Shrock, 1980) that showed that end-point measurements were sensitive to neutrino mass only if it were the dominant decay mode. “There is considerable interest in whether the neutrino (or antineutrino) emitted in weak interactions is a mass eigenstate or a linear superposition of primitive neutrinos of definite mass. If the latter is the case, then energy spectra of β particles will show kinks associated with the emission of energetically allowed neutrinos of different mass. An examination of β spectra can therefore be used to look for massive neutrinos and, if observed, to determine the mixing amplitudes” (Simpson, 1981b, p. 2971). Simpson, using the same apparatus that he had used in his earlier experiment, searched for a neutrino with a mass between 100 eV and 10 keV. He found no evidence for such a neutrino (Fig. 3).

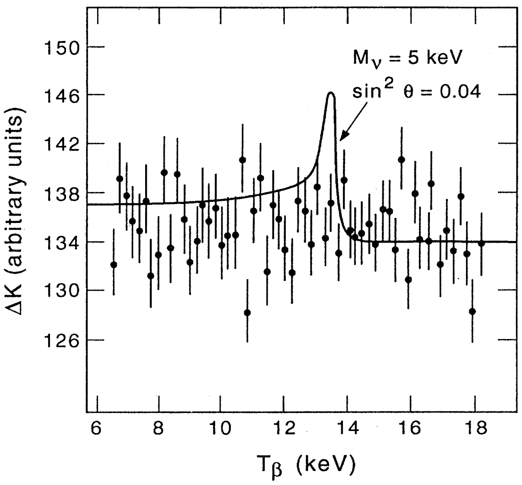

FIG. 3. Magnitude of the difference of adjacent points of the Kurie plot for ^{3}H as a function of the kinetic energy of the β particles. The smooth curve is theoretically expected for a heavy neutrino with a mass of 5 keV and a mixing strength of 4%. From Simpson (1981b).

During the period 1981–1985 there had been, and continues to be, interest in whether or not there are massive neutrinos. This was due, in part, to reports by a Soviet group (Lubimov *et al.*, 1980) that gave limits on the mass of the neutrino of $14 \leq m_\nu \leq 46$ eV, at the 99% confidence level.[10] Schreckenbach *et al.* (1983) had also searched for a massive neutrino and reported, “To conclude, we have found no evidence for a massive neutrino in the nuclear beta decay of ^{64}Cu for the range $m_\nu = 30-460$ keV. Limits below 1% were achieved” (p. 208). Boehm and Vogel reviewed the subject of neutrino mass in 1984 and concluded, “To date there has been no confirmed evidence that neutrinos have finite mass. A reported deviation in the beta decay endpoint in ^{3}He [tritium], if confirmed, may yet indicate a mass in the range 20–30 eV [a reference to the result reported by Lubimov *et al.*]” (p. 131). This was where matters stood when Simpson reported the existence of the 17-keV neutrino.

B. The initial reaction

1. Experimental

Simpson’s positive result for the 17-keV neutrino was published in April 1985. By the end of the year the results of five other experimental searches for the particle had appeared in the published literature (Altzitzoglou

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[10] The question of whether the neutrino has mass, or if it is a superposition of states that have mass, can be separated into two parts. The first is whether or not it is close to zero mass, but finite. The second is whether or not a heavy neutrino, with mass of order keV, exists. In this essay I shall concentrate on the latter.

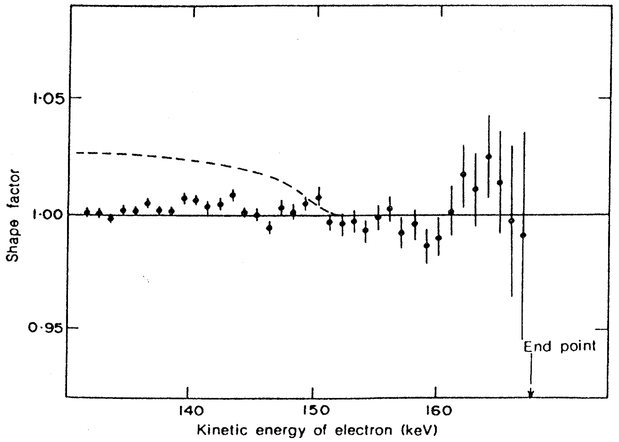

FIG. 4. Shape factor of the β spectrum of ^{35}S. The dashed line is the shape expected for a 17-keV neutrino with a mixing strength of 3%. From Datar *et al.* (1985).

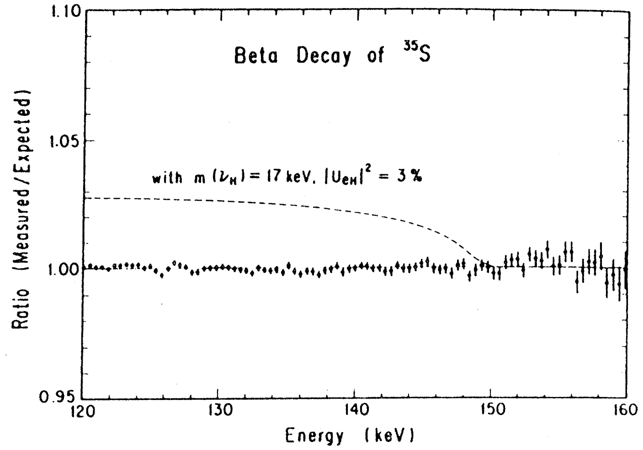

FIG. 5. Ratio of the measured ^{35}S beta-ray spectrum to the theoretical spectrum. A 3% mixing of a 17-keV neutrino should distort the spectrum as indicated by the dashed curve. From Ohi *et al.* (1985).

et al., 1985; Apalikov *et al.*, 1985; Datar *et al.*, 1985; Markey and Boehm, 1985; Ohi *et al.*, 1985). All of them were negative. The experiments set limits of less than 1% for a 17-keV branch of the decay, in contrast to Simpson's value of 3% (see Table I). Typical results are shown in Figs. 4 and 5 and should be compared to Simpson's result shown in Fig. 1. No kink of any kind is apparent.

Each of the experiments examined the beta-decay spectrum of ^{35}S and searched for a kink at an energy of 150 keV, 17 keV below the end-point energy of 167 keV. Three of the experiments—those of Altzitzoglou *et al.*, of Apalikov *et al.*, and of Markey and Boehm—used magnetic spectrometers. Those of Datar *et al.* and of Ohi *et al.* used Si(Li) detectors, the same type used by Simpson. In the latter two cases, however, the source was not implanted in the detector, as Simpson had done, but was separated from it. Such an arrangement would change the atomic physics corrections to the spectrum. In addition, as noted above, the experiments used a ^{35}S

TABLE I. Summary of results (Hime, 1992).

Experiment	Isotope	$(\sin^2\theta)\times 100$	M_2 (keV)	Reference
Solid State				
Guelph	^{3}H in Si(Li)	2-3	17.1	Simpson (1985)
INS Tokyo	^{35}S	<0.15 (90% C.L.)	17	Ohi *et al.* (1985)
Bombay	^{35}S	<0.60 (90% C.L.)	17	Datar *et al.* (1985)
Guelph	^{3}H in Si(Li)	1.10±0.30	17.07±0.09	Hime and Simpson (1989)
	^{3}H in HPGe	1.11±0.14	16.93±0.07	Hime and Simpson (1989)
	^{35}S	0.73±0.11	16.9±0.4	Simpson and Hime (1989)
Oxford	^{35}S	0.78±0.09	16.95±0.35	Hime and Jelley (1991)
	^{63}Ni	0.99±0.22	16.75±0.36	Oxford report
LBL	^{14}C in HPGe	1.2±0.3	17.1±0.6	Sur *et al.* (1991)
IBEC Studies				
CERN/ISOLDE	^{125}I	<2.0 (98% C.L.)	17	Borge *et al.* (1986)
Zagreb	^{55}Fe	<1.6 (95% C.L.)	15–45	Zlimen *et al.* (1988) Zlimen *et al.* (1990)
	^{71}Ge	1.6±0.8	17.1±1.3	Zlimen *et al.* (1991)
LBL	^{55}Fe	0.85±0.45	21±2	Norman *et al.* (1991)
Buenos Aires	^{71}Ge	0.80±0.25	13.8±1.8	TANDAR preprint
Magn. Spectrom.				
Princeton	^{35}S	<0.40 (99% C.L.)	17	Altzitzoglou *et al.* (1985)
ITEP	^{35}S	<0.17 (90% C.L.)	17	Apalikov *et al.* (1985)
Caltech	^{35}S	<0.25 (90% C.L.)	17	Markey and Boehm (1985)
	^{63}Ni	<0.25 (90% C.L.)	17	Wark and Boehm (1986)
Chalk River	^{63}Ni	<0.28 (90% C.L.)	17	Hetherington *et al.* (1987)
Caltech	^{35}S	<0.60 (90% C.L.)	17	Becker *et al.* (1991)
Munich	^{177}Lu	<0.80 (83% C.L.)	17	Conf. report

beta-decay source, which had a higher end-point energy than did the tritium used by Simpson (167 keV in contrast to 18.6 keV). As discussed below, this higher end-point energy made particular corrections to the beta-decay spectrum less important.

2. Theoretical

Questions were also raised concerning the theoretical model used by Simpson to analyze his data. In order to demonstrate that a kink existed in the beta-decay spectrum, one had to compare the measured spectrum with that predicted theoretically. This involved a rather complex calculation, which included various atomic physics effects, particularly screening by atomic electrons, and it was Simpson's calculation of these effects that was questioned. Haxton (1985) noted, "A number of conventional approximations in treating final-state Coulomb effects should fail for small β energies [Simpson's kink had been observed at very low energy]. . . . A particular class of the neglected atomic effects, those corresponding to exchange terms in the sudden approximation are shown to generate corrections of order η^4 [a parameter related to the electron energy] to the standard Coulomb function, producing a distortion in the β spectrum qualitatively similar to that observed by Simpson. Similarly, the standard treatment of screening corrections becomes unreliable whenever η is not small. Thus it is possible that a complete treatment of atomic effects will provide a conventional explanation of the observed distortion" (p. 807). Haxton's own calculation indicated that "exchange corrections are shown to produce a distortion in the tritium beta spectrum similar in shape to that for heavy-neutrino emission, though significantly smaller" (p. 807). See Fig. 6.

A similar point was made by Eman and Tadic (1986).

> The recent observation of a distortion in the β decay of tritium for electron kinetic energies $T < 1.5$ keV depends on the choice of the Fermi function $F(Z,W)$. This function enters into the Kurie plot in which the expression $K = [N_\beta(Z,W)/pWF(Z,W)]^{1/2}$ is plotted vs T. Here $N_\beta(Z,W)$ is the measured number of β particles at an energy W and a momentum p, and Z is the charge of the daughter nuclei. In principle, the Fermi function $F(Z,W)$ includes all known effects, such as finite size, screening, radiation, exchange, and higher multipoles. Screening corrections will be discussed in the next section. These corrections lower the value of the Fermi function $F(Z,W)$ for β particles of low kinetic energy T. Hence the value of K increases at low T, in comparison to the Fermi function $F_0(Z,W)$ calculated for the Coulomb potential.
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> The main aim of this paper is to study the screening corrections. Should these turn out to be smaller than those used by Simpson, the value of K would increase at low T, so that the hump [kink] in the Kurie plot would disappear. In fact, our analysis indicates that this might very probably be the case, so that the observed distortion might have a more conventional origin. However, the uncertainties in the calculation of the Fermi function do not allow one to rule out heavy-neutrino emission completely. (p. 2128)

The results of their calculation are shown in Fig. 7. The calculation, however, depended strongly on a parameter, D, whose value was not well determined. They also noted that experiments on ^{35}S involved higher kinetic energies, where screening effects were expected to be less important.

A further attempt to explain Simpson's result using accepted physics was made by Lindhard and Hansen (1986). They considered atomic physics corrections beyond those already discussed. "A detailed account of the decay energy and Coulomb-screening effects raises the theoretical curve in precisely the energy range [1.5 keV in the tritium beta-decay spectrum] so that little, if any, of the excess remains" (p. 965). Drukarev and Strikman (1986) also considered atomic effects in beta decay. They concluded that "the final-state interaction of a β electron with atomic electrons has been calculated to accuracy $(\alpha Z/v)^2$. It is shown that previous studies devoted to the final-state interaction have not taken into ac-

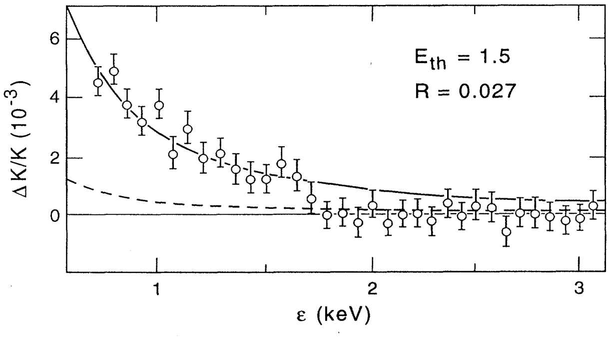

FIG. 6. Simpson's data compared to the theoretical deviation in $\Delta K/K$ attributable to the neglect of screening corrections in the standard treatment of Coulomb distortions. The dashed line is the theoretical calculation. The solid line is the theoretical result multiplied by 6. From Haxton (1985).

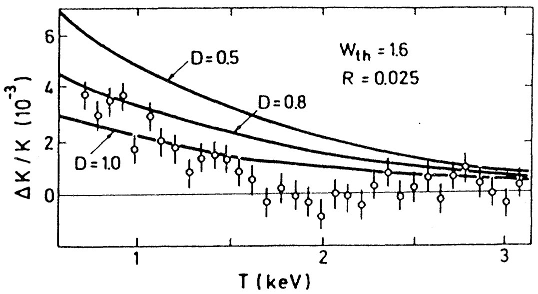

FIG. 7. $\Delta K/K$ as a function of the kinetic energy of the β particles. The curves are not fits to the experimental data of Simpson (1985). From Eman and Tadic (1986).

count all diagrams contributing in the first nonvanishing approximation. Correct allowances for the final-state interaction makes it impossible to explain the discrepancy between the theory and the experimental results of Simpson by the emission of a neutrino" (p. 686).

A different criticism of Simpson's analysis was offered by Kalbfleisch and Milton (1985). They suggested that his result might be an artifact of systematic effects in his experiment. In particular, they noted that Simpson had used a piecewise treatment of the spectrum: 0.7–3.2, 6.5–18, and 9.5–17 keV. Simpson had also allowed the end-point energy to vary considerably in each of the segments, from 18.7 to 19.3 keV. This was far larger than accepted variations. Simpson himself had remarked on this point. "In fitting Q, M_2, R, and an overall normalization were varied. While Q is now well determined to lie between about 18.57 and 18.61 keV, it was necessary to allow it to vary to achieve a good fit in the energy range of interest which is a long way from the endpoint. Incomplete pile-up rejection, *inadequacy of the screening correction to F(E,Z) [the Fermi function]*, and any remaining inaccuracies of the energy calibration could account for obtaining a Q value different from the true one" (Simpson, 1985, p. 1892, emphasis added). They also suggested that Simpson's result argued for a serious discrepancy between theory and experiment for the lifetime of tritium.

By the end of 1985, there were apparently well-confirmed experiments that disagreed with Simpson's claim of a 17-keV neutrino, albeit with a different source (^{35}S in contrast to ^{3}H) and, in some cases, with different types of experimental apparatus. There were also plausible suggestions that might explain his result using accepted physics, and which did not involve a heavy neutrino.[11] Work continued.

C. The search goes on

Although Simpson's claim had been severely challenged, not everyone agreed that it had been conclusively refuted. The situation was more uncertain than it appeared in the published literature. In January 1986, Simpson presented a paper at the Moriond workshop on massive neutrinos (Simpson, 1986b),[12] in which he presented supportive results from an experiment that used a somewhat different apparatus. In this case the detector had been implanted with tritium at a different energy and with a much lower concentration (about $\frac{1}{40}$ that of the original detector). The results are shown in Fig. 8.[13] They are "consistent with the emission of a 17.1 keV neutrino, with a mixing probability between 2 and 3%. It would seem to be not accidental that two detectors by different manufacturers implanted quite differently with very different amounts of tritium should show the same distortion of the β-spectrum of tritium" (Simpson, 1986b, p. 569).[14]

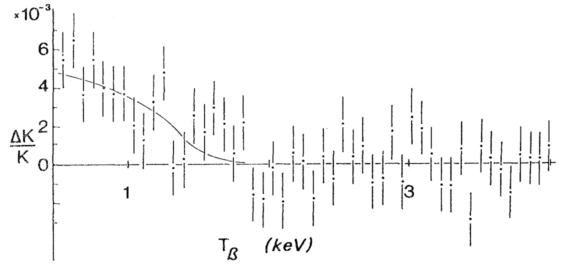

FIG. 8. Distortion of the Kurie plot of ^{3}H as a function of electron kinetic energy, obtained with the low-dose detector. From Simpson (1986b).

Simpson also discussed the question, raised by Haxton and by Eman and Tadic, of the adequacy of the exchange and screening corrections used in this theoretical model. He remarked that different corrections did produce changes in the β spectrum and in $\Delta K/K$, but found that they reduced the size of the kink by approximately 20%. This agreed with Haxton's estimate of the effect. The kink was, however, still clearly present when a different, and presumably better, calculation was used (Fig. 9). Simpson also questioned the negative results reported in the five experiments on ^{35}S. He argued that the type of analysis used, which fitted the beta-decay spectrum over a rather large energy range, would tend to minimize the effect due to a heavy neutrino. He commented that 45% of the effect occurred within 2 keV of the neutrino

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[11] There was considerable discussion among the active researchers in the field. Haxton, Eman and Tadic, and Lindhard and Hansen all acknowledged helpful conversations with Simpson concerning both his experimental apparatus and his theoretical calculations. Although Eman and Tadic's paper was not published until mid-1986, Simpson knew of it by private communication and made use of it in a calculation presented at the Moriond workshop, 25 January–1 February, 1986.

