A world without solar neutrinos

German physicist Erich Bagge discusses how he came to understand, and ultimately prove, that neutrinos do not exist, in an interview with the Fusion Energy Forum's Jonathan Tennenbaum.

Erich Bagge, a student of Werner Heisenberg and Arnold Sommerfeld, is a pioneer of the nuclear energy industry in West Germany and the designer of the world's first nuclear-powered commercial vessel, the Otto Hahn. *Bagge is a professor emeritus of physics at the Christian Albrecht University in Kiel, Germany. The interview was conducted by Jonathan Tennenbaum, director of the European Fusion Energy Forum, and first appeared in the German-language magazine,* Fusion *in 1991 (No. 1). It was translated from the German by John Chambless.*

Q: Recently, sensational results were announced from the Soviet-American Gallium Experiment (SAGE). This experiment was supposed to have measured the flow of neutrons from the Sun, and yet it seems that they measured exactly nothing! Some physicists have already reacted, and have announced a "revolution in physics." They want to explain the negative results by means of "neutrino oscillation," whereby neutrinos are changed on their way from the Sun to the Earth and therefore could not be measured in the SAGE experiment. This attempt at an explanation seems to be somewhat far-fetched.

You, Professor Bagge, have asserted for years that solar neutrinos do not exist! You developed a theory of beta decay in which, in contradiction to the neutrino hypothesis of Wolfgang Pauli from the 1930s, neutrinos are not necessary. And in your early 1990 book *Welt und Antiwelt als physikalische Realität* (*World and Anti-World as Physical Reality*), you referred to the SAGE experiment and predicted the negative results. Your predictions are now confirmed. Please tell us how you came to doubt the existence of beta-decay neutrinos.

Dr. Bagge: In fact, it happened like this: Until 1972, I supported the idea in my lectures that neutrinos must play an important role in elementary particle physics and also in the realm of nuclear physics because, otherwise, there is no way to understand the electron energy deficit in beta decay. There, neutrinos were a marvelous help, and I supported this old idea of Wolfgang Pauli for almost 40 years and passed it on to my students.

Then, I first heard of the experiments to detect solar neutrinos, especially after the experiments to measure neutrinos (or, more precisely, anti-neutrinos) that come from nuclear reactors had not yielded the desired results. It simply didn't work out very well. They were working with extremely delicate effects that demanded a very complicated and difficult technology. Those were the experiments of Prof. F. Reines [University of California at Irvine] and his colleagues in the United States. Then, however, the idea was broached that it would be much more simple to directly seek for the proper neutrinos that must come from the Sun.

In the Sun, other processes occur than in a nuclear reactor. In a nuclear reactor, there is a normal beta decay in which electrons are produced and, in combination with the negative electrons, anti-neutrinos. Reines attempted to detect these. In the Sun, one of the essential processes is that in which two protons unite into a deuteron, liberating one positron and then producing one proper neutrino. And these neutrinos are produced in such a quantity that they should be measurable

Stuart Lewis

Prof. Erich Bagge during an August 1985 seminar with the U.S. Fusion Energy Foundation.

even here on Earth, despite the low probability of such particles setting off a nuclear process. Professor Davis in the United States first attempted to do that, for which he constructed a fantastic experiment. I often expressed my admiration to my students that this experiment had been done, at such expense, to detect solar neutrinos.

At that time, Davis held a lecture at the international cosmic-ray conference in Denver in 1972 on his measurements. The result was—in the solar neutrino units, or SNUs, which Davis also introduced—0.4 SNU, where 5 SNU really should have been observed. The inaccuracy that he specified for his measurements was 0.6 SNU. That meant, at least for any normal physicist, that the result was null; the range of variations was twice as large as the measured effect.

Q: Was that the conclusion that was drawn then?