[12] The Moriond Workshops play an extremely important role in speculative/controversial issues. They provide a forum for those working in the field to meet, present papers, and have both formal and informal discussions and criticism. For a discussion of the role that the Moriond workshops played in another controversial episode, that of the fifth force, a proposed modification of Newton's law of gravity, see Franklin (1993a).

[13] Details of the experimental apparatus are contained in Simpson (1985).

[14] Simpson is relying here, as he did earlier in his discussion of why he performed a search for a low-mass neutrino with a solid-state detector, on the idea that "different" experiments provide more confirmation of a hypothesis or of an experimental result than do repetitions of the "same" experiment. For a discussion of this, see Franklin and Howson (1984).

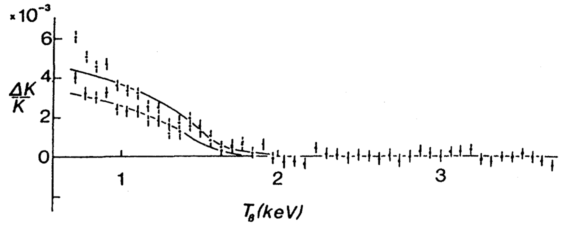

FIG. 9. Effects of screening on the deviation of the Kurie plot. The upper curve used a screening potential of 99 eV, whereas the lower curve used 41 eV. The upper curve gives $\tan^2\theta=0.028$ (mixing probability) and threshold energy 1.57 keV. The lower curve gives a mixing probability of 0.022 and threshold energy 1.53 keV. From Simpson (1986b).

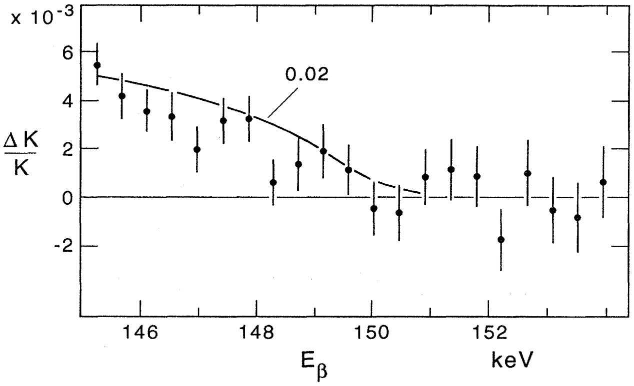

FIG. 10. $\Delta K/K$ for the ^{35}S spectra of Ohi *et al.* (1985) as recalculated by Simpson. From Simpson (1986a).

threshold and that "... in trying to fit a very large portion of the β spectrum, the danger that slowly-varying distortions of a few percent could bury a threshold effect seems to have been disregarded. One cannot emphasize too strongly how delicate is the analysis when searching for a small branch of a heavy neutrino, and how sensitive the result may be to apparently innocuous assumptions" (Simpson, 1986b, p. 576).[15] Simpson reanalyzed the results of each of the five experiments and argued that two of those (Apalikov *et al.*, 1985; Ohi *et al.*, 1985) showed statistically significant effects that agreed with his tritium results. His reanalysis of the result of Ohi *et al.* is shown in Fig. 10 (see also Simpson, 1986a). He also stated that the result of Datar *et al.* was, in fact, consistent with his, but that because of statistical limitations nothing more could be concluded. For the last two experiments, those of Altzitzoglou *et al.* (1985) and Markey and Boehm (1985), he argued that the analysis was inadequate to decide whether there was a distortion in the β spectrum at 150 keV, the 17-keV neutrino threshold.

The situation seemed unresolved. Borge and collaborators (1986), after summarizing the uncertain evidence, which included Simpson's reanalysis, remarked, "Rather than entering into this controversy here, we provide our own independent piece to the puzzle" (p. 591).[16] Their experiment looked at a related, but somewhat different, phenomenon in beta decay, internal bremsstrahlung in electron capture (IBEC). In ordinary electron capture, a nucleus of charge Z absorbs an atomic electron, transforming itself into a nucleus with charge Z-1, with the emission of a monoenergetic neutrino. In the process of capture, the electron may interact with the atomic electrons and produce a photon (usually in the x-ray energy region). This is internal bremsstrahlung electron capture. This latter process produces a continuous spectrum of x rays and is reduced relative to ordinary electron capture by a factor of α, the fine-structure constant, approximately $\frac{1}{137}$. Under certain favorable conditions, as shown by De Rujula (1981), when the energy available for the decay, the Q value, is resonant with electron binding energies in the atom, the rate of IBEC can be increased by several orders of magnitude. It can then be used as a sensitive alternative test for a massive neutrino.[17] This experiment also involved the detection of x rays rather than the detection of electrons, which made it somewhat different.

In this experiment, too, much depended on the theoretical model used for comparison with the experimental result. Borge and his collaborators found that when they fitted their spectrum of ^{125}I with a six-parameter curve, which included the mass and the mixing probability of the heavy neutrino as free parameters, "the effect of the heavy neutrino to a large extent can be absorbed by other parameters.... Thus, in comparing different hypotheses for m_2,c_2 [the heavy-neutrino mass and mixing probability] it is essential each time to carry out an independent adjustment of the other free parameters. Analogous problems occur, of course, in the ^{35}S experiments. We feel, *in complete agreement with the opinions expressed by J. J. Simpson...that the limits on c_2 derived in [the experiments of Ohi et al. (1985) and of Datar et al. (1985)] are misleading as the parameters were not fitted again under the assumption of a heavy neutrino; instead the contribution from this was simply added.* The

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[15] As we shall see, others agreed with Simpson. Bonvicini (1993), in a very detailed analysis, showed that a smoothly varying shape-correction factor could, in fact, either mask or mimic a kink in the spectrum. This will be discussed later. It was also noted that the method of analysis chosen might create a signal when one was not really present. This question of the energy range used in the analysis of the data will be quite important in the subsequent history.

[16] A preliminary report of this experiment appeared in Riisager (1986).

[17] As Borge *et al.* (1986) pointed out, IBEC is actually sensitive to the mass of the neutrino, whereas ordinary beta decay involves an antineutrino. This then made the experiment a test of CPT invariance, which requires that particles and antiparticles have identical masses.

approach taken here, and also in Refs. [7] and [10] (Altzitzoglou *et al.*, 1985; Markey and Boehm, 1985), leads to much more conservative limits on c_2" (Borge *et al.*, 1986, pp. 593–594, emphasis added). They concluded, however, that their result excludes a 17-keV neutrino with a mixing probability of 2–4%, at confidence levels of 98 and 99.9% for the ends of the interval, and that "it supports the results of the ^{35}S measurements, which exclude the corresponding antineutrino" (p. 595).[18]

Negative evidence on the 17-keV neutrino continued to accumulate. Hetherington *et al.* (1987) reported no evidence for the heavy neutrino in their measurement of the beta-decay spectrum of ^{63}Ni, using a magnetic spectrometer. A preliminary negative result had been presented at the 1986 Osaka conference (Hetherington *et al.*, 1986). "However, there was some concern about this conclusion because of the relatively strong absorption in the detector window and other possible instrumental effects. In this paper we present results from an entirely new set of data taken with a thinner window and with explicit evaluation of the impact of instrumental corrections" (Hetherington *et al.*, 1987, p. 1504).[19]

There was evidence of continuing cooperation and collaboration within the beta-decay community. "Simpson drew our attention to the fact that a measurement of the shape of the ^{63}Ni beta spectrum could provide an ideal test of the existence of the 17-keV neutrino. This spectrum's end point (67 keV) is lower than that of ^{35}S [167 keV], offering better resolution and counting statistics, but high enough to avoid the very low-energy problems associated with tritium. It has a single allowed branch with a half-life long enough (100 yr) to avoid normalization problems" (Hetherington *et al.*, 1987, p. 1504).

The need for care in the performance of the experiment was also evident. In preliminary measurements, excess counts were found above the end-point energy of the ^{63}Ni spectrum, which indicated the presence of background, most probably due to scattering of the decay electrons. Extra antiscatter baffles were added to the experimental apparatus, which solved the problem.[20] The group took data in a broad energy range, 25–70 keV, with additional runs in the narrower energy range 46–54 keV, in which effects of the 17 keV neutrino, if it existed, would appear. Thus such effects could be sought in both narrow and wide energy ranges. Recall Simpson's earlier comment about the possibility that using a wide energy range might hide a threshold effect due to the 17-keV neutrino.

There were also difficulties in calculating the expected spectrum shape that was to be compared with the experimental data. Despite the best efforts of the group, "it was found in the analysis that a shape 'correction' of the form $S=(1+\alpha E)$ was required in order to obtain a good fit. This is probably caused by uncertainties in the instrumental corrections, e.g., window absorption, penetration through the edges of the counter slits, electrostatic effects on transmission, etc.... It should be noted that the inclusion of an unknown shape correction (α) does not bias the result obtained for $|U_{e2}|^2$ [the mixing probability] provided that both parameters are allowed to float simultaneously (as was the case in all results quoted here except where otherwise noted). This reflects the ability of the least squares technique to distinguish between a continuously varying effect in the data and a discontinuous threshold effect" (Hetherington *et al.*, 1987, p. 1508).[21]

Their conclusions for both the wide-scan and narrow-scan spectra agreed (the results for the wide-scan spectrum are shown in Fig. 11). "The shape of the plot and the reduced χ^2 value clearly rule out this large a mixing fraction [3%] for the 17 keV neutrino" (p. 1510). They set an upper limit of 0.3% for the mixing probability of the 17-keV neutrino. They agreed with Simpson that the stricter limit of 0.15% set by Ohi *et al.* was probably not warranted because of the analysis procedure used. They did, however, offer a note of caution concerning Simpson's analysis. "It has been argued [by Simpson] that in order to avoid systematic errors, only a narrow portion of the beta spectrum should be employed in looking for the threshold effect produced by heavy neutrino mixing. If one accepts this argument, our data in the narrow scan region set an upper limit of 0.44%. However, we feel that concentrating on a narrow region and excluding the rest of the data is not warranted provided adequate care is taken to account for systematic errors. The rest of the spectrum plays an essential role in pinning down other parameters such as the endpoint. Furthermore, concentrating on too narrow a region can lead to misinterpretation of a local statistical anomaly as a more general trend which, if extrapolated outside the region, would diverge rapidly from the actual data" (p. 1512).[22] This experiment was generally regarded as the most complete magnetic spectrometer experiment done to that point (see Bonvicini, 1993, p. 98).

Further evidence against the 17-keV neutrino was provided by Zlimen and collaborators (1988), using the internal bremsstrahlung technique on ^{55}Fe. They concluded, "We obtain a negative result and, at the 99.7% confidence level, our limit for the fraction of emitted neutrinos in the mass range $16.4\rightarrow 17.4$ keV is <0.0074" (p. 539).

The group was quite concerned with the construction

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[18] They also thanked Simpson for interesting discussions.

[19] At the same conference Wark and Boehm (1986) also presented negative results on the 17-keV neutrino.

[20] The presence of such antiscatter baffles themselves could be a source of problems. This possibility is discussed later.

[21] "The penalty paid for having an unknown shape correction is that its interdependence with $|U_{e2}|^2$ raises the error in that parameter" (Hetherington *et al.*, 1987, p. 1508).

[22] The group reported a value for the end-point energy $E_0=66.946\pm0.020$ keV, in disagreement with the accepted value of 65.92 ± 0.15 keV.

of a theoretical model to compare with their experimental result.

> The decay of ^{55}Fe ($Q = 231.4 \pm 0.7$ keV) is an allowed transition and the theoretical understanding of such transitions is well developed.... An experimental investigation has been made by Berenyi *et al.*, with an accuracy comparable to that attained in the study of β-ray shape factors. The agreement between experiment and theory is better than 1% over a wide energy range. As our analysis is limited to a relatively narrow energy range we can be confident that the shape of the IBEC spectrum is known to a high degree of accuracy. However, it must be emphasized that our technique does not depend on there being an absolute accuracy of 1%. It is only necessary that the theory is sufficiently well-established that, in the absence of heavy neutrino emission, there are no kinks in the spectrum in the energy region used in our analysis. The recent careful investigation of Borge *et al.* has also shown that the shape of the IB spectrum is in excellent agreement with the theoretical predictions. (p. 540)

The technique used was to look for kinks in the IBEC spectrum produced by the emission of a heavy neutrino. Internal bremsstrahlung can proceed from different atomic shells, which does, in fact, produce kinks in the spectrum. The kink due to a 17-keV neutrino would occur at an energy of $(Q - 17.1 - B_{1s})$, where Q is the energy available for decay and B_{1s} is the binding energy of the 1s state, the lowest energy atomic state, and the dominant decay mode. No kinks are expected below this energy, and, in order to have an energy range in which only one kink was expected, they set an upper limit to their energy of $(Q - 17.1 - B_{2s})$, where B_{2s} is the binding energy of the 2s state.

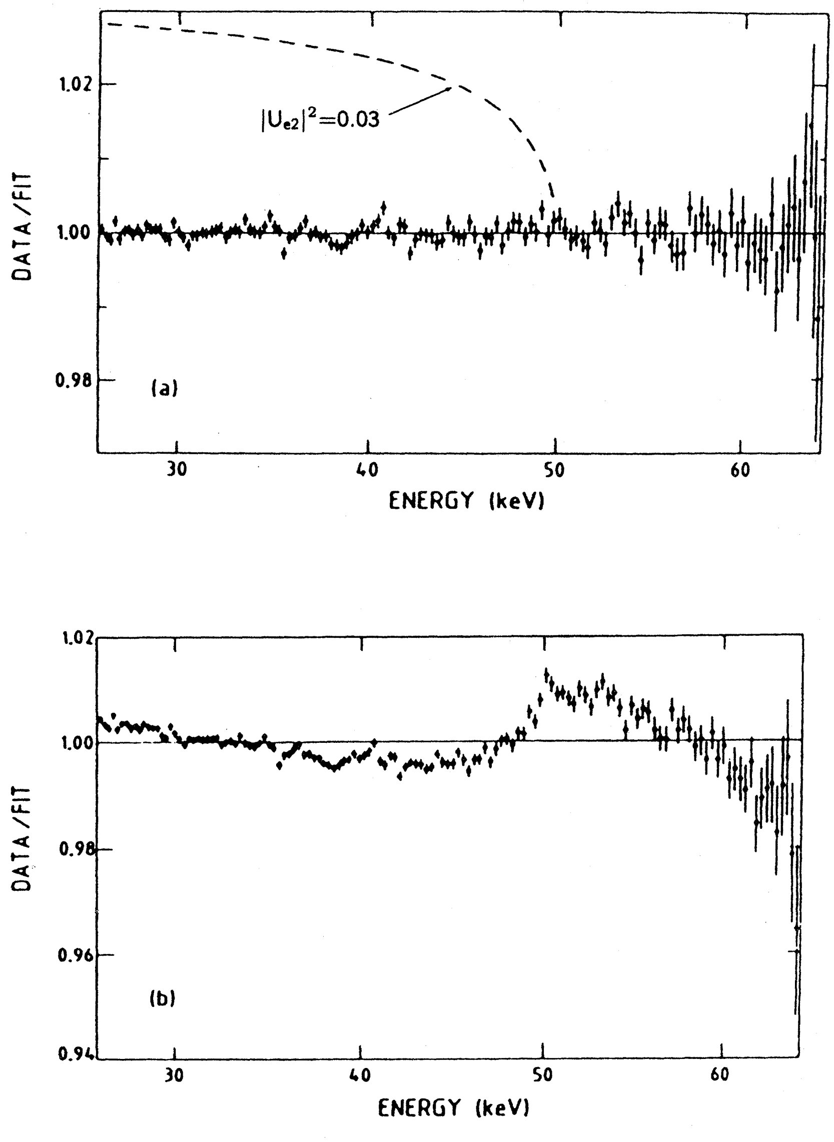

FIG. 11. "Ratio of data to fit for wide scan spectrum. (a) $|U_{e2}|^2$ fixed to zero and the other parameters optimized. For comparison the dashed line shows the expected shape for $|U_{e2}|^2=3\%$. The best fit parameters are $E_0=66.946$ keV, $\alpha=0.000\,65$ keV^{-1}, and $\chi^2_\nu=0.862$. (b) Best fit for $|U_{e2}|^2$ fixed at 3%. The other parameters are $E_0=67.019$ keV, $\alpha=0.0049$ keV^{-1}, and $\chi^2_\nu=7.76$. The shape of the plot and the reduced χ^2 value clearly rule out this large a mixing fraction for the 17 keV neutrino." From Hetherington *et al.* (1987, p. 1510).

At the end of 1988 the situation seemed much as it had been at the end of 1985. There seemed little reason to believe in the existence of a 17-keV neutrino. Aside from Simpson's original result and his reanalysis of the negative results of others, no other evidence for such a particle had been presented. There had been nine negative experimental searches as well as plausible explanations that might explain his result using accepted physics.

D. The tide starts to turn

In April 1989,[23] two new experimental results, obtained by Simpson and Hime, were published that supported the existence of the 17-keV neutrino (Hime and Simpson, 1989; Simpson and Hime, 1989). The effect of discussions and criticism within the research community on the performance and analysis of experiments, noted earlier, is clearly seen in these papers.