Bagge: Yes, of course, there was discussion of that. Davis himself said that what he observed was essentially a null effect. But that was in 1972. I did not directly take part in this discussion, but was deeply impressed by it. I went home and, so to speak, carried the results around with me, and reflected on what the reason could be that nothing came out of the experiment. And then, at some time or other, it became clear to me: If we can observe nothing, we must explain the beta effect in a different way. Although Professor Reines believed he had observed the anti-neutrinos from a nuclear reactor, his colleagues ultimately doubted that result. It spread in the literature, and Reines was always asked whether it were credible.

Then, however, Davis "improved" his results by interpreting the same experiment a year and a half later in a different way, and, in fact, in such a way that criticism was again set off, not only from his American colleagues, but particularly from us here. For example, one of our earlier colleagues, Professor Grupen, looked at the new interpretation by Davis and said that this is not in the least supportable from a physics point of view.

Davis had done the following: He took certain of the measures and to a certain extent dropped the cases in which his original procedure gave negative results. These negative values had come about in that he received, in addition to the effects that had to be set off by solar neutrinos, additional effects as the result of cosmic radiation, and thus a background effect that had to be subtracted from the measured values. Since these radiation effects have natural variations, it can happen, as chance would have it, that, on subtracting the average variation, a measured value below the theoretically expected average background effect appears, that is, a negative value. But a positive effect can also be set off by the variations of the cosmic radiation. Davis then said, negative values cannot occur in reality, and so he set the negative values to zero and then recorded only the positive values. And if these are fit together, he suddenly had 1.6 SNU rather than 0.3 SNU plus an inaccuracy of 0.6 SNU. And is that is quite a lot!

I knew about that, because Professor Grupen informed me of it very early. In the meantime, but independently of this development, and thus until Davis announced his new interpretation, at the end of 1973 or beginning of 1974, I had reflected on how beta decay can be explained without neutrinos. At any rate, I experienced this entire development of neutrino theory.

When I was studying in 1935 with Werner Heisenberg in Leipzig, I saw that people were a bit hesitant about Pauli's theory. Of course, Fermi's theory of beta decay with the help of Pauli's neutrino hypothesis yields the right electron energy spectrum—and for that reason it must somehow reflect an important aspect of reality—but it caused anxiety that people were operating with something that really can't be observed. Neutrinos are supposed to be electrically neutral and could actually not be observed with the technology of that time. At that time, this anxiety was rather serious.

As was later reported, Pauli himself said at that time, "I have done something that a theoretical physicist really should never do, namely, attempted to explain something that cannot be understood with something that cannot be observed." I didn't know then of this statement, but noticed Heisenberg's uncertainty. He said many times about Fermi's theory, "It's very beautiful, but. . . ."

In short, I was carrying all this around. Then it suddenly struck me that everything could be brought into order by means of Dirac's picture of the anti-particles of the "anti-world."

Q: Can you describe that more exactly?
Bagge: Yes. The Diracian anti-world comes about more or less of itself from a consistent interpretation of Dirac's theory of electrons, and it is nothing more than a consequence of Einstein's energy expression in the special theory of relativity [see box]. This expression is, in fact, ambiguous, and points to, in addition to the usual positive energy values, the possibility of negative energies. At first, people were happy to ignore this because they said to themselves, there can't be negative energies. In Dirac's theory, however, negative energies are unavoidable.

Dirac's theory is so symmetrically constructed on positive and negative energies that we really have to ascribe a physical reality to the negative energies. If all states of negative energy are occupied by electrons, then the "Dirac sea" or anti-world is produced. It is an old idea from potential theory that if all such states are occupied, then the electric fields of the electrons so to speak balance one another out. Essentially, they set up a constant potential that is, however, not observable. Thus, it appears to us in the "upper world" of positive energies as though it were an empty vacuum. In fact, however, the vacuum is, according to Dirac's theory, anything but empty, and is rather densely filled with electrons of negative energy.