The first experiment was done on ^{3}H (tritium; Hime and Simpson, 1989), the same substance used in Simpson's original experiment. Once again, the tritium was implanted in a solid-state detector, but in this experiment the detector was a hyperpure crystal of germanium, rather than an Si(Li) detector. "It was deemed important to check the earlier result by measuring the ^{3}H β spectrum in a different detector" (p. 1837). One problem with embedding the tritium in a germanium detector is that the embedding process may cause radiation damage, which causes pulse-height defects and will therefore result in an incorrect spectrum. It was known, however, that such damage could be removed by annealing at a temperature $\leq 200\,^\circ$C, whereas the tritium remains bound in the germanium for temperatures up to $500\,^\circ$C.[24] The annealing was done in several steps. The crystal was first removed from the cryostat and allowed to warm to room temperature. Although this seemed to remove the pulse-height defect, a 0.45-mm dead layer remained in the detector. Further annealing took place *in situ* using heating coils to attain temperatures from 90 to 135 °C. A dead layer of 0.14 mm remained, and further annealing, by heating to 180 °C for about 10 hours, was done. This solved the problem completely. Possible experimental difficulties concerning whether or not the decay electron energy would be completely absorbed in the detector were solved by implanting the tritium in the center of the detector, to avoid edge effects, and by embedding it with sufficient energy so that its depth was approximately 0.3 mm, which was large compared to the mean absorption length in germanium of approximately 20 μm for 18.6-keV photons, and large compared to the mean path length for 18.6-keV electrons ($\sim 1.6\ \mu$m), the maximum energy of the decay. (For further details see Hime and Simpson, 1989, pp. 1839–1841.)

Data were taken both after the *in situ* annealing, using two different detector electronics systems, and in two longer runs taken after the annealing process was completed (runs B and C; run C had improved energy resolution). The results from all four runs were consistent, and the results for runs B and C are shown in Fig. 12. Hime and Simpson concluded, "The excess of counts observed in the low-energy region of the tritium spectrum is best described by the emission of a 16.9 ± 0.1 keV neutrino and a mixing probability between 0.6 and 1.6% when allowance is made for uncertainty in the effective screening potential appropriate for tritium bound within a crystal lattice" (p. 1837).

Notice that the mixing probability has decreased by approximately a factor of 3 when compared to Simpson's original result. Recall, however, that questions had been raised concerning the theoretical corrections for screening and exchange. Hime and Simpson remarked that using the screening potential suggested by Lindhard and Hansen (1986) reduced the original 3% mixing probability to 1.6%. They also analyzed both their new germanium data and the original Si(Li) data, allowing the screening potential to vary and looking for the best fit. They found, for the original data, best values of 38 ± 10 eV and $(1.1\pm0.3)\%$, for the screening potential and mixing probability, respectively. This was in good agreement with the values of 42.6 eV and $(1.1\pm0.2)\%$ for the new

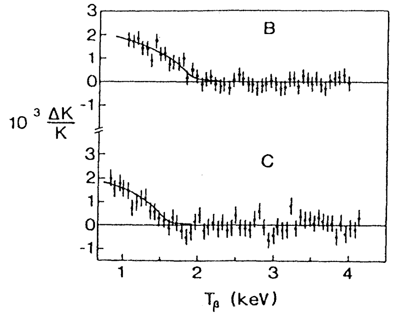

FIG. 12. "The fractional deviations $\Delta K/K$ in the Kurie plots of spectrum B and spectrum C from a straight line using an effective screening potential of 42.6 eV. The smooth curves are as predicted in Eq. (5) for the emission of a heavy neutrino after accounting for resolution smearing. $M_2=16.85$ keV with $\sin^2\theta=1.1\%$ in the case of spectrum B with FWHM$\approx$405 eV. $M_2=17.00$ with $\sin^2\theta=1.3\%$ in the case of spectrum C with FWHM$=$310 eV" (Hime and Simpson, 1989, p. 1846). These results are for tritium. M_2 is the mass of the heavy neutrino, and $\sin^2\theta$ is the mixing probability. Run C had somewhat better energy resolution.

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[23] The papers were received at the *Physical Review* on September 9, 1988, and the results were, no doubt, known to those working in the field well before publication.

[24] Such a detector is normally run at liquid-nitrogen temperature ($-196\,^\circ$C).

germanium data.[25]

The second experiment done by Simpson and Hime (1989) was on ^{35}S, the element whose spectrum had provided considerable evidence against the existence of the 17-keV neutrino. This experiment used two different ^{35}S sources, 0.5 μCi and 5.0 μCi, and a Si(Li) detector. This detector also runs at liquid-nitrogen temperature (-196 °C), and a problem was found with the buildup of water vapor on the detector. "In addition, a copper cryopanel of ~ 300 cm^2 surface area surrounds the silicon detector and is cooled to liquid nitrogen temperature through a copper cold finger. This provides a large cold surface that freezes out residual water vapor in the chamber that can otherwise freeze on the detector surface. Without this cold surface a continuous build-up on the detector took place as observed by a continuous energy shift of the internal conversion electron lines of ^{57}Co. The centroid positions and shape of these lines remain sufficiently stable for periods of 4–5 days with the copper cryopanel in place" (p. 1826). An experimental problem had been identified and solved.

The main source of possible distortion of the β spectrum in their spectrometer was backscatter or back diffusion of the decay electrons. Simpson and Hime claimed that experiments by others had shown that the fraction of electrons backscattered was approximately 32%, and that it was essentially independent of the electron energy (see Simpson and Hime, 1989, for references).

The two runs with different sources, *B* and *C*, gave similar results and were combined to give a final result of $M_2 = 16.9 \pm 0.4$ keV and $\sin^2\theta = (0.73 \pm 0.09)\%$, for the mass and mixing probability of the heavy neutrino, respectively (Fig. 13). "A threshold anomaly 17 keV from the end point in the measured ^{35}S spectra from that expected from theory for the emission of a single-component massless neutrino is the only distortion observed in the spectrum over the energy interval ranging from 110 to 166 keV. The agreement between theory and experiment below this anomaly indicates that the systematic effects associated with the technique of the measurement, including detector response function and background, are well understood. It is very unlikely that systematic uncertainties would affect the shape of the spectrum in only an isolated region and not continuously over the entire β spectrum. It must be emphasized that *no arbitrary* shape factor has been required in analyzing the ^{35}S spectra to achieve a good fit, reinforcing confidence in the knowledge of the systematic features governing the shape of the measured spectrum" (Simpson and Hime, 1989, p. 1833).

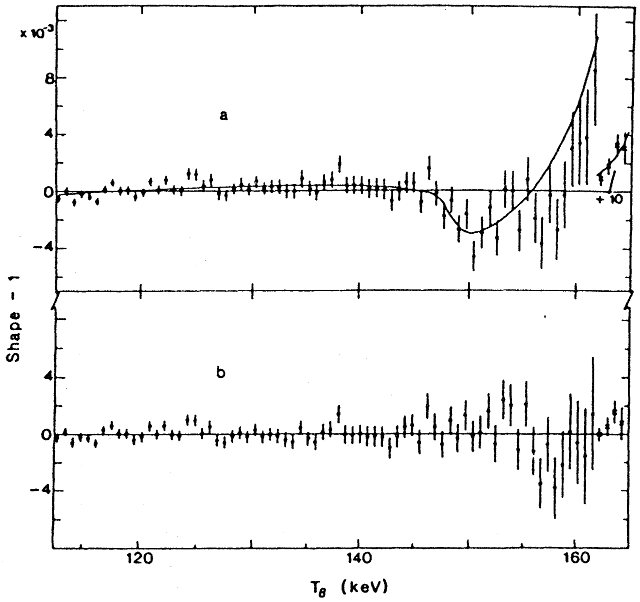

FIG. 13. "The deviation of the shape function from a constant for the combined data of runs *B* and *C* (two different ^{35}S sources, 0.5 μCi and 5.0 μCi). In (a) the theoretical spectrum has $\sin^2\theta = 0$. $\chi^2/\nu = 2.0$. The smooth curve shows the shape expected for $M_2 = 17$ keV and $\sin^2\theta = 0.008$. In (b) the experimental data are divided by a theoretical fit with $M_2 = 17$ keV and $\sin^2\theta = 0.0075$. $\chi^2/\nu = 1.0$" (Simpson and Hime, 1989, p. 1830).

They regarded the two experiments, on tritium and on ^{35}S, as providing increased support for the existence of the 17-keV neutrino. "The present result [on ^{35}S] is remarkably similar to the results of the measurement of the β spectrum of tritium. Since the β energy and the experimental technique are so different in the ^{3}H and ^{35}S measurements, it would have to be a remarkable coincidence for extraneous experimental effects to produce the similar results" (p. 1835).[26]

Simpson and Hime also discussed the previous negative results and concluded, "The present results are in disagreement with the claims of previous groups measuring β spectra of ^{35}S and ^{63}Ni. In the present experiment all important systematic effects are understood and accounted for. This is not generally the case in the other experiments,[27] and it can be argued that, with the possible exception of two of the previous ^{35}S experiments (Apalikov *et al.*, 1985; Ohi *et al.*, 1985), these results are more correctly described as providing no support for a 17-keV neutrino at the 0.7% level rather than ruling it out. The two exceptions perhaps give weak confirmation of the 17-keV neutrino" (p. 1835). Not everyone working in the field agreed.

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[25] "... the goodness of fit is not a strong function of the screening potential used. However, it is important to emphasize that even when zero screening is used the excess of counts at low energy is not completely removed" (Hime and Simpson, 1989, p. 1846). It was still consistent with a mixing probability of 0.5%.

[26] Recall the earlier discussion of the increased support by "different" experiments.

[27] Recall the earlier discussion of the shape factor needed in the experiment of Hime and Simpson (1989).

The continuing experimental work by Simpson and Hime encouraged further theoretical work on corrections to the tritium beta-decay spectrum.[28] Weisnagel and Law (1989) included internal bremsstrahlung effects, which had not previously been considered, as well as other atomic physics effects to produce what they considered to be the most complete theoretical model of the spectrum. Using Simpson's original data and their model, they reported "a best fit for the neutrino mass $m_\nu = 17.2$ keV and a mixing probability $R = 2.5\%$" (p. 904), in agreement with Simpson's original result (see Fig. 14).

Weisnagel and Law also suggested that the previous theoretical work of Eman and Tadic (1986) and of Lindhard and Hansen (1986), which had attempted to explain Simpson's result on the basis of atomic physics effects, had overestimated the size of these effects because of differences in spectrum normalization procedures. Their own calculation indicated that the effect suggested by Lindhard and Hansen could explain only one-third of the electron surplus seen by Simpson.

The positive reports by Simpson and Hime encouraged Zlimen and collaborators (1990) to extend their analysis of the internal bremsstrahlung spectrum of ^{55}Fe to the mass range 15–45 keV, in comparison with their original result for the range 16.4–17.4 keV. They concluded, "We have no evidence for the existence of a heavy neutrino with a mass larger than 20 keV. Although our results confirm that any possible heavy neutrino in the 15–20 keV region have relative intensities well below the value of 3% of (Simpson 1985a), they do not exclude the new results of Simpson and Hime. New detailed measurements are needed in this energy range" (p. 426). This was the first report by anyone other than Simpson and his collaborators, or from Simpson's own reanalysis of other experimenters' data, of a result that was consistent with the existence of a 17-keV neutrino. More would follow.

Even before these results were available in the published literature, they had been presented at workshops and conferences and communicated privately to others.[29] For example, Sheldon Glashow, a Nobel Prize-winning theoretical physicist, cited both the published work of Simpson and Hime (1989) and private communications from Hime and from Norman at Berkeley, both of which would soon be published, as evidence for the existence of the 17-keV neutrino. He then incorporated the heavy neutrino in a model that accounted for the solar neutrino deficit while remaining "in accord with known constraints from particle physics and cosmological theory" (Glashow, 1991, p. 255). He was, in fact, quite enthusiastic about Simpson's work. "Simpson's extraordinary finding proves that Nature's bag of tricks is not empty and demonstrates the virtue of consulting her, not her prophets. That a simple extension of the standard model seems to work on earth and in the stars shows she is not malicious" (p. 257).[30]

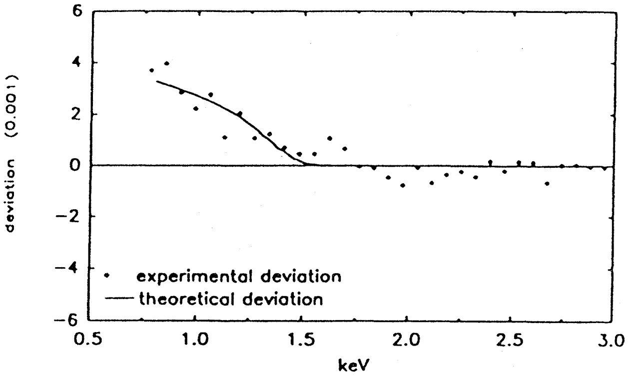

FIG. 14. Deviation spectrum for the tritium experimental data of Simpson as a function of electron kinetic energy for the sum of all the effects studied by Weisnagel and Law (1989) in the energy region up to 3 keV.

Glashow was not alone. In an article in the May 1991 *Physics Today,* Schwarzschild (1991) wrote an article entitled "Four of Five New Experiments Claim Evidence for 17-keV Neutrinos." Schwarzschild cited the positive results already published by Hime and Jelley, as well as the positive results presented at the 1990 Bratislava conference on nuclear physics by Norman (^{14}C and ^{55}Fe) and by Ljubocic. (All of these experiments will be discussed in detail below.) The only new negative result cited had been presented at the 1991 Moriond workshop by Becker and collaborators (1991). Schwarzschild cited Glashow's enthusiastic response, quoted above, along with Shrock's more cautious "To the theorists who say the 17-keV neutrino can't be right, *and* to those who offer

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[28] Although the work by Simpson and Hime may have encouraged the new work, it certainly did not initiate it. During the 1970s, Law, in collaboration with Campbell, published three papers on atomic physics corrections to nuclear beta decay. Law and Weisnagel were colleagues of Simpson at the University of Guelph and acknowledged discussions with Simpson and Hime.

[29] In general, unless specific references are made to private communications or to conference presentations, I shall use the published versions of the papers. The published versions usually have more details and are also when the physics community, rather than the group of specialists, becomes aware of the results. There are times, however, when I shall use these less formal presentations.

[30] Glashow was not an uncritical theorist who accepted experimental results merely because experimenters presented them. In an earlier episode, that of the fifth force, a modification of Newton's law of gravity, Glashow rejected both the speculation and the evidence on which it was based. "Unconvincing and unconfirmed kaon data, a reanalysis of the Eötvös experiment depending on the contents of the Baron's wine cellar [an allusion to the importance of local mass inhomogeneities in the analysis], and a two-standard deviation geophysical anomaly! Fischbach and his friends offer a silk purse made out of three sows' ears and I'll not buy it" (qtd. in Schwarzchild, 1986, p. 20).

a nice model purporting to explain it... our present theories don't even explain the well known fermion masses" (Schrock, qtd. in Schwarzschild, 1991, p. 19). Schwarzschild concluded, "On one thing everyone seems to agree. After six years, the experimenters must begin to resolve the stubborn discrepancy between the two different styles of beta-decay experiment [solid-state detectors and magnetic spectrometers]" (p. 19).

The first of the results to appear in print were those of Hime and Jelley. "After his apprenticeship with Simpson, Hime is now at Oxford, where he and Nick Jelley have recently completed a new measurement of the beta-decay spectrum of ^{35}S (Hime and Jelley, 1991). This high-statistics extension of the Simpson technique with an improved instrumental geometry is, by consensus, the most compelling of the experimental results that claim to see the 17-keV neutrino" (Schwarzschild, 1991, p. 17).[31]

The experimental apparatus used by Hime and Jelley is shown in Fig. 15.[32] It included source and detector apertures as well as an aluminum antiscatter baffle [(d), (c), and (e) in the figure, respectively]. "The aim was to provide a well defined geometry in which electrons are normally incident on silicon, an improvement on the scheme used at the University of Guelph (Simpson and Hime, 1989) where no form of collimation was used" (Hime, 1993, p. 166). This was to ensure that electrons would not penetrate the edges of the detector and to guard against electrons scattering from the walls of the vacuum chamber into the detector, thus possibly distorting the energy spectrum.[33]

During the operation of the experiment, it was found that some electrons were, in fact, losing additional energy by penetrating the edge of the detector, and so the geometry of the apparatus was changed to reduce this effect (runs 1 and 2). During both experimental runs the calibration and stability of the apparatus were monitored in two different ways: (1) having daily calibration runs using gamma rays from a ^{57}Co source, and (2) using monoenergetic electrons from internal-conversion sources at the beginning, middle, and end of each run.

The results of both runs were consistent with each other, and combining results from both sets of data gave $M_2=(17.0\pm0.4)$ keV and $\sin^2\theta=0.0084\pm0.0006 \pm0.0005$, for the mass of the heavy neutrino and the mixing probability, respectively (Fig. 16). (The two uncertainties on the mixing probability are statistical and an estimate of systematic uncertainty, respectively.) Hime and Jelley (1991) concluded, "The data strongly support the claim that the electron neutrino couples to a heavy mass eigenstate in agreement with the measurement of the ^{35}S spectrum at Guelph" (p. 448).

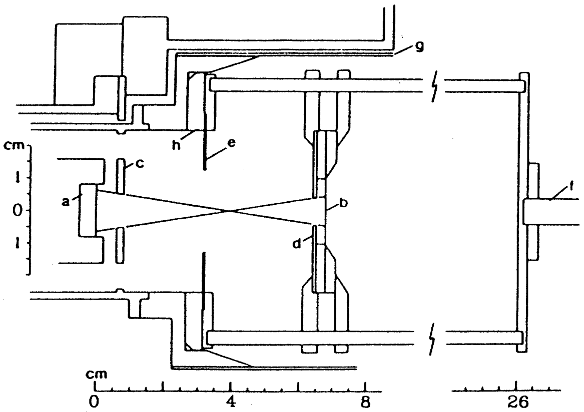

FIG. 15. Experimental apparatus of Hime and Jelley (1991): (a) Si(Li) detector; (b) source substrate; (c) Al detector aperture; (d) Cu source aperture; (e) Al antiscatter baffle; (f) linear motion feedthrough; (g) liquid-nitrogen cryopanel; (h) Teflon centering ring; (i) vacuum chamber.