If an electron is lacking anywhere, a state in the "sea" of negative energy states is not occupied, then it appears to us as if a particle had been produced there with a positive charge, but otherwise with properties quite similar to those of the electron. Briefly put, that is Dirac's theory of positrons as "holes," as gaps in an otherwise fully occupied "sea" of electrons of negative energy. This theory has similarities with the conception that arose simultaneously, and is now quite common, according to which gaps in the layered structure of solid bodies behave like electrons with a positive charge.

If we assume, I said to myself, that there really are such negative energy states, then beta decay can be explained quite differently. Ultimately, beta decay of a nucleus is nothing more than a transformation of a neutron into a proton. Now, we know from physical experience that the neutron is 1.26 million electron volts (MeV) in mass equivalence heavier than the proton, and this energy surplus can, so to speak, serve to make a proton out of the neutron.

Then, I thought to myself: If the neutron consists of an electron and a proton—thus it appears because afterward it is a proton and an electron, and today we can even produce a neutron out of a proton and an electron, under appropriate conditions—if the electron is, so to speak, there, the neutron can use the energy surplus it has over the proton to transfer an energy surplus to an electron of the anti-world. Then the anti-world electron flows up into the upper world, and the hole in the sense of Dirac's theory that is produced in the anti-world is united in the next moment—it is produced in the same place at which the neutron is—with the electron into a "nothing." The charge is balanced for, and the electron has vanished, so to speak, into the neutron and becomes a proton. Thus we have an electron and a proton in the upper world. That was the idea.

Naturally, I said to myself, if we ascribe a certain character of reality to the anti-world electron, we can't act as if it merely had a negative energy, but it must also have a corresponding *impulse*. This impulse must be taken into consideration, and we can calculate the beta-decay process quite normally with conservation of energy and impulse. That runs practically according to the pattern discovered by Fermi as early as 1934. In this connection, we do not only speak of a neutrino, but rather of an anti-world electron that in the nucleus makes an exchange of energy and impulse with a neutron, and exactly the energy spectrum as calculated by Fermi is produced.

Yet something made me uncertain—and as long as it has remained with that, I would not have dared to publish anything on it. I thought, what interaction can it be that could have the effect of lifting an anti-world electron into an electron of the upper world? And that can only be the magnetic moment of the neutron.

As is known, the neutron has a certain magnetic moment. A neutron cannot have an effect by means of a Coulomb field since it is electrically neutral. But it can enter into combination with an anti-world electron via its magnetic moment. And the interaction is large enough, it can even be very strong. If that is calculated with quite ordinary quantum me-

Dirac's theory and pair production

The following is excerpted from "What Really Happens in Pair Production and Beta Decay? Why Neutrinos Don't Exist," Fusion, *Nov.-Dec. 1985.*

English theoretical physicist Paul Dirac developed a relativistic theory of the electron in 1927 that defined positive and negative energies of electrons, independent of their positive and negative charges. Negative energies proved necessary to account for observed physical processes.

Dirac's theory had the Special Theory of Relativity as its point of departure and took its fundamental features from it, including the fundamental postulate that positive and negative energies are equally warranted. This heritage, together with the fact that positrons normally do not occur in nature as stable—that is, as permanently existing particles—was the basis for Dirac's forumlation of his famous Hole Theory.

Dirac postulated that the physical world has a double structure with positive and negative electron energy states. The positive energy state is the condition of the observable "upper world," while the negative state is in the "anti-world" (or Dirac sea) and is initially unobservable. According to Dirac, given the full occupation of negative energy states, it can be demonstrated mathematically that the interactions of all electrons exactly compensate for one another.

Dirac reasoned that if an electron from the anti-world is hit by a highly energetic photon (gamma quantum) of the upper world, the electron can absorb the photon's energy by interaction in the electron's electromagnetic field. The electron then appears in the upper world. Simultaneously, a hole appears in the anti-world that represents a disturbance of the condition of full occupancy. This deficit in negative charge in the anti-world is observed in the upper world as a positron.