They noted that by restricting their data to the energy region above 120 keV they had made their result less sen-

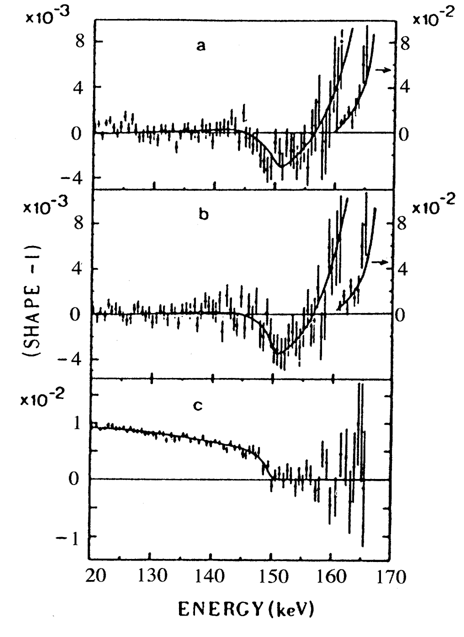

FIG. 16. Shape factors: (a) run 1 and (b) run 2 obtained by dividing the experimental spectra by the best least-squares fit to the region 120–160 keV when no heavy-neutrino mixing is allowed; (c) the combined runs when normalizing to a single component over the region above 150 keV. The smooth curves in each case indicate the expected deviation for the emission of a 17-keV neutrino with $\sin^2\theta=0.009$ (Hime and Jelley, 1991). In run 2 the source aperture was made smaller to reduce the fraction of decay electrons striking the edge of the detector aperture.

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[31] Schwarzschild's comments appeared in *Physics Today,* a semipopular magazine that is distributed to all members of the American Physical Society.

[32] I include the details here because they will be important later in the story.

[33] "Improvements" may not make the experiment better, as we shall see later. For a case in which technological improvements to an apparatus precluded the replication of what ultimately were very important results, see Franklin (1986, Chap. 2).

sitive to fine details of the electron response function. "Consequently, if the distortion observed in the beta spectrum arises from some unknown systematic feature associated with this method of measurement [solid-state detectors], then it appears to be much more subtle than a misunderstanding of the electron response function" (p. 448). They also cited a preliminary result from Berkeley on the ^{14}C spectrum (to be discussed in detail later) as well as the earlier positive results on tritium and ^{35}S as supporting the existence of the 17-keV neutrino. In addition, they remarked on the criticism of the earlier negative results. "To date no response has appeared concerning these criticisms nor has any new result been reported in the literature from the authors of the work" (p. 441).

The Berkeley results on ^{14}C were published at approximately the same time as Schwarzschild's article (Sur *et al.*, 1991).[34] The technique was similar to that used by Simpson, in which the radioactive source had been embedded in the solid-state detector. In this case the detector was a germanium crystal in which ^{14}C was dissolved. A novel feature of the detector was that the electrode was "divided by a 1-mm-wide circular groove into a 'center region,' 3.2 cm in diameter, and an outer 'guard ring.' By operating the guard ring in anticoincidence mode, we can reject events occurring near the boundary, which are not fully contained within the center region" (p. 2444). Such events would give an incorrect energy and thus distort the spectrum. Their results are shown in Fig. 17 and give a value of 17±2 keV and (1.40±0.45±0.14)% for the mass of the heavy neutrino and its mixing probability, respectively, "which supports the claim by Simpson that there is a 17-keV neutrino emitted with ~1% probability in β decay" (p. 2447).[35] They also claimed to rule out the null hypothesis (no heavy neutrino) at the 99% confidence level.

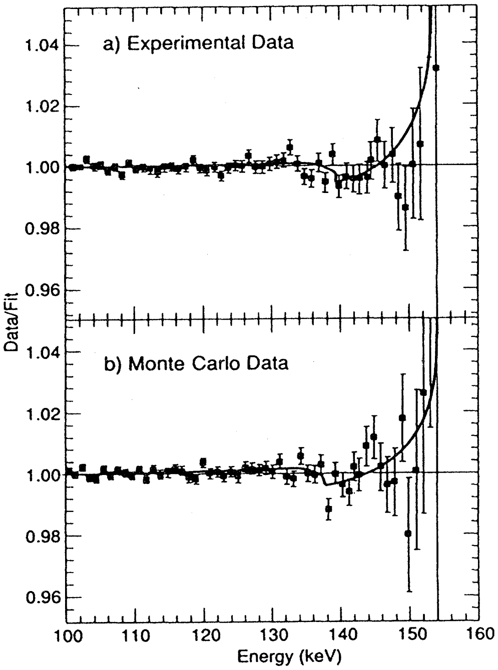

FIG. 17. Ratio of the data to a theoretical fit assuming the emission of only zero mass neutrinos. The horizontal line is the shape expected for zero mass neutrinos. a) Analysis of data; b) Monte Carlo–generated data, which contain a 1% fraction of a 17-keV neutrino. The curves illustrate the shape expected from the best fits to the data, which includes a 17-keV neutrino (Sur *et al.*, 1991).

The Berkeley group included new results on the internal bremsstrahlung spectrum of ^{55}Fe as well as their ^{14}C results in Norman *et al.* (1991). The ^{55}Fe experiment used a germanium detector (a solid-state device) and made use of a sodium iodide anticoincidence shield to veto both Compton-scattered gamma rays and external background. They used the last 55 keV of the ^{55}Fe spectrum and obtained a best fit for $M_2=21\pm2$ keV, a somewhat different value for the mass than had been found in all the other experiments, and a mixing probability of (0.85±0.45)%, where both uncertainties are one standard deviation. They noted, however, that their fit was not as good as that obtained in their ^{14}C experiment, which they attributed to their lack of precise knowledge of their detector response function. They also found that the position of the "kink" (the mass of the heavy neutrino) could be moved by varying the energy dependence of the detector. They concluded, "Thus, these results are suggestive that there is a feature ~17 keV below the end-point of the ^{55}Fe IB [internal bremsstrahlung] spectrum, but further study of this system is clearly necessary" (p. S298).

The results that Ljubcic had presented at Bratislava were published shortly thereafter (Zlimen *et al.*, 1991). The technique used was quite similar to that used in their earlier internal bremsstrahlung experiments (described earlier), though this experiment used a ^{71}Ge source. "The evidence for a small kink is not apparent in a visual inspection of the IB spectrum. However, if we normalize the spectrum to a spectrum which assumes $m_H=0$ [no heavy neutrino], the kink becomes visible. . ." (pp. 562, 563; see also Fig. 18). Their values were $17.2^{+1.3}_{-1.1}$ keV and (1.6±0.79)% for the mass of the heavy neutrino and the mixing probability, respectively. Both of these results were at the 95% confidence level and were in agreement with Simpson's results.

The one new negative result presented at this time was by Becker *et al.* (1991). The experiment used a magnetic spectrometer to measure the beta-deay spectrum of ^{35}S. They, too, allowed a varying shape factor in fitting their data. They fit their data in two energy ranges: a wide

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[34] Although there are several references to the Berkeley results being presented at the Bratislava conference, no paper appears in the published proceedings.

[35] The Berkeley group also used a fitted "shape factor," something that had been criticized in the magnetic spectrometer experiments (see earlier discussion).

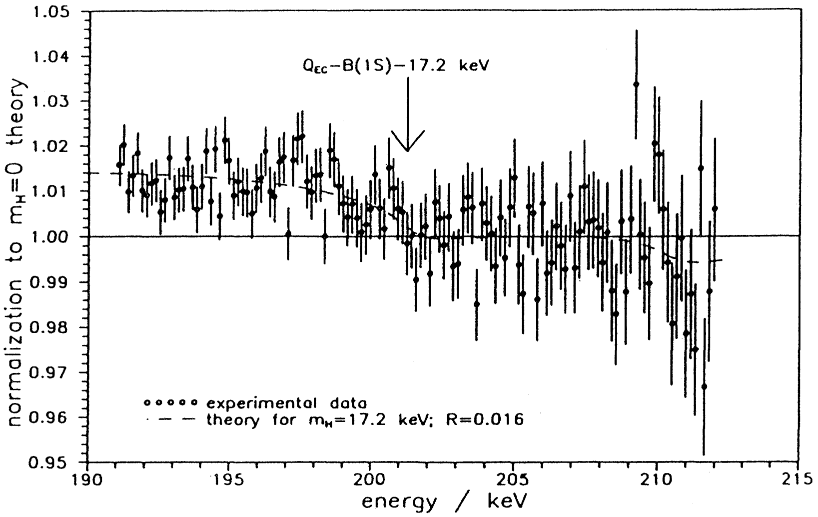

FIG. 18. Experimental corrected IB spectrum divided by the theoretical spectrum which assumes no heavy neutrino, normalized to the region above the expected kink. The dashed line represents the theory for a 17.2-keV neutrino with a probability of 1.6% (Zlimen *et al.*, 1991).

scan, 100–165 keV, and a narrow scan, 132–163 keV. In both cases the fit clearly favored no 17-keV neutrino, and they ruled out such a neutrino with a mixing probability of 0.8% (the value found by Hime), with a confidence level greater than 99%. They admitted, however, that their wide-scan fit for no 17-keV neutrino, while considerably better than that which included such a neutrino, had an excessive χ^2 (53 for 35 degrees of freedom, with a probability of approximately 0.05, which they regarded as unlikely). They attributed this to possible systematic errors. The fit for the narrow scan was considerably better (χ^2 of 13.8 for 20 degrees of freedom, with a probability of greater than 0.8).

III. THE DISAPPEARANCE

"This time it vanished quite slowly, beginning with the end of the tail, and ending with the grin, which remained some time after the rest of it had gone." The Cheshire Cat in Lewis Carroll's *Alice in Wonderland.*

A. The tide ebbs

The year 1991 was the high point in the life of the 17-keV neutrino. Although the evidence for its existence was far from conclusive, its existence had been buttressed by the recent results of Simpson and Hime, of Hime and Jelley, of the Berkeley group (Norman, Sur, and others), and by Zlimen and others. From this point on, however, the evidence would be almost exclusively against it. Not only would there be high-statistics, extremely persuasive negative results, but serious questions would also be raised about its strongest support.

The Europhysics conference on high energy physics held in July 1991 illustrates the uncertain status of the 17-keV neutrino. Simpson (1992) offered a summary of the evidence favoring its existence, whereas Morrison (1992a) offered a rather critical and negative review. Simpson summarized the recent positive evidence for the 17-keV neutrino, which, in the light of his previous criticism, he regarded as having more evidential weight than the negative results discussed earlier. (See Table II for the positive evidence cited by Simpson.)

Morrison's summary was rather negative.[36] He began with Koonin's (1991) soon to be published calculation on tritium. Recall that questions had been raised earlier concerning atomic physics effects in tritium decay (Haxton, 1985; Drukarev and Strikman, 1986; Eman and Tadic, 1986; Lindhard and Hansen, 1986; Weisnagel and Law, 1989). The calculated effects had all been rather smooth, and although one could argue about their size, there was a question as to whether such a smooth effect could account for a "kink" in the beta-decay spectrum. Koonin proposed the BEFS (beta environment fine structure), which gave rise to an oscillatory structure in the spectrum and depended on the embedding of the tritium in a crystal structure (Fig. 19). "At the 10^{-3} level of accuracy currently of interest, it will be important only for those sources in which tritium is embedded in a host material" (p. 469). This calculation seemed to cast doubt on the positive results obtained with tritium, all of which had used such a source. Morrison concluded, "The conclusion is that tritium, or another beta source with a low end-point, should not be used to look for heavy neutrinos because of uncertainty in the expected spectrum shape" (1992a, p. 600).[37]

Morrison also discussed Simpson's previous criticism of the negative searches. In particular, he examined Simpson's reanalysis of Ohi's data. "The question then is, How could the apparently negative evidence of Fig. [5] become the positive evidence of Fig. [10]? The explanation is given in Fig. [20], where a part of the spec-

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[36] A second summary appeared in Morrison (1992b).

[37] Bonvicini (1993) also considered tritium experiments to be too limited statistically.

TABLE II. Experimental evidence for a 17-keV neutrino (Simpson, 1992).

Iostope	ν Mass (keV)	Mixing angle θ^a	Reference
^{3}H [Si(Li)]	17.1±0.2	0.105±0.015	Hime and Simpson (1989), Simpson (1985)
^{3}H in Ge	16.9±0.1	0.105±0.015	Hime and Simpson (1989)
^{35}S	16.9±0.4	0.082±0.008	Hime and Simpson (1989)
	16.95±0.35	0.088±0.005	Simpson and Hime (1989)
^{14}C in Ge	17.0±0.5	0.114±0.015	Sur *et al.* (1991)
^{63}Ni	16.75±0.38	0.101±0.011	Hime, Oxford report (OUNP-91-20).

[a] The mixing probability is essentially the square of the mixing angle.

trum near 150 keV is enlarged. Dr. Simpson only considered the region 150±4 keV (or more exactly +4.1 and −4.9 keV). The procedure was to fit a straight line, shown solid, through the points in the 4-keV interval above 150 keV, and then to make this the baseline by rotating it down through about 20° to make it horizontal. This had the effect of making the points in the interval 4 keV below 150 keV appear above the extrapolated dotted line. This, however, creates some problems, as it appears that a small statistical fluctuation between 151 and 154 keV is being used: the neighboring points between 154 and 167, and below 145 keV, are being neglected although they are many standard deviations away from the fitted line. Furthermore, it is important, when analyzing any data, to make sure that the fitted curve passes through the end-point of about 167 keV, which it clearly does not" (p. 600).[38]

Morrison also noted that the shape-correction factors needed in magnetic spectrometer experiments were smooth and unlikely to obscure a kink due to a heavy neutrino, and remarked that there were problems due to backscattering of electrons in the positive experiments of Simpson and Hime and of Hime and Jelley. In looking at the experimental situation, Morrison cited the most recent positive results along with new negative results on ^{35}S from Caltech (discussed below), on ^{177}Lu from Grenoble, and the tritium result of Bahran and Kalbfleisch. His summary of results is given in Table III.

The results of Bahran and Kalbfleisch (1992a) were presented at this conference.[39] Their experiment investigated the spectrum of tritium using a gas proportional chamber.[40] Their results are shown in Fig. 21, along with those of Simpson (1985) and of Hime and Simpson (1989). No excess of events is seen and their 99% confidence level upper limit was 0.4% for 17-keV neutrino mixing.[41]

In the published version of his paper, Simpson responded to Morrison's view that the early negative results should be taken seriously in evaluating the evidence.[42] He noted that the magnetic spectrometer experiments all needed a shape-correction factor that was of the order of several percent in the energy region of interest. "It is therefore difficult to see how these experiments can rule out a 1% effect, which requires an accuracy of perhaps 0.2% over the analyzed region" (Simpson, 1992, p. 598).[43]

Simpson's view of the early negative magnetic spectrometer results was strongly supported by Bonvicini's work [published first as a 1992 CERN report (CERN-PPE/92-54) and later as Bonvicini (1993)].[44] In this work Bonvicini discussed the question of whether a kink in the energy spectrum due to an admixture of a 17-keV neutrino could be masked by the presence of unknown distortions, such as the shape-correction factors used in magnetic spectrometer experiments. "Most urgent in this discussion is why experiments where the β^- energy is measured calorimetrically tend to see the effect, and those which use spectrometers do not. My analysis . . . shows that *large continuous distortions in the spectrum*

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[38] The effect seen by Simpson was quite sensitive to the energy interval chosen. In general, an experimental result should be robust against such changes. Recall also the earlier comments of Hetherington and others concerning the danger of mistaking a statistical fluctuation for a physical effect.

[39] These results were published in Bahran and Kalbfleisch (1992b).

[40] Using a gas source avoids problems associated with embedding the tritium in a crystal, but still requires atomic physics corrections.

[41] Bahran and Kalbfleisch note that a 1% anomaly had been seen in the tritium spectrum at approximately 1 keV, the kink energy for a 17-keV neutrino, in 1959 (Conway and Johnston, 1959). This effect was attributed to a possible nonlinearity in the energy response of their proportional chamber at low energies. They also noted that the experimental result of Hime and Jelley did not include the effect of radiative corrections to the spectrum. The results Simpson included in the published version of his talk included such corrections. The issue of tritium results is still unresolved.

[42] One of the difficulties of using papers in published conference reports is that they contain modifications made well after the conference. Thus Simpson could respond to what Morrison had said. As seen below, Morrison responded to Simpson's criticism at a subsequent conference.

[43] Recall, however, that Ohi's experiment did not use such a device, but rather a solid-state detector.

[44] The major difference between the CERN report and the published paper is a detailed discussion of the negative Tokyo experiment (Ohshima *et al.*, 1993). This experiment is discussed in detail. Quotations are from the 1993 published paper.

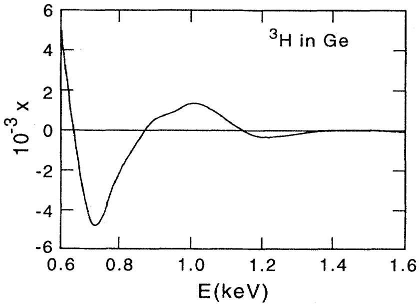

FIG. 19. Beta environmental fine structure (BEFS) for tritium in a germanium crystal at a temperature of 80 K (Koonin, 1991).

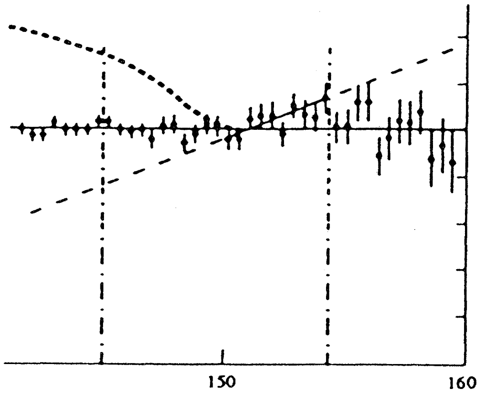

FIG. 20. Morrison's reanalysis of Simpson's reanalysis of Ohi's result (Morrison, 1992a).

can indeed mask or fake a discontinuous kink [emphasis added]. In the process I point to some deep inconsistencies in all the spectrometer experiments considered here" (Bonvicini, 1993, p. 97). He performed a detailed analysis and Monte Carlo simulation of what were then generally regarded as best experiments on either side of the 17-keV neutrino issue: the positive result from ^{35}S by Hime and Jelley (1991) and the negative result from ^{63}Ni by Hetherington and others (1987). He also analyzed several other experiments.[45]

Bonvicini concluded that the positive Hime and Jelley result was statistically sound. He cautioned, however, that the electron response function in this experiment had been only partially measured, and that this might be a possible problem.[46] As the subsequent history shows, this statement was prescient. Bonvicini's analysis of the experiment of Hetherington *et al.* concluded that, although their use of a 2.5% shape-correction factor was certainly acceptable when searching for a 3% kink, when one looked for a 0.8% kink more work was needed. His summary of the overall situation was as follows. "A look at the published data seems to indicate that the statistical criteria listed above would eliminate all the negative experiments considered here, but it is left to the authors to look at their data" (p. 114). As far as the positive experiments were concerned, he rejected the Hime-Jelley result on ^{63}Ni on the grounds of poor statistics and large correlation between kink and distortions. The Berkeley result had too large (6%) a shape-correction factor. "The positive experiments do not offer definitive proof of a total ERF [electron response function] measurement, but having found the kink in the same apparatus with two different nuclei [the Hime-Jelley results on ^{35}S and ^{63}Ni] seems to eliminate the possibility of one common missing distortion as the source of the kink" (p. 114). Bonvicini continued, "The ^{35}S result of Hime and Jelley is statistically sound, as they have run the checks suggested in this paper, while the earlier Simpson and Hime result[s] have not been analyzed. Thus there is only one experiment at this time and in my knowledge where one could say that a kink is certainly there" (p. 116).[47]

Bonvicini disagreed quite strongly with Morrison's very negative view of the situation. "The conclusions of this review differ from those of [a reference to a CERN report by Morrison]. I do not share the same enthusiasm for anything sporting a decent χ^2, or the same belief in infinitely small systematic errors. Conclusions based on 'experiment counting' (one counts experiments and the majority wins) is most definitely not the way to assess this controversy" (p. 114).

Bonvicini also suggested the need for more good experiments and gave criteria for what would constitute such a good experiment. These included (1) direct measurement of the electron response function at more than one point across the fitted spectrum; (2) cross checks of the fitted correlation coefficients; (3) use of at most one small linear shape factor; (4) no narrow energy scans—the scan should include a range at least twice the neutrino mass; (5) results of a fit shown with a shape factor with one order more.

Bonvicini's work argued quite strongly that the negative results of the previous magnetic spectrometer experiments were inconclusive, and he suggested the design of experiments that either used no shape-correction factor

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[45] Bonvicini ignored experiments on tritium on the grounds that the Coulomb correction factor in such experiments is quite large for low-energy electrons (where the kink due to the 17-keV neutrino would be seen) and difficult to calculate precisely. He also suggested for future work that experiments on tritium be avoided.

[46] The electron response function was measured at a single energy. Bonvicini suggested that it should be measured at several energies spanning the fitted energy spectrum.

[47] This last point appears only in the published paper (1993). The 1992 CERN report contains only the previous statement about the results with two different nuclei.

TABLE III. Summary of experimental results (Morrison, 1992a).

Laboratory	First author	Year	Source	Technique (keV)	Mass	% Mixing (C.L. %)
Guelph	Simpson	1989	^{35}S	Si(Li)	16.9±0.4	0.73±0.11
Oxford	Hime	1991	^{35}S	Si(Li)	17.0±0.4	0.84±0.08
Berkeley	Sur	1991	^{14}C	Ge	17±2	1.40±0.5
Zagreb	Zlimen	1990	^{71}Ge	Int. brems.	17.2	1.6(2*sd*)
Tokyo	Ohi	1985	^{35}S	Si(Li)	No evid.	<0.3(90%)
Princeton	Altzitzoglou	1985	^{35}S	Mag. sp.	No evid.	<0.4(99%)
ITEP	Apalikov	1985	^{35}S	Mag. sp.	No evid.	<0.17(90%)
BARC/TIFR	Datar	1985	^{35}S	Mag. sp.	No evid.	<0.6(90%)
Caltech	Markey	1985	^{35}S	Mag. sp.	No evid.	<0.25(90%)
Caltech	Becker	1991	^{35}S	Mag. sp.	No evid.	<0.6(90%)
Chalk River	Hetherington	1986	^{63}Ni	Mag. sp.	No evid.	<0.3(90%)
Caltech	Wark	1986	^{63}Ni	Mag. sp.	No evid.	<0.25(90%)
CERN-ISOLDE	Borge	1986	^{125}I	Int. brems.	No evid.	<0.9(90%)
ILL Grenoble	Schreckenbach	1991	^{177}Lu	Mag. sp.	No evid.	<0.2(90%)
Guelph	Simpson	1985	^{3}H	Si(Li)	~17.1	About 3
Guelph	Hime	1989	^{3}H	Ge	16.9±0.1	0.6–1.6
Oklahoma	Bahran	1992a	^{3}H	Prop. ctr.	No evid.	<0.4(90%)

or had such overwhelming statistical accuracy that a kink would always be visible. As we shall see, experiments of this type were, in fact, performed and were decisive in answering the question as to whether the 17-keV neutrino existed.[48]

The new Caltech result to which Morrison had referred appeared in Radcliffe *et al.* (1992). This experiment also looked at the ^{35}S spectrum with a magnetic spectrometer. Radcliffe and co-workers took data in two different runs: a wide energy range, 130–167 keV; and a narrow scan of 10 keV around the kink expected at 150 keV for the 17-keV neutrino. Both runs were consistent with no heavy neutrino and excluded a 17-keV neutrino with a 0.85% mixing probability at the 99.3% confidence level and the 99.9% confidence level for the wide- and narrow-scan runs, respectively. Their result for the narrow-scan run is shown in Fig. 22. No kink is seen.

An interesting feature of this experiment was their simulation of a kink in the spectrum. All of the previous searches for a heavy neutrino with magnetic spectrometers had been negative, and a question had been raised as to whether this type of apparatus was, in fact, capable of detecting such a kink. The experimenters shielded 10% of their detector with a 17-micron aluminum foil. The electrons would lose energy in passing through the foil, and they expected this energy loss to produce a kink in the spectrum that would simulate a heavy neutrino with a 1% admixture. Their results with the foil in place are shown in Fig. 23. A kink, clearly visible, gave a best fit for a mass of 15.6 keV with a mixing factor of 2.5%, thus demonstrating that a magnetic spectrometer experiment was sensitive enough to detect a 17-keV neutrino, at least at that level. One might legitimately wonder (see Fig. 23) whether the apparatus was sensitive enough to detect a heavy neutrino with 1% mixing. The shape of the spectrum distortion reduced was also different from that expected for a heavy neutrino.

Further argument against the existence of the 17-keV neutrino was provided by the theoretical reanalysis of the data of Hime and Jelley (1991) by Piilonen and Abashian (1992). They used the published data[49] and constructed the relative deviations (DATA-FIT)/FIT, shown in Fig. 24. "While the combined data [*c*] is certainly consistent with the 17 keV hypothesis, there is simply not enough statistical precision near 150 keV to see an unmistakable kink" (p. 226). Piilonen and Abashian examined various alternative explanations for the results of Hime and Jelly. These included ambient background, unrejected pileup, the theoretical spectrum, and radiative corrections. None of these explained the measurements. They also performed a detailed simulation of the experimental apparatus to try to get "an accurate determination of all the contributions to the electron response function as well as their dependence on energy" (p. 229). Their simulation of electron scattering and energy loss was more complex than that used by Hime and Jelley. Their result is shown in Fig. 25 and was still in disagreement with a massless-neutrino beta spectrum. They concluded, "We agree with Hime and Jelley that there is a serious distortion in their ^{35}S data, though we cannot pinpoint any definite cause for it. We believe that if the original data is reanalyzed by Hime and Jelley with a more realistic electron response function such as we have derived in our simulation, then the consistency of this distortion with a

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[48] I am grateful to an anonymous referee for emphasizing the importance of Bonvicini's work.

[49] They did not have access to the raw data.

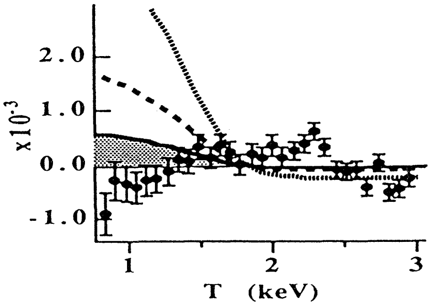

FIG. 21. $\Delta K/K$ with upper limit of 0.4% compared with the positive claims of Simpson (1985; 2.7%, dotted) and of Hime and Simpson (1989; 1.1%, dashed). From Bahran and Kalbfleisch (1991).

two-component neutrino hypothesis (with $m_2 = 17$ keV) will disappear" (p. 233).

The most detailed summary of the evidence, and a moderate position, was provided by Hime in early 1992 (Hime, 1992).[50] Although Hime was an active participant in the controversy, and one of those who provided persuasive evidence in favor of the 17-keV neutrino, his summary seems quite fair and judicious. He provided a reasonably complete history of the experiments and their results and devoted considerable attention to possible experimental problems or difficulties.

He first considered the issue of the atomic physics corrections to the tritium results and noted that taking account of the criticism reduced the size of Simpson's original result from 3% to ~1%. He observed that part of the difficulty with these calculations was that the experiments did not use free tritium, but rather tritium bound in a crystal lattice. "The main point emerging from the analysis is that the *sudden* excess of counts in the tritium spectrum cannot be explained via atomic physics alone, unless effects are present that are yet to be contemplated" (Hime, 1992, p. 1303).

Hime also discussed Simpson's reanalysis of the early negative results in ^{35}S and remarked that they were based on a reanalysis of the data over only a narrow band of energy. "The difficulty remains, however, that an analysis using such a narrow region could mistake statistical fluctuations as a physical effect. The claim of positive effects in these cases [by Simpson] should be taken lightly without a more rigorous treatment of the data" (p. 1303).[51]

Hime also examined the issue of the uniformly negative results provided by magnetic spectrometer experiments. He noted that such experiments eliminated the problem of backscattering and energy loss that appeared in experiments using external sources and solid-state detectors. He also stated that whereas magnetic spectrometer experiments still required an extra shape-correction factor, "it is a point for debate whether or not sensitivity to a heavy neutrino is preserved. It is clear that the addition of extra degrees of freedom will reduce the sensitivity of the data, but it remains difficult to see how a smooth correction would completely remove a 'kink'" (p. 1309). Hime observed that "given the obvious disagreement between magnetic spectrometer searches on the one hand and the positive results with solid state detectors on the other, it is now generally agreed that insight into the discrepancy could be made if the sensitivity of a magnetic spectrometer to uncover a heavy neutrino signal could be experimentally demonstrated. Proposals include measurements with a mixed source (such as 99% ^{35}S + 1% ^{14}C), or artificially invoking energy loss in part of the spectrum at some predetermined level. This latter approach was suggested by the Caltech group [see earlier discussion of Radcliffe *et al.* (1992)] and has been implemented in their program" (p. 1310).

Hime also critically examined the positive evidence for the 17-keV neutrino, some of which he had himself provided. He argued that his Oxford results on ^{35}S and on ^{63}Ni had improved on the original Guelph results of Simpson and of Simpson and Hime by changing the geometry of the experiment from a diffuse to a collimated source. He also argued that the dominant systematic uncertainty in these experiments, that due to uncertainty in the backscattering component of the electron response function, had been adequately checked. "It seems that an alternative description of the Oxford data requires an effect that does not show up in direct measurements of the detector response to monoenergetic electrons" (p. 1305). He also noted that there were possible background and veto problems with the positive result on ^{14}C reported by the Berkeley group, and that an analysis of "unvetoed" data reduced their mixing probability for a heavy neutrino from 1.2 to 0.75 %. He also questioned the TANDAR result. "Even more recently, an experiment at the TANDAR facility in Argentina measured the ^{71}Ge spectrum, yielding equally confusing values for a heavy neutrino mass. In particular, their data yield a best fit with $M_2 = 13.8 \pm 1.8$ keV and $\sin^2\theta = 0.80 \pm 0.25\%$. Fixing $M_2 = 17$ keV does not provide a better fit to the data than does a massless neutrino spectrum. There are, however, some unsettling aspects to this experiment. In the first place, in fitting the ^{71}Ge spectrum to a two-component neutrino, the χ^2 distribution for the heavy neutrino mass exhibits two relative minima (one at about 9 keV and another, slightly deeper, at 13.8 keV). This is not expected for the emission of a single heavy neutrino and is inconsistent with other measurements finding evidence for a 17 keV neutrino. This suggests that, either the heavy neutrino hypothesis is not compatible with the data or that systematic effects are present which have not been properly accounted for" (pp. 1307–1308).

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[50] Although the article was not published until 20 May 1992, it was received at *Modern Physics Letters* on 13 March 1992.

[51] Hetherington *et al.* (1987), Bonvicini, and Morrison also agreed on this point.

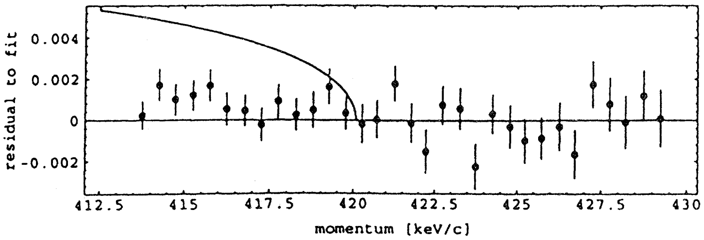

FIG. 22. Data for run *B*, a high-statistics study of the beta spectrum near 420 keV/*c*, normalized above the kink. The curve shows the expected spectrum with a 0.85% admixture of a 17-keV neutrino (Radcliffe *et al.*, 1992).

Hime's summary of the situation seems quite reasonable (see Table I).

> The evidence accumulated both for and against the existence of a 17-keV neutrino presents an unresolved conundrum. On the positive side, a diverse range of isotopes have been studied in many experimental environments, all of which yield self-consistent results. While a working alternative for explaining these results has not been realized, potential hazards have not necessarily been exhausted.
>
> It remains an unsettled debate whether or not the null results obtained with magnetic spectrometers are weakened by unresolved systematic effects. In particular, a thorough analysis of the effects of polynomial shape corrections is desired. Results do exist, however, where systematic uncertainties associated with shape corrections have been properly analyzed, making it difficult to see how a 17 keV neutrino would not be revealed if it does indeed exist. A demonstration of the sensitivity to uncover a heavy neutrino signal in such an experiment could provide insight into this puzzle and potential schemes were outlined above. A useful alternative would be to determine the spectrometer response *a priori* via "complementary experiments."
>
> In the meantime a host of experimental efforts continue with the hope to elucidate the issue. All experiments share the difficult task of determining the shape of a continuous energy spectrum. Furthermore, the accuracy required is at the level of a few tenths of a percent. It is clear that the difficulties associated with low energy β decay measurements are predominantly of a systematic origin and that a resolution of the "17 keV conundrum" will require a careful and critical analysis of both positive and negative results. (pp. 1312–1313).

Thus there were four differing summaries of the situation: one positive, by Simpson; one negative, by Morrison; and two neutral, by Hime and by Bonvicini. The situation seemed unresolved. The experimental efforts under way, combined with the critical and careful analysis for which Hime had hoped, would decide the issue.

B. The kink is dead

Support for the existence of the 17-keV neutrino began to erode shortly after the publication of Hime's review. In early August 1992, the Berkeley group presented a conference report that included a statistically improved result from ^{14}C of $M_2=17\pm1$ keV and a mixing probability of $(1.26\pm0.25)\%$ (Norman *et al.*, 1993). They also reported "a high statistics measurement of the inner bremsstrahlung spectrum of ^{55}Fe and find no indication of the emission of a 17-keV neutrino" (p. 1123).[52] The analysis method used in the ^{55}Fe experiment was to examine the second derivative of the beta-decay spectrum for a kink. "In the present ^{55}Fe experiment, we have sufficiently high statistics that a true 'local' analysis could be performed [over a narrow energy region].[53] It is well known that taking the second derivative of a spectrum can sometimes reveal small peaks that might otherwise be missed. We have found that the second derivative technique is also a powerful way to reveal the distortion in a spectrum produced by the emission of a massive neutrino. . . . The second derivative of the resulting spectrum is shown in Figure [26]. There is clearly no hint of a structure near 208 keV [the energy at which a kink due to a 17-keV neutrino would be expected]. Thus our ^{55}Fe experiment shows no evidence for the emission of a 17 keV neutrino" (p. 1125). They cited three other recent negative results[54]: two with magnetic spectrometers on ^{35}S and ^{63}Ni, and one on ^{35}S that used a solid-state silicon detector.[55] These results, along with their own, supported their view: "We thus conclude that, whatever causes the 'kink' in our ^{14}C spectrum, it is not a neutrino" (p. 1126).

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[52] The Berkeley group had earlier reported a result of $M_2=21\pm2$ keV with a mixing probability of $(0.85\pm0.45)\%$ for ^{55}Fe.

[53] High statistics avoids the problem of statistical fluctuations affecting the results. Recall Morrison's earlier discussion of Simpson's reanalysis of Ohi's data.

[54] Two of these results were also presented at the conference, and one had been communicated to the Berkeley group by preprint.