With conceptions developed in this manner, Dirac explained the production of electron-positron pairs by gamma quanta, and simultaneously explained why a positron of the upper world can vanish when an electron from the upper world refills the hole in the anti-world, with both particles then becoming unobservable. Both processes are totally easily observable in physical experiments.

Dirac's interpretation of pair production and annihilation automatically gives the quantum theoretician a rule for calculating the frequencies of the corresponding processes. Following preliminary work by the theoretical physicist F. Sauter in 1933, and calculations by Sauter and W. Heitler, these computations were done in an extremely comprehensive and detailed investigation by Hans Bethe and Heitler. Their results, the Bethe-Heitler theory, have played a major role in modern physics.—*Erich Bagge*

chanical methods, it emerges that the neutron has a lifespan of the order of magnitude—as is known today—of 800 seconds. Back then, Fermi could not calculate that, he didn't have this concept. In 1934, he thought there must be another interaction, a so-called weak interaction, as he called it then, that set off this beta decay. The neutrino that he had taken over from Pauli was, so to speak, switched on in this weak interaction. But he didn't know the constant; he had to determine it from empirical data.

This constant isn't at all needed in the methods as I used them. The magnetic interaction that is obtained is exactly large enough so that the right lifespan of the neutron and the right spectral distribution for the beta decay is produced. I said to myself, this conception could not work so well if it were not a good representation of reality.

I was thus involved in these things and had calculated everything when I got an invitation from Leningrad. The head of the Joffe Institute in Leningrad, who is still alive, invited me for a lecture on cosmic radiation. But I thought at the time, why should I always talk about cosmic radiation; for once, I'll give a report on these other things. And that is what I did, and the following happened: On their own, the Russians had brought an Italian physicist, Pontecorvo, who had quietly gone to Russia after the war. He was present at my lecture. He asked me questions that I could answer without difficulty. Nonetheless, Pontecorvo, as a student of Fermi, said, "I don't believe it." What could I do?

One or two years later, I was, however, invited back to Leningrad to present these things more precisely. The Russians took my work from me, translated it without asking me, and published it in the academic reports out of Moscow under my name! They didn't say a word to me about any of that! I later heard of it from a Russian colleague. Thus the Russians learned very early of what I had done.

At this latter lecture, one of those who attended said, if all of this exists, couldn't experiments be done? In between, I additionally considered that pair production must be interpreted fundamentally from the way that Bethe and Heitler had done.

I knew the Bethe-Heitler theory well, since I had become acquainted with it as a student under Heisenberg. I had also always presented the Bethe-Heitler theory in my lectures as the *nec plus ultra* of modern physics, because to a certain extent, it correctly represented the effective cross-section of pair production, and because this theory played a fundamental role in understanding extensive [cosmic ray] air showers.

The Bethe-Heitler theory is, in fact, *almost* right. That is connected with the fact that it does use the Diracian picture as an idea, and thus the theory of holes, according to which an energy quantum flies past and lifts an underground electron into the upper world and there, where the hole has been produced, a charge deficit is produced that is observed in the upper world as a positron. Here, however, the energy of the hole, previously of the underground electron, is simply "knocked upstairs" so that the positron receives exactly the energy that the anti-world electron had as negative energy, merely in absolute amount. They form a circle, as it were, and have knocked it around at zero from negative to positive energy.

That was not a truly physical thought, since it observed the law of conservation of energy but not of impulse. I made that clear to myself very early. So I thought, we must change the Bethe-Heitler theory on this point. In this connection, it turned out that that doesn't make a *large* deviation since the theory of conservation of energy is fulfilled. What merely effects the conservation of impulse are small corrections. But they are corrections that are nevertheless large enough that, with pair production through light quanta of 6 million electron volts—more precisely, 6.3 MeV—approximately 570,000 electron volts are lacking. It does make a difference, but it is only about 10%, which today is easily measurable. And we measured it.

I had previously referred to the idea of such an experiment in Leningrad. At that time, I thought and hoped, perhaps someone has the guts and will do such a thing. But I waited years. I attempted then to persuade some of my students at Kiel to do it. But you know, my Kiel students were all so well trained in the old way of thinking that began with Pauli in 1930 that they now no longer believed "old Bagge" at all! They said, "Old Bagge is now suddenly doing something entirely different with all the physics that he taught for so many years." All my old students who were now assistants told the new students that anything from Bagge is all junk. And so I got no one to do the experiment.

Subsequently, I thought, there is probably nothing left but to do it myself. At first, I had only failures in Kiel, but then in 1981-1982, an Egyptian student came unexpectedly to me, Ahmed Abu El-Ela. I knew his teacher, Professor Nadi from Cairo, a first-rank physicist, well. His student was to take a degree with me. I said to him, "Mr. El-Ela, I am emeritus; I cannot give you normal doctoral work. But I would like to have something that interests me. You do some pair production with light quanta in Wilson cloud chamber."

Fortunately, we had had a cloud chamber in Kiel in operation for years. So we began first with light quanta of 2.6 MeV from thorium. I knew other similar experiments had already been done and that approximately the Bethe-Heitler spectrum had been produced. But if we consider, for 10,000 Compton electrons there is only around one pair. The effective cross-section for the production of pairs at energies of 2 MeV is very small, but the cross-section for Compton electrons is much greater—it's a matter rather of impacts with electrons in the target. And I said then, we won't be able to see anything here.

Now, I knew from my own work with nuclear reactors, that Compton electrons become more and more infrequent the higher the energy is, but at around 6 MeV, there is a point of intersection where the curve for Compton electrons drops downward and pair production begins to predominate. That is a wonderful point. I proposed to Abu El-Ela that he do the experiment at [the nuclear research reactor] in Geesthacht where we have light quanta of 6 MeV. These irritate the physicists in Geesthacht, who have to do everything to screen these quanta—but for us, they would be just right.

First, Abu El-Ela wanted to carefully go over the literature of pair production, and after he had read it all through, he came to me one day and said, "My dear Professor, please give me some other work. Nothing will come of this. I've read the entire literature on pair production, and it has all been so well measured. There is this American work of Delsasso, Fowler, and Lauritsen, and then there is other work—it is all so well measured that there just isn't any doubt about it any more." I replied, "Now listen, that isn't true at all because I know that if you look at pair production in other experiments then there are always a lot of Compton electrons that simply *should* not be there."

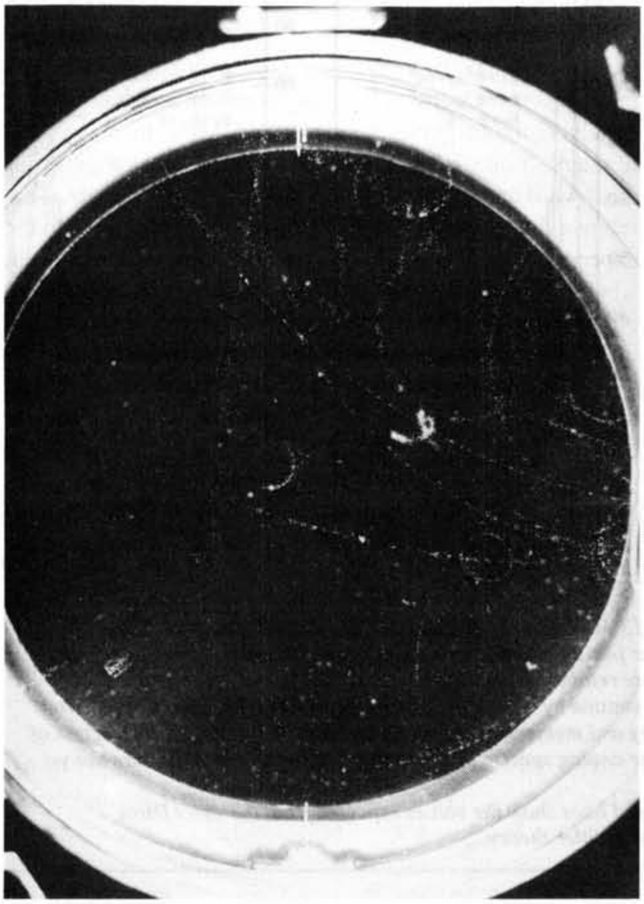
Poor El-Ela then reluctantly went to Geesthacht. In the course of time, in fact, he made 4,000 photographs of pair production with the help of 6 MeV quanta that otherwise so horribly upset the reactor physicists. We irradiated nitrogen in the reactor, whence nitrogen-16 is produced from nitrogen-15, which then decays within 10 seconds into oxygen that is excited to 6.3 MeV. This excited oxygen decays and emits a light quanta of 6.3 MeV. That is almost the perfect energy, and it also worked. We had to quickly "shoot" the nitrogen-16 out of the reactor into an experimental chamber, which took place within 2 seconds. Then it activated its gamma, which radiated into the Wilson chamber [see **Figure 1**].