[55] Two of these experiments, those of Ohshima *et al.* (1993) and of Mortara *et al.* (1993), will be discussed in detail. The Caltech result, a preprint, has not appeared in the published literature.

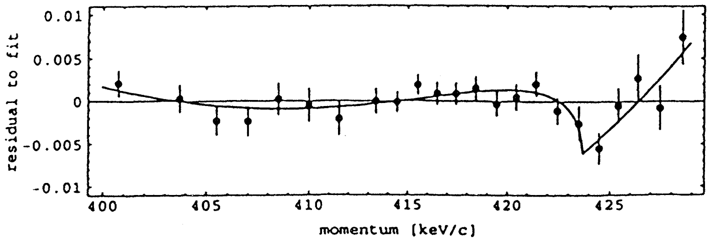

FIG. 23. Synthetic kink induced in the beta spectrum of ^{35}S by a 17-μm aluminum foil. The solid curve is the spectrum expected with a 2.5% admixture of a 15.6-keV neutrino (Radcliffe *et al.*, 1992).

The magnetic spectrometer experiment on ^{63}Ni by the Tokyo group was also presented at the conference (Ohshima, 1993). There was also an earlier published result of the same experiment (Kawakami *et al.*, 1992).[56] The experimenters noted some of the problems of experiments that used wide energy regions and commented that "we have concentrated on performing a measurement of high statistical accuracy, in a narrow energy region, using very fine energy steps. Such a restricted energy scan. . . also reduced the degree of energy-dependent corrections and other related systematic uncertainties" (Kawakami *et al.*, 1992, p. 45). The data were taken over three overlapping energy ranges: 41.2–46.3, 45.7–51.1, and 50.5–56.2 keV (the threshold for a 17-keV neutrino occurs at approximately 50 keV). The results of their experiment are shown in Fig. 27, for (a) the mixing probability allowed to be a free parameter, and (b) with the probability fixed at 1%. The effect expected for a 17-keV neutrino with a 1% mixing probability is also shown in (a). No effect is seen. Their best value for the mixing probability of a 17-keV neutrino was [-0.011 ± 0.033 (statistical) ±0.030 (systematic)]%, with an upper limit for the mixing probability of 0.073% at the 95% confidence level. This was the most stringent limit yet. "The result clearly excludes neutrinos with $|U|^2\geq 0.1\%$ for the mass range 11 to 24 keV" (Ohshima, 1993, p. 1128).[57]

Although the experiment's narrow energy range was designed to minimize the dependence of the result on the shape correction, the experimenters also checked on the sensitivity of their result to that correction. They normalized their data in the three energy regions using the counts in the overlapping regions, and divided their data into two parts: (A) below 50 keV, which would be sensitive to the presence of a 17-keV neutrino, and (B) above 50 keV, which would not. They then fit their data in (B) and extrapolated the fit to region (A). The resulting fit was far better than one that included a 1% mixture of the 17-keV neutrino, demonstrating that the shape correction was not masking a possible effect of a heavy neutrino. Bonvicini noted that this experiment, with its very high statistics, had answered essentially all of his criticism of spectrometer experiments convincingly. "Thus, I conclude that this experiment could not possibly have missed the kink and obtain[ed] a good χ^2 at the same time, in the case of an unlucky misfit of the shape factor" (Bonvicini, 1993, p. 115).

The 17-keV neutrino received another severe blow when Hime, following the suggestion of Piilonen and Abashian, extended his calculation of the electron response function of his detector to include electron-scattering effects and found that he could fit the positive results of Hime and Jelley without the need for a 17-keV neutrino (Hime, 1993). This seemed to remove one of the most persuasive pieces of evidence for the heavy neutrino. "It will be shown that scattering effects are sufficient to describe the Oxford β-decay measurements and that the model can be verified using existing calibration data. Surprisingly, the β spectra are very sensitive to the small corrections considered. Consequently, any reinterpreta-

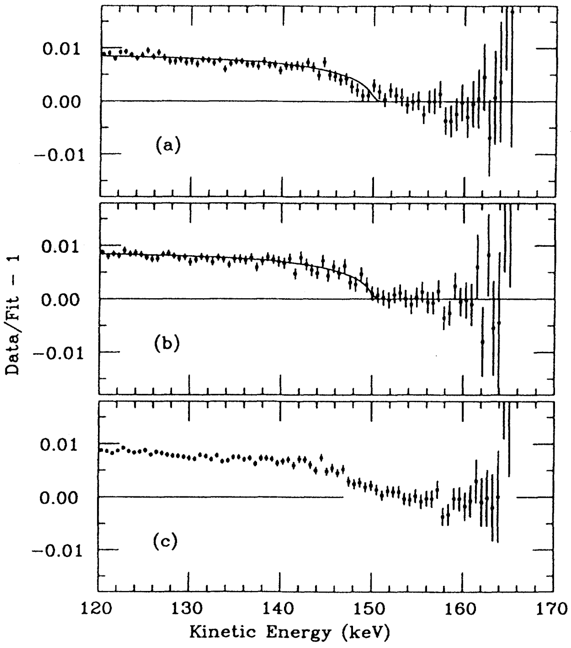

FIG. 24. Relative residual for the data of Hime and Jelley (1991) for (a) run 1, (b) run 2, and (c) combined. The solid curves are for a 17-keV neutrino with 0.9% mixing probability (Piilonen and Abashian, 1992).

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[56] The published paper appeared in early August 1992, but had been received at the journal on 16 April 1992. The conference paper, presented in early August 1992, set a more stringent limit on the presence of the 17-keV neutrino. A more detailed account of the experiment appeared in Ohshima *et al.* (1993).

[57] The published value in Kawakami *et al.* (1992) was ($0.018\pm0.033\pm0.033$)%, with an upper limit of 0.095%. $|U|^2$ is the mixing probability.

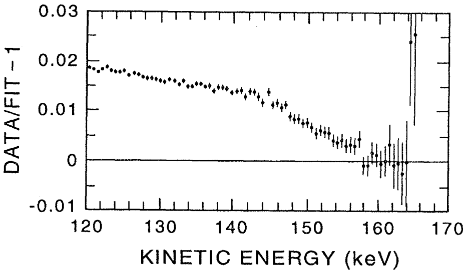

FIG. 25. Relative residuals for the data of Hime and Jelley compared to a massless-neutrino spectrum calculated with the response function of Piiloneen and Abashian (1992).

tion of the data is reliable only if the scattering amplitudes can be computed or measured accurately, and *independent* of the β-decay measurements" (p. 166).

Hime briefly reviewed the evidence, noting that the major evidence against the existence of the 17-keV neutrino came from magnetic spectrometer experiments in which questions had been raised concerning the shape corrections. He commented that Bonvicini (in a CERN report, discussed earlier) had shown that nonlinear distortions could mask the presence of a heavy-neutrino signature and still be described by a smooth shape correction. He remarked, however, that "a measurement of the ^{63}Ni spectrum (Kawakami *et al.*, 1992) has circumvented this difficulty. The sufficiently narrow energy interval studied, and the very high statistics accumulated in the region of interest, makes it very unlikely that a 17-keV threshold has been missed in this experiment" (Hime, 1993, p. 165). He also cited a new result from a group at Argonne National Laboratory (Mortara *et al.*, 1993, discussed in detail below) that provided "convincing evidence against a 17-keV neutrino." In particular, the Argonne group had demonstrated the sensitivity of their magnetic spectrometer experiment to a possible 17-keV neutrino by admixing a small component of ^{14}C in their ^{35}S source and detecting the resulting kink in their composite spectrum. These negative results provided the impetus for Hime's reexamination of his result.

Hime's new Monte Carlo study included the effect of "electrons which enter the detector after scattering from the aluminum baffle (see Fig. 15), electrons which penetrate the edges of the apertures, and electrons which back-diffuse from the source substrate" (p. 167).[58] These scattering effects had not been included in the original analysis. The dominant effect in the experiment was the scattering from the aluminum baffle, which resulted in 1.2–1.4 % of the electrons detected originating from scattering in the baffle, and which exhibited a peak in their energy distribution. The Oxford data were reanalyzed with these additional effects included.

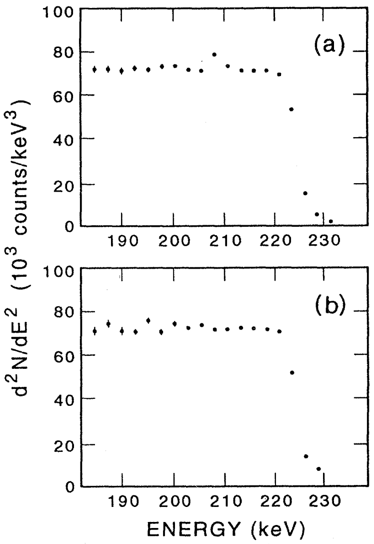

FIG. 26. Second derivative of the inner bremsstrahlung spectrum of ^{55}Fe: (a) Monte Carlo data generated with a 1% admixture of a 17-keV neutrino; (b) experimental data after subtracting ^{59}Fe. From Norman *et al.* (1993).

> The results of this analysis are listed in Table [IV], where comparison is made between the present model and that of a 17-keV neutrino in the absence of intermediate scattering corrections. The inclusion of intermediate scattering effects describes the spectral distortions surprisingly well. Furthermore, the fraction of electrons in the additional LET [low-energy tail] component which has been fitted by the data is in very good agreement with that expected from the calculations presented above. The sensitivity of the data to the various effects also agrees with expectations. In the case of ^{35}S, for example, the data are only weakly sensitive to the effects of aperture penetration or to the presence of the source substrate. On the other hand, a fit to the ^{63}Ni data with only the aluminum baffle included yields a result that is inconsistent with both the ^{35}S data and calculations. The agreement is significantly improved, however, after accounting for electron back-diffusion from the source substrate, and the effects of aperture penetration are marginal.
>
> Residuals are presented in Fig. [28] in the form of shape factors derived from optimum fits to the data after including intermediate scattering effects and assuming a single-component massless neutrino. The ^{35}S shape factors (Figs. [28(a)] and [28(b)]) hint at spectral distortion beyond ~150 keV. While the intermediate scattering contributions cannot produce a "kink" per se, the chi-squared analysis (Table [IV]) indicates that any difference between the two models considered [with and without a 17-keV neutrino] cannot be distinguished by the statistics of the data. (Hime, 1993, p. 169)

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[58] The Monte Carlo calculation included the best data then available and, as we shall see, was checked against an experimental result, independent of the beta-decay experiment. Pickering (1984) has argued that one can always question such Monte Carlo results. I shall discuss this point in detail later.

TABLE IV. Reanalysis of Oxford data (Hime, 1993).

Experiment	f_{baffle} (%)	$(\sin^2\theta_{17})\times 10^2$	Q (keV)	χ^2/ν
^{35}S run #1	0	0	167.0169(33)	135.8/76
	0	0.752(100)	167.0626(47)	78.9/75
	1.544(175)[a]	0	167.0614(50)	75.2/75
	1.403(175)[b]	0	167.0655(50)	74.6/75
	1.335(175)[c]	0	167.0674(50)	74.4/75
^{35}S run #2	0	0	167.0194(37)	127.3/76
	0	0.833(107)	167.0686(53)	68.6/75
	1.260(190)[a]	0	167.0549(56)	74.9/75
	1.187(190)[b]	0	167.0592(56)	74.7/75
	1.136(190)[c]	0	167.0601(56)	74.6/75
^{35}S data combined	0	0	167.0182(24)	195.9/76
	0	0.816(75)	167.0679(34)	76.9/75
	1.414(131)[a]	0	167.0595(37)	78.3/75
	1.285(131)[b]	0	167.0622(37)	77.4/75
	1.224(131)[c]	0	167.0641(37)	77.2/75
^{63}Ni	0	0	66.8218(39)	173.8/72
	0	1.018(99)	66.8654(56)	69.4/71
	2.730(265)[a]	0	66.8863(62)	71.2/71
	2.698(265)[b]	0	66.8816(62)	71.0/71
	1.676(265)[c]	0	66.8792(62)	68.9/72

[a] Antiscatter baffle (varied).

[b] Antiscatter baffle (varied) + apertures.

[c] Antiscatter baffle (varied) + apertures + source backing.

Still, there remained a possibility that the new calculation was incorrect. Hime was able to independently confirm his model by measuring the electron response function using monoenergetic internal-conversion electron sources occupying the same geometry as the beta-decay sources used in the original experiments. The comparison between the measurements and the calculation is shown in Fig. 29. "The solid curve drawn through these residuals is taken directly from the calculations presented above, including the effects of baffle-scattering, aperture penetration, and back-diffusion from the source substrate. The data reveal a structure that agrees well with the model, both in overall shape and intensity" (p. 170).[59]

Hime concluded, "The distortions observed in the ^{35}S and ^{63}Ni experiments at Oxford are significantly suppressed when account is made for intermediate scattering effects that were overlooked in the original analysis. Indeed, the heavy neutrino hypothesis can be replaced with that based on scattering effects. Essentially, there is a 100% correlation between f_{int}, the probability for intermediate scattering, and $\sin^2\theta_{17}$, the mixing probability for the 17-keV neutrino. Hence, without independent knowledge of the effects considered, it would be impossible to rule out a 17-keV neutrino based solely on fitting the β spectra. Nonetheless, the presence of intermediate scattering effects has been uncovered in a more detailed analysis of IC [internal conversion] electron spectra (p. 171). . . . When regard is made for intermediate scattering effects, an upper limit (90% CL) of 0.35% and 0.53% on the mixing probability for a 17-keV neutrino is obtained, using the ^{35}S and ^{63}Ni data respectively" (p. 172). He also suggested that, despite the very different geometries, intermediate scattering effects might explain the original Guelph results. Such effects could not, however, explain those results obtained with a source embedded in the detector, such as Simpson's original result and the Berkeley result on ^{14}C.

Further evidence against the 17-keV neutrino was provided by the Argonne group headed by Freedman (Mortara *et al.*, 1993). The experiment used a solid-state Si(Li) detector, the same type used by Hime, an external ^{35}S source, and a solenoidal magnetic field to focus the decay electrons. The apparatus had a 2π sr solid angle (50% efficiency), which allowed a thin source that reduced scattering in the source and still allowed a high counting rate. The solenoidal field was shaped so that the angle between the electron velocity and the solenoid axis decreased as the electron moved toward the detector. This helped to reduce backscattering from the detector, which is larger at glancing angles. In addition, the field shape reflected some of the backscattered electrons to the detector by the magnetic mirror effect. The backscatter-

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[59] Hime attributed the small peak at the high-energy end to electron ionization of the silicon K shell with the subsequent escape of silicon K x rays. It casts no doubt on the confirmation of the model.

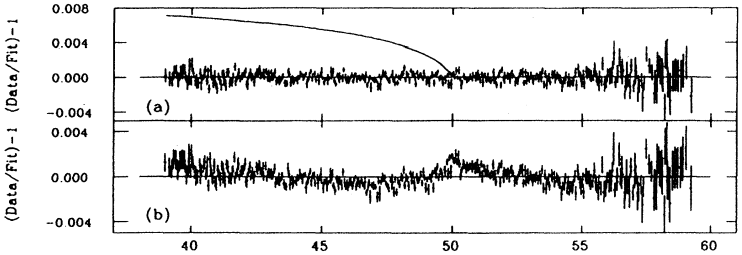

FIG. 27. Deviations from the best global fit with $|U|^2$ free (a) and fixed to 1% (b). The curve in (a) indicates the size of a 1% mixing effect of the 17-keV neutrino. From Ohshima (1993).

ing was reduced to less than 7% of the incident intensity, and a Monte Carlo simulation indicated that the fraction in the backscattering tail was nearly independent of energy. The apparatus also required no collimator. Furthermore, the electron response function was measured at several points in the fitted spectrum. "The present experiment requires that we know the electron response function between 120 and 167 keV. Measurements of the conversion lines of ^{139}Ce at 127, 160, and 167 keV are the principal constraint on the model of the electron response function" (p. 395). Previous ^{35}S experiments had used an electron response function extrapolated from the lower energy ^{57}Co lines. Finally, and perhaps most importantly, this experiment required no arbitrary shape-correction factor.

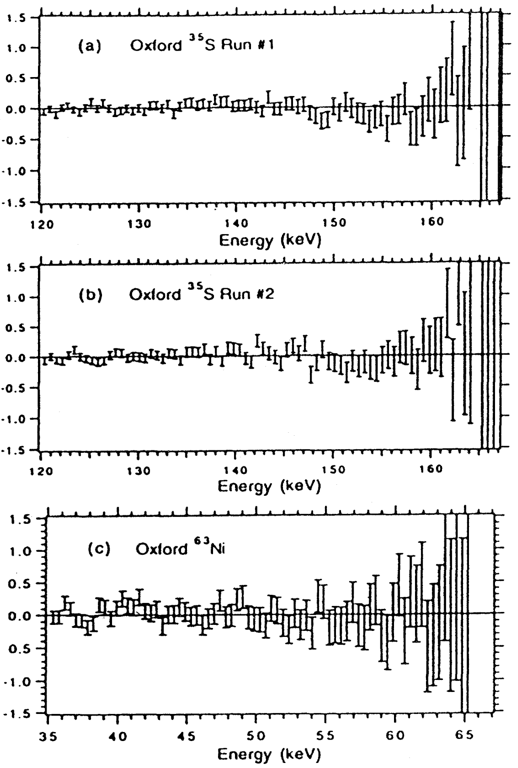

FIG. 28. Shape factors extracted from Oxford data for (a) ^{35}S run #1, (b) ^{35}S run #2, and (c) ^{63}Ni, after implementing the best-fit theoretical spectrum including intermediate scattering effects and assuming a single-component, massless neutrino. From Hime (1993).

The experimenters also demonstrated the sensitivity of their apparatus to a possible 17-keV neutrino.

> To assess the reliability of our procedure, we introduced a known distortion into the ^{35}S beta spectrum and attempted to detect it. A drop of ^{14}C-doped saline ($E_o - m_e \sim 156$ keV) was deposited on a carbon foil and a much stronger ^{35}S source was deposited over it. The data from the composite source were fitted using the ^{35}S theory, ignoring the ^{14}C contaminant. The residuals are shown in Figure [30]. The distribution is not flat; the solid curve shows the expected deviations from the single component spectrum with the measured amount of ^{14}C. The fraction of decays from ^{14}C determined from the fit to the beta spectrum is $(1.4\pm 0.1)\%$. This agrees with the value of 1.34% inferred from measuring the total decay rate of the ^{14}C alone while the source was being prepared. This exercise demonstrates that our method is sensitive to a distortion at the level of the positive experiments. Indeed, the smoother distortion with the composite source is more difficult to detect than the discontinuity expected from the massive neutrino. (Mortara *et al.*, 1993, p. 396)

Their final result, shown in Fig. 31, was $\sin^2\theta = -0.0004\pm 0.0008$ (statistical) ± 0.0008 (systematic) for the mixing probability of the 17-keV neutrino. "In conclusion, we have performed a solid-state counter

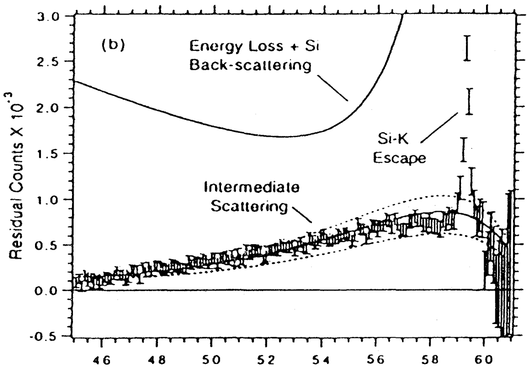

FIG. 29. ^{109}Cd spectrum accumulated in Oxford geometry. Residuals extracted from the 61-keV K = IC tail when intermediate scattering effects are neglected. The solid curve shows the effect calculated for intermediate scattering. From Hime (1993).

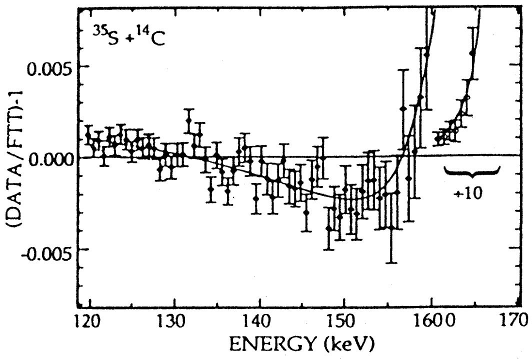

FIG. 30. Residuals from fitting the beta spectrum of a mixed source of ^{14}C and ^{35}S with a pure ^{35}S shape; the reduced χ^2 of the data is 3.59. The solid curve indicates residuals expected from the known ^{14}C contamination. The best fit yields a mixing of $(1.4\pm0.1)\%$ and reduced χ^2 of 1.06. From Mortara *et al.* (1993).

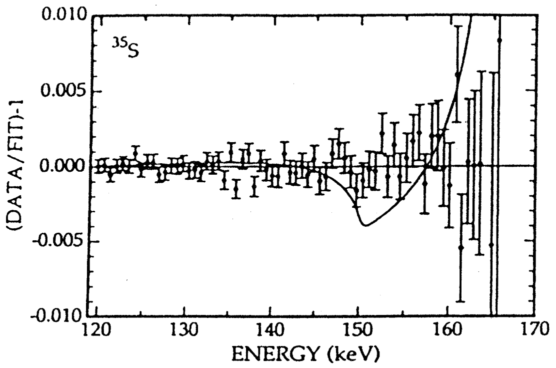

FIG. 31. Residuals from a fit to the pileup-corrected ^{35}S data assuming no massive neutrino; the reduced χ^2 for the fit is 0.88. The solid curve represents the residuals expected for decay with a 17-keV neutrino and $\sin^2\theta = 0.85\%$; the reduced χ^2 of the data is 2.82. From Mortara *et al.* (1993).

search for a 17 keV neutrino with an apparatus with demonstrated sensitivity. We find no evidence for a heavy neutrino, in serious conflict with some previous reports" (p. 396).

This experiment was clearly convincing. It met all the criteria previously suggested by Hime and Bonvicini and demonstrated an ability to detect a kink in the spectrum had one been there. Along with the extremely high statistics Tokyo experiment discussed earlier, it provided very strong evidence against the existence of the 17-keV neutrino.

The Berkeley group published a later, higher-statistics result for the internal bremsstrahlung spectrum of ^{55}Fe (Wietfeldt, 1993a). They discussed the question of whether a smooth distortion in the spectrum would affect their analysis. "Finally to verify that this analysis would not be affected by a smooth distortion in the spectrum, Monte Carlo spectra were generated with arbitrarily chosen linear and quadratic factors, with and without massive neutrinos; and these spectra were analyzed in the same way. The presence or absence of a neutrino kink was always correctly found. . . . Thus, we conclude that the effect reported previously in lower statistics experiments, and interpreted to be the result of a 17 keV neutrino, is not in fact caused by a massive neutrino" (p. 1762). "In particular, a 17 keV neutrino with $\sin^2\theta = 0.008$ is excluded at the 7σ level" (p. 1759).[60]

Once again, Schwarzschild provided a summary of the situation in *Physics Today,* in an April 1993 article entitled "In Old and New Experiments, the 17-keV Neutrino Goes Away" (Schwarzschild, 1993). He noted that the 17-keV neutrino had received five severe blows during the preceding twelve months. Among these were the high-statistics and persuasive results of Kawakami's group in Tokyo (Kawakami *et al.*, 1992; Ohshima, 1993; Ohshima *et al.*, 1993) and those of the Argonne group (Mortara *et al.*, 1993). Schwarzschild also quoted Hime on the question of electron scattering that led to his reanalysis. "I didn't pay too much attention to this critique [that of Piiloneen and Abashian] at the time. We hadn't included scattering off the baffles because we knew that scattering at the detector was a much bigger source of electron energy degradation. That's something we *had* included in our fits and shown that it had no effect on our 17-keV signal. And besides, Simpson and I had seen the same 17-keV signal in a variety of earlier geometries at Guelph that had nothing to do with baffles. But after Freedman's result [Mortara *et al.*, 1993] I knew I had to take a serious second look" (Hime, qtd. in Schwarzschild, 1993, p. 18). Schwarzschild remarked that Jelley had confirmed Hime's reanalysis of their results and had redesigned the apparatus to avoid the problems. Jelley continued to take data, and Schwarzschild reported that the 17-keV neutrino signal seemed to have gone away.

Schwarzschild also raised the issue of the positive results from ^{14}C obtained by the Berkeley group.

> After the report of Norman's initial four-month run, his Berkeley group found 17-keV signals in each of three additional runs of comparable statistics. Just as they were about to publish these new confirmatory results at the end of 1991, the group acquired a new data acquisition system that allowed them, for the first time, to discard events by off-line software veto. Because the betas have a range of about 100 μm in germanium, one wants to discard decays that are too close to the edge of the crystal, lest they get out without depositing their full energy. To that end, the Berkeley group had surrounded the detector's fiducial volume with a Ge guard ring designed to veto events too close to the edge.

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[60] The probability of a 7σ effect is $2.6\times10^{-10}\%$.

> All the 1990–91 running had been done with just an on-line hardware veto from the guard ring. But now, with the new software-veto capability, the group could take a closer look at the events the guard ring was discarding. And what they found was very disturbing; it called all their previous results into question. Far too many events were being vetoed, and there was a peculiar correlation between the energies recorded in the central crystal and in the guard ring. Eventually it was found that the culprit was electronic cross talk between the central detector and the ring.
>
> With the errant guard ring taken care out of commission, the Berkeley group took new ^{14}C data throughout 1992 with a software fiducial veto and found that its 17-keV neutrino had vanished. (Schwarzschild, 1993, p. 18)

The Berkeley group continued to work on trying to find the reason for the artifact in their ^{14}C data. The cause, found in 1993, was quite subtle. The way in which the center detector was separated from the guard ring was by cutting a groove in the detector. "The n^+ is divided by a 1-mm-wide circular groove into a 'center region' 3.2 cm in diameter, and an outer 'guard ring.' By operating the guard ring in anticoincidence mode, one can reject events occurring near the boundary which are not fully contained within the center region" (Sur *et al.*, 1991, p. 2444). Such events would not give a full energy signal and would thus distort the observed spectrum.

What the Berkeley group found was that ^{14}C decays occurring under the groove shared the energy between both regions without necessarily giving a veto signal, thus giving an incorrect event energy and distorting the spectrum. Although their earlier tests had indicated that the ^{14}C was uniformly distributed in the detector, their new tests showed that between one-third and one-half of the ^{14}C was localized in grains. They also found that approximately 1% of the grains were located under the groove. Thus the localization of the ^{14}C combined with the energy sharing gave rise to a distortion of the spectrum that simulated that expected from a 17-keV neutrino (Norman, 1994; Wietfeldt *et al.*, 1993b, 1994).

There was virtually no evidence left that supported the existence of the 17-keV neutrino. Simpson was not, however, totally convinced. Although he admitted that he owed Glashow a bottle of wine, the stake of a wager on the existence of the 17-keV neutrino, he remarked, "Still it's very peculiar that all these different experimental arrangements should have conspired to give the same spurious signal. At the moment it appears that only the Guelph results remain to be explained, so we're continuing our experiments" (Simpson, quoted in Schwarzschild, 1993, p. 18). As of November 1993 Simpson was still working on the problem. He agreed that the preponderance of evidence was against the existence of the 17-keV neutrino, but he hesitated to say that it was definitely gone. He noted that the presence or absence of the effect was quite sensitive to the method of data analysis used, although he believed that the later experiments seemed to avoid that problem by using both wide and narrow energy range analysis. He also remarked on the oddity that the very different experimental artifacts, those of Hime and of the Berkeley group, both gave effects at the same neutrino mass, an unlikely occurrence. These artifacts do not, however, explain his original positive results. He is currently searching for a possible error or artifact that might explain why his original result was incorrect (Simpson, 1993).

The consensus of the physics community is, however, that the 17-keV neutrino does not exist.

IV. DISCUSSION

This episode illustrates important points about the methodology of scientific practice. This methodology is particularly apparent in cases such as this in which discordant experimental evidence both supports and disconfirms an experimental result or a speculative hypothesis. Perhaps the most important point is that the decision that the 17-keV neutrino did not exist was a reasonable one, based on epistemological considerations. As we have seen, the discord between the experimental results was resolved by a combination of finding errors in one set of experiments with the accumulation of evidential weight in the other set.

Other commentators on science have questioned whether epistemological arguments enter into this type of decision.[61] For example, Harry Collins argues for what he calls the "experimenters' regress" (Collins, 1985). In his discussion of the early experimental attempts to detect gravity waves, Collins argues that we cannot be sure that we can actually build a gravity wave detector, that we might have been fooled into thinking we had the recipe for constructing one, and that "we will have no idea whether we can do it until we try to see if we obtain the correct outcome. *But what is the correct outcome* [emphasis in original]?"

> What the correct outcome is depends upon whether or not there are gravity waves hitting the Earth in detectable fluxes. To find this out we must build a good gravity wave detector and have a look. But we won't know if we have built a good detector until we have tried it and obtained the correct outcome! But we don't know what the correct outcome is until. . .and so on ad infinitum.

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[61] Collins and Pickering have argued, for example, that factors such as career interests, consistency with existing community commitments, recycling of expertise and utility for future practice enter into such decisions. I believe the history shows no evidence of this, although such factors certainly enter into the question of pursuit, discussed below.

> The existence of this circle, which I call the "experimenters' regress," comprises the central argument of this book. (Collins, 1985, p. 84. In these quotations one could easily substitute "17-keV neutrino" for "gravity waves" without changing the sense of Collins's statement.)

More succinctly, "proper working of the apparatus, parts of the apparatus and the experimenter are defined by the ability to take part in producing the proper experimental outcome. Other indicators cannot be found" (Collins, 1985, p. 74). I have argued elsewhere that Collins's analysis of the gravity wave episode is incorrect (Franklin, 1994). I also believe that the history of both gravity waves and the 17-keV neutrino shows not only that such epistemological indicators can be found, but were.

What are these epistemological indicators or criteria? In previous work I have argued for an epistemology of experiment, a set of strategies that can be philosophically justified and used to argue for the validity of an experimental result. I have also shown that they are, in fact, used by practicing scientists. These include (1) experimental checks and calibration, in which the experimental apparatus reproduces known phenomena; (2) reproduction of artifacts that are known in advance to be present; (3) intervention, in which the experimenter manipulates the object under observation; (4) independent confirmation using different experiments; (5) elimination of plausible sources of error and alternative explanations of the result (the Sherlock Holmes strategy); (6) use of the results themselves to argue for their validity; (7) use of an independently well-corroborated theory of the phenomena to explain the results; (8) use of an apparatus based on a well-corroborated theory; and (9) use of statistical arguments (Franklin, 1986, 1990).

The problem is that *all* of the experiments discussed here offered such strategies and arguments in support of their results.[62] The question was whether these epistemological strategies had been applied correctly.[63] How does one argue that such a strategy or a correction to an experimental result has been incorrectly applied? One possibility is to show that its use in a particular experiment generates a contradiction with accepted results. A second possibility is to show that some plausible source of error or an alternative explanation of the result has not been considered.[64] (What is considered plausible may change with time, as discussed below.) One might also examine assumptions concerning the operation of the apparatus and demonstrate empirically that they are incorrect. One might also show that plausible explanations of results, suggested by others, are incorrect. All of these occurred in this episode.

Other criteria may also exist. In a particular experiment some epistemological strategies may have been applied successfully whereas others had failed. This is illustrated in the episode concerning experiments on atomic parity violation. In this case there was a conflict between the discordant results of the Washington and Oxford atomic-parity-violation experiments and the SLAC E122 experiment on electron scattering. The Oxford experiment had admitted systematic uncertainties that were the same size as the predicted effect. In addition, the Washington results were internally inconsistent. Both of these effects made their results less credible. The SLAC E122 experiment had no such failures and therefore had more evidential weight.[65]

Sometimes the failure to reproduce an observation, despite numerous attempts to do so, might be legitimately regarded as casting doubt on the original observation, even if no error has been found in that experiment. This would be a case of a preponderance of evidence.[66]

The history shows us that deciding on the correct answer to the question of the existence of the 17-keV neutrino involved not only numerous repetitions of the experiment, but also criticism and discussion of the experimental results, of the experimental apparatuses, and of the methods of analysis used. The history also shows that these criticisms and discussions were taken seriously and acted upon by the scientists involved. This was, in effect, applied epistemology.

Let us review in detail how the decision that the 17-keV neutrino did not exist was reached. What makes this process so interesting is that the original discordant results were obtained with two different, and seemingly reliable, types of experimental apparatuses. One might worry that it was a peculiarity of one of the types of ap-

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[62] Recall the effort that Simpson devoted to calibrating and checking his apparatus.

[63] Rasmussen (1993) has argued that these strategies are open to negotiation and dispute. That is certainly true in principle, but I do not agree that it happens in practice (Rasmussen, 1993). I have presented case studies in which these strategies are explicitly used. See, for example, Franklin (1986, Chap. 7).

[64] In the case of the fifth force, a proposed modification of the law of gravity, the positive results reported by experiments measuring gravity on towers and in mine shafts were shown to have neglected the effects of local terrain. For details see Franklin (1993a).

[65] Not all experiments are equal. Some experiments are more equal than others (with apologies to George Orwell). For details of this discussion see Franklin (1990, 1993b), Ackermann (1991), Lynch (1991), and Pickering (1991).

[66] In the case of the fifth force, no error has been found in Thieberger's positive result. There have been, however, numerous other experiments that have given negative results. The overwhelming weight of these negative results has persuaded the physics community, as well as Thieberger himself, that the original result is wrong.

paratus that either created an artifact or that masked a real effect. As Schwarzschild remarked in 1991, "On one thing everyone seems to agree. After six years, the experimenters must begin to resolve the stubborn discrepancy between the two different styles of beta-decay experiments" (Schwarzschild, 1991, p. 19). Both Simpson's original report of the 17-keV neutrino and the other positive results were obtained using solid-state detectors. Such detectors had been in wide use since the early 1960s and their use was well understood. The early negative results were obtained using magnetic spectrometers. This type of apparatus had been used in nuclear beta-decay experiments since the 1930s, and both the problems and the advantages of using this technique had been well studied [see Franklin (1990, Chap. 1) for details of some early experiments].

Simpson's first report of the 17-keV neutrino was unexpected. It was not predicted, or even suggested, by any existing theory. Faced with such an unexpected result, the physics community took a reasonable approach. Some scientists tried to explain the result within the context of accepted theory. They argued that a plausible alternative explanation of the result had not been considered. This involved the question of whether the theory used in the analysis of the data and in comparisons with the experimental result was correct. This is an important point. An experimental result is not immediately given by an examination of the raw data, but requires considerable analysis. In this case the analysis included atomic physics corrections, needed for the comparison of the theoretical spectrum and the experimental data. Everyone involved agreed that such corrections had to be made. There were, after all, large effects of this kind exhibited in the phenomenon of internal bremsstrahlung. The atomic physics corrections used by Simpson in his analysis, particularly the screening potential, were questioned by other scientists. All of these suggestions were aimed at accommodating the unexpected result. Several calculations indicated, at least qualitatively, that Simpson's result could be accommodated within accepted theory, and that there was no need for the suggestion of a new particle.

The physics community also tried to replicate Simpson's results. Within a year, five attempted replications of Simpson's experiment, using primarily, but not exclusively, magnetic spectrometers, all gave negative results. This was an attempt to provide independent confirmation of a result using different experiments. The apparatuses used in the attempts used a different decay source, ^{35}S as opposed to tritium, and magnetic spectrometer apparatuses as opposed to solid-state detectors. By using different sources, one could check on whether Simpson's observed effect might be due to some atomic physics phenomena peculiar to his choice of decay source. Had positive results been found, then one would have concluded that no such effects existed, and the experiments would have provided more support for Simpson's original result than would have been the case had the experiments used the same source.[67] The difficulty is that, although greater support for a result is provided when different experiments agree, when such different experiments disagree, we do not know which result is correct and will suspect that the different results are caused by some difference in the experimental apparatus, or in the analysis of the data.

In addition, Simpson offered several criticisms of those early negative results. These involved the analysis procedures used in those experiments. One feature of magnetic spectrometer experiments was the need for a smooth, energy-dependent "shape-correction factor" to obtain the decay spectrum. In ordinary experiments the use of such a factor was not crucial; but, as Simpson pointed out, when one looked for effects of the order of one part in a thousand, then one had to be quite certain of one's analysis procedure ($\Delta K/K$, the quantity of interest in the beta-decay spectrum, was approximately 10^{-3}; see Figs. 1 and 9). Simpson also questioned other aspects of the analysis. The first was the use of a wide energy range, rather than a narrow one, to calculate the expected spectrum and to fit the spectrum parameters. He noted that 45% of the effect of a heavy neutrino occurred within 2 keV of the neutrino threshold. He also criticized the procedure of merely adding the expected effect of a 17-keV neutrino to the best-fit spectrum, rather than incorporating the effect into the spectrum and then determining the best fit. He claimed that these procedures tended to minimize the effect of the proposed particle. In both of these criticisms, Simpson was questioning whether the Sherlock Holmes strategy of eliminating plausible sources of error or alternative explanations of the result had been correctly applied. The question was whether the analysis procedures used might mimic or mask the effect of a 17-keV neutrino. Simpson was suggesting that they might. As we have seen, others agreed with Simpson.

Subsequent experiments acquired sufficient statistics so that a local analysis (a narrow energy range which minimized the effects of the shape-correction factor) could be used; and other experiments used both a narrow and a wide energy range, so that any difference due to the energy range used in the analysis might be seen. In addition, the type of spectrum fitting suggested by Simpson was used in several of the later experiments (see, for example, Hetherington *et al.*, 1987; Becker *et al.*, 1991; Radcliffe *et al.*, 1992; Norman *et al.*, 1993; Ohshima, 1993). These experiments found no evidence for a 17-keV neutrino and eliminated the analysis procedure as a possible explanation of their failure to find it. Simpson also reanalyzed the early negative results using his own preferred analysis procedure and argued that they did not, in fact, argue against the existence of the 17-keV neutrino.

Just as others took Simpson's criticisms seriously, so

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[67] For a discussion of the support provided by the "same" and "different" experiments, see Franklin and Howson (1984).

Simpson reacted to the criticism of others. He reanalyzed his own data using the different atomic physics correction to the spectrum that his critics had suggested and found that his effect, although reduced in size, was still present.

Simpson had enough confidence in his own work to continue his investigation, despite both the criticism and the negative results,[68] and reported additional positive results (Hime and Simpson, 1989; Simpson and Hime, 1989). Recall that there was not much experimental work done on the 17-keV neutrino between the five negative results reported in 1986 and these new results. This further confirmation of the original Simpson result, using both a different source and a different solid-state detector, encouraged others to further investigate the phenomenon. Several of these experiments obtained positive results, with particularly persuasive results provided by Hime and Jelley (1991) and Sur *et al.* (1991).

At the same time, other experiments with both improved statistics and analysis procedures were finding increasingly persuasive negative results. Piilonen and Abashian (1992) suggested that a background effect might be simulating the presence of a 17-keV neutrino in the Hime-Jelley experiment. The persuasive negative results encouraged Hime to consider the Piilonen-Abashian suggestion seriously[69] and to reanalyze his own result. He found, using an experimentally checked Monte Carlo calculation,[70] that scattering of the decay electrons in the experimental apparatus cold explain his result, without the need for a 17-keV neutrino. At approximately the same time, the Berkeley group found an error in their own positive result on ^{14}C. Their attempt to guard against a spectrum distortion caused by decays close to the edge of their detector, which do not deposit their full energy, had not worked. It had, instead, caused a spectrum distortion that mimicked the effect of a 17-keV neutrino. Improvements in the apparatus had allowed them to examine the energy deposition in both the central detector and the guard ring for each event, whereas the previous setup had not allowed this. They had assumed that there was no energy sharing and found that there was an effect that distorted their spectrum. They had found not merely a plausible source of error, but an actual error in their result.

The newer negative results were persuasive because of their improved statistical accuracy, and because, in the case of the Argonne experiment, physicists were able to demonstrate that their experimental apparatus could detect a kink in the spectrum if one were present (Mortara *et al.*, 1993). This was a direct experimental check that there were no effects present that would mask the presence of a heavy neutrino. These experiments met Hime's suggested criteria of a demonstrated ability to detect a kink combined with high statistics so that a local analysis of the spectrum could be done.[71] Ohshima (1993) had also shown that the shape-correction factors used in their experiment did not mask any possible 17-keV neutrino effect. This combination of almost overwhelming and persuasive evidence against the existence of a 17-keV neutrino, combined with the demonstrated and admitted problems with the positive results, decided the issue. There was no 17-keV neutrino. It seems clear that this decision was based on experimental evidence, discussion, and criticism, or, in other words, epistemological criteria. The process of designing a good "17-keV neutrino" detector was not simply a matter of deciding whether the particle existed and then asserting that a good detector was one that gave the correct answer. The community decided which were the good detectors, based on epistemological criteria, and then decided that the particle did not exist.[72]

Another interesting aspect of this episode is that it was almost completely driven by experiment and observation. The existence of a heavy neutrino would have had important implications for theory, particularly for electroweak interactions, and for cosmology. In the early 1980s, for example, the well-established Weinberg-Salam unified theory of electroweak interactions might very well have accommodated a light, massive neutrino. A neutrino with mass in the keV range would, however, have made the revised theory "ugly."[73] A heavy neutrino would also have had important implications for astrophysics. During the 1980s astrophysicists were exploring the possibility that the "missing mass" in existing cosmological theories might be accounted for by heavy neutrinos.[74] Although these theoretical considerations might have had an effect on work on the 17-keV neutrino, they did not. Activity in the field was determined by experimental results and by the desire to answer the question of whether the 17-keV neutrino existed. These experiments had a life of their own.

This is clearly shown in Fig. 32, which shows the number of preprints on the 17-keV neutrino received at CERN as a function of time (Morrison, 1993). This is

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[68] This is the question of pursuit, the further investigation of a phenomenon or of a theory, rather than justification, the process by which a result or theory becomes accepted as scientific knowledge. These are not always easy to separate, but it is quite clear from the history that a decision on the 17-keV neutrino had not yet been reached at that time. For further discussion see below and Franklin (1993b).

[69] New evidence may make an explanation more plausible.

[70] I will discuss later the question of experimentally checking a Monte Carlo calculation.

[71] In addition, Morrison showed that Simpson's most persuasive reanalysis of one of the early negative results was dependent on a statistical fluctuation. Hetherington *et al.* (1987) had also suggested that this might be a problem.

[72] These arguments provided good grounds for the belief that the 17-keV neutrino did not exist, but did not, of course, guarantee it.

[73] This was Steven Weinberg's description (Weinberg, 1993).

[74] Observations of galactic rotations also pointed to "dark matter" and missing mass.

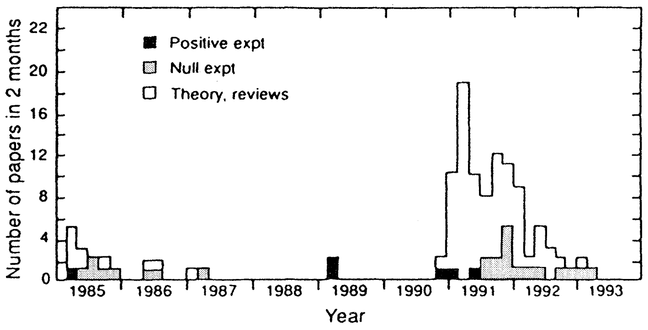

FIG. 32. Preprints on the 17-keV neutrino received at CERN as a function of time. From Morrison (1993).

not a complete picture of the activity in the field, because not everyone sent preprints of their work to CERN; but it does give an accurate relative picture of work in the field. The figure shows an initial spurt of activity, both experimental and theoretical, triggered by Simpson's original claim in 1985. Some of that early theoretical work consisted of the attempts to explain Simpson's result without the need for a heavy neutrino (discussed earlier). Within a year, five negative experimental results were reported. These negative results, combined with the alternative explanations, had a chilling effect on work in the field. As we have seen, however, work continued, albeit at a low level of activity.

A second and much larger burst of activity began in late 1990. This coincided with the new positive results on the 17-keV neutrino reported by Hime and Jelley at Oxford and by Norman's group at Berkeley. One may speculate that the reason these results had such a positive effect, whereas the positive results reported in 1989 by Simpson and Hime had no such effect, was that they were the first positive results reported by scientists other than Simpson. Physicists may have wondered whether it was some artifact produced by Simpson's experimental apparatus or some problem with his data analysis that was producing the effect.[75] The support for the 17-keV neutrino provided by other experimental groups using different experimental apparatuses and data analysis procedures was greater than that provided by Simpson's similar repetitions of his own experiment.[76] The numerous and persuasive negative results reported from 1991 to 1993 ended activity in the field. Experimental evidence has shown that the 17-keV neutrino did not exist.

One should also note the important and legitimate role that Monte Carlo calculations, computer simulations of experiments, played in this episode. It was Hime's Monte Carlo calculation of the effect of electron scattering in his experimental apparatus that convinced him, as well as the rest of the physics community, that his result supporting the existence of the 17-keV neutrino was incorrect. The Berkeley group also used Monte Carlo methods to check that their analysis procedure was not masking or creating the effect of the 17-keV neutrino. They deliberately inserted the effect of such a neutrino into some, but not all, of their simulations and found that their analysis procedure correctly identified the presence or absence of the neutrino in every case. Monte Carlo simulation was also important in Bonvicini's study.

Pickering, however, has questioned the use of such Monte Carlo calculations and has suggested that their use in experiments precludes the use of the results as evidential support (Pickering, 1984). In discussing the use of such a simulation in the Gargamelle experiment, which reported the existence of weak neutral currents, Pickering noted that several of the inputs to the calculation could be questioned. These included the beam characteristics, the interaction of nucleons with atomic nuclei, neutron production, and idealized experimental geometry. "My object here is simply to demonstrate that assumptions were made which could be legitimately questioned: one can easily imagine a determined critic taking issue with some or all of these assumptions. Moreover, even if all of the assumptions were granted, it remained the case that they were input not to an analytic calculation, but to an extremely complex numerical simulation. The details of such simulations are enshrined in machine code and are therefore inherently unpublishable and not independently verifiable. Thus the sceptic could legitimately accept the input to the calculation but continue to doubt its output" (Pickering, 1984, p. 96).

What Pickering overlooks is that considerable effort is devoted to checking the results of that calculation by comparison with experimental evidence that is independent of the result in question.[77] The results of this checking are, in fact, publicly available in the published work. Thus Hime's Monte Carlo calculation had shown that intermediate scattering effects in his aluminum baffles could account for his data, just as well as did the assumption of a 17-keV neutrino. He checked his calculation by comparing it to data taken with the same experimental apparatus and geometry using a monoenergetic internal-conversion electron source. The excellent fit between

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[75] Glashow's positive comment on the 17-keV neutrino came only after he had learned of the Berkeley and Oxford results, although he did cite the 1989 Simpson-Hime results.

[76] This is an example of "different" experiments providing more support for a hypothesis than do repetitions of the "same" experiment. For a general discussion of this, see Franklin and Howson (1984). Recall, however, that the 1989 Simpson-Hime experiments did include significant differences from the original Simpson experiment. These included the use of a germanium, rather than a Si(Li), detector in the tritium experiment, and the use of a ^{35}S source with a Si(Li) detector in the other experiment. Simpson was certainly aware of possible problems in his first experiment, and also aware of the fact that different experiments would provide more support for his conclusion. See the earlier discussion of these 1989 experiments.

[77] In addition, the input parameters to the Monte Carlo calculations are the best and most reliable ones that the experimenters can find.

these measurements and his simulation argued for the correctness of his calculation (see Fig. 29).

Such checks are usually done. For example, in an experiment designed to measure the energy dependence of the form factor in K^+_{e3} decays, $K^+ \rightarrow e^+ + \pi^0 + \nu$, the way in which the energy-dependent parameter λ was fixed was by comparing Monte Carlo–generated spectra with different values of λ with the experimental data (Imlay *et al.*, 1967). The Monte Carlo simulation was checked by comparing its results with a sample of background events.

> It was also necessary to know the energy distributions relating to background events. These distributions were obtained from the Monte-Carlo generated sample of spurious K^+_{e3} events. Indications of the validity of this calculation were obtained from the distributions of positron momentum, γ-ray energy, and π^0 energy for those *events which were rejected by selection criterion 3.* This criterion required that the counter behind each spark chamber give a pulse if the shower in the chamber contained sparks in either of its last two gaps. These rejected events should differ from the background events in the final sample of 1867 nominal K^+_{e3} events only with regard to selection criterion 3. Thus, when reconstructed as K^+_{e3} decays, the background events that passed and failed criterion 3 should have exactly the same distributions. These are shown in Fig. [33], along with the calculated distributions for Monte-Carlo generated spurious events. The good agreement provides strong support for the background calculation, particularly since these distributions differ substantially from the corresponding distributions for good events. (Imlay *et al.*, 1967, p. 1209)

Pickering also overlooks the fact that the robustness of the results of a Monte Carlo calculation is checked against reasonable variations in the input parameters. This is because, as Pickering himself notes, these parameters are not exactly known. Typically, the results are not sensitive to such variations. If they are, then the results must be used with extreme care and may not, in fact, be usable.

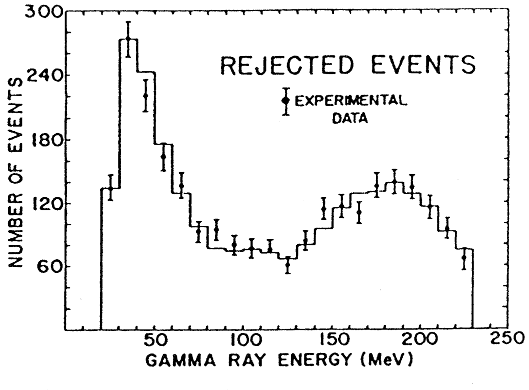

FIG. 33. Comparison of Monte Carlo–generated γ-ray spectrum with experimental data for rejected events. From Imlay *et al.* (1967).

Determined critics or skeptics might question such Monte Carlo calculations, but they would have to discount the independent evidence provided.

In thinking about this episode, as well as other episodes, we should distinguish between the processes of pursuit and justification; between the further investigation of a phenomenon or a theory and the process by which it is accepted as knowledge by the scientific community (see note 63). Although both of these processes were going on simultaneously, we should note that belief that a hypothesis or a result is correct is not a necessary prerequisite for working on it. The reasons for further investigating (pursuing) a hypothesis or result are not usually the reasons by which one justifies belief in them. With respect to the existence of the 17-keV neutrino, the attitude of scientists working on the problem varied from belief to disbelief with various intermediate positions. Recall the differing summaries of the situation offered in 1991 and 1992. Simpson was quite positive; Morrison, quite negative; and Hime adopted a moderate, agnostic position.

During the period 1985–1993 considerable theoretical work was done on the 17-keV neutrino.[78] These papers attempted to incorporate the particle into accepted particle theory, to include it in a new theory, or to look for further implications of such a particle. Not everyone was as positive as Glashow about the existence of the particle (see earlier quotation). More agnostic views were, "The possible discovery of a 17 keV neutrino in β-decay experiments is a challenge to both astrophysics and cosmology" (Altherr *et al.*, 1991, p. 251) and "Recent experimental evidence for a 17 keV neutrino mass eigenstate with 0.8% mixing to ν_e, while still disputed, has led to extensive theoretical investigations because it is very difficult to reconcile a particle with these properties with standard particle theories, not to mention cosmology and astrophysics" (Madsen, 1992, p. 571).

Scientists may have reasons other than belief in the correctness of the theory or result for pursuing it further. As Madsen indicated above, an experimental result may call for a new theory because it is incompatible with accepted theory. One might also work on something because it fits in with an existing research program or because it looks like a fruitful, important, or interesting line of research. "The existence of massive neutrinos would have profound implications for both particle physics and astrophysics" (Norman *et al.*, 1991, p. S291).

Experimenters may have additional experimental reasons for pursuit. These may include the fact that the experiment can be done with existing apparatus or with small modifications of it. The measurement may also fit

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[78] A survey of papers and reprints received at the Stanford Linear Accelerator Center, a major research facility, for the period 1985–1992 shows approximately 60 theoretical papers on the 17-keV neutrino.

in with an existing series of measurements in which the experimenters have expertise. We might call these instrumental loyalty and the recycling of expertise. Simpson had been using a solid-state detector to search for massive neutrinos in β-decay experiments for several years before he reported the existence of the 17-keV neutrino, and the other groups had considerable experience in doing beta-decay experiments. Another reason for pursuit might be that the experimenters might have thought of a clever way to do the experiment. Thus the Berkeley group remarked, "Moreover, we were aware of a unique detector... that was ideally suited for this experiment" (Sur *et al.*, 1991, p. 2444).

How was the decision concerning the existence of the 17-keV neutrino made? I believe I have shown that the decision that it did not exist was made on the basis of valid experimental evidence. I have also argued that epistemological criteria were used in the evaluation of that evidence. The process also involved discussion and criticism that was taken seriously by everyone involved. Popper has characterized science as "critical rationality." That seems an apt description.

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