Now, we already knew all the old experiments. We knew that we must not in any case use a target that was too thick. The American [physicists who tried to test the Bethe-Heitler theory in 1936-1937] L.A. Delsasso, W.A. Fowler, and C.C. Lauritsen had used a target 0.3 mm thick, which meant that an electron or positron that went through the foil had to have at least 800,000 electron volts. If by chance the positron has a lesser energy, then it is trapped in the layer and not observed at all. Then an electron comes out with a relatively larger energy and that was taken to be a Compton electron.

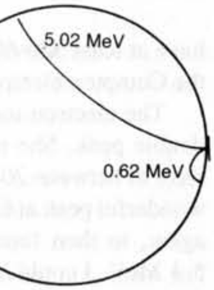
Delsasso, Fowler, and Lauritsen fell into this rubbish. That is, a surplus of positrons are produced accidentally—they couldn't have known that then—with quite small energies, a 100,000 or less electron volts. Anyway, they do not

FIGURE 1

Inside the Wilson cloud chamber at Geesthacht



The chamber is filled with helium and surrounded with two Helmholtz coils that produce a homogeneous magnetic field of 703 gauss. A cartridge filled with nitrogen is irradiated in the reactor and then "shot" into an experimental chamber, where a gamma then radiates into the cloud chamber.
The electron exits upward, with energy of 5.02 MeV, and the positron exits downward with 0.62 MeV, as shown in the schematic. Under the Bethe-Heitler theory, the two energies were supposed to be nearly equal in most cases.



Sources: Erich Bagge, *Fusion*, Nov.-Dec. 1985.

emerge from a 0.3 mm thick layer of lead. But I said to the Egyptian student, we will make a film out of gold—that is somewhat lighter than lead—as thin as is possible. We used 25 μm of gold, while the American was 330 μm of lead.

Thus completely beautiful pairs came out. First, El-Ela made his 4,000 pair photographs, and then he returned to Kiel in order to analyze them. In fact, in Kiel I had a machine for analyzing such cases, a sterocomperator. With that, the radii of curvature of positrons and electrons can be rather exactly measured.

One day, he came to me and was miserably depressed. He said, "I am standing between two stools. I am getting something that doesn't fit the theory *at all*." (He meant the Bethe-Heitler theory.) "I am getting far too many slow positrons and far too many energetic electrons" [see **Figure 2**].

So I answered, "For Heaven's sake, that is exactly what I wanted to prove! That is exactly what my theory predicts!" My theory predicted that the positrons must have a smaller energy—I knew that already, and that is very easy to explain. If the light quantum travels into the anti-world, so to speak, it has a large value for its effective cross-section with an electron of the anti-world if this electron has a small energy. And if it has a small energy, then after it has moved into the upper world, it remembers that it had a small energy. That means, the light quantum, so to speak, "fishes" out electron fish swimming on the surface of the anti-world, and therefore, the positrons that are produced as holes naturally have a smaller energy. That emerges automatically.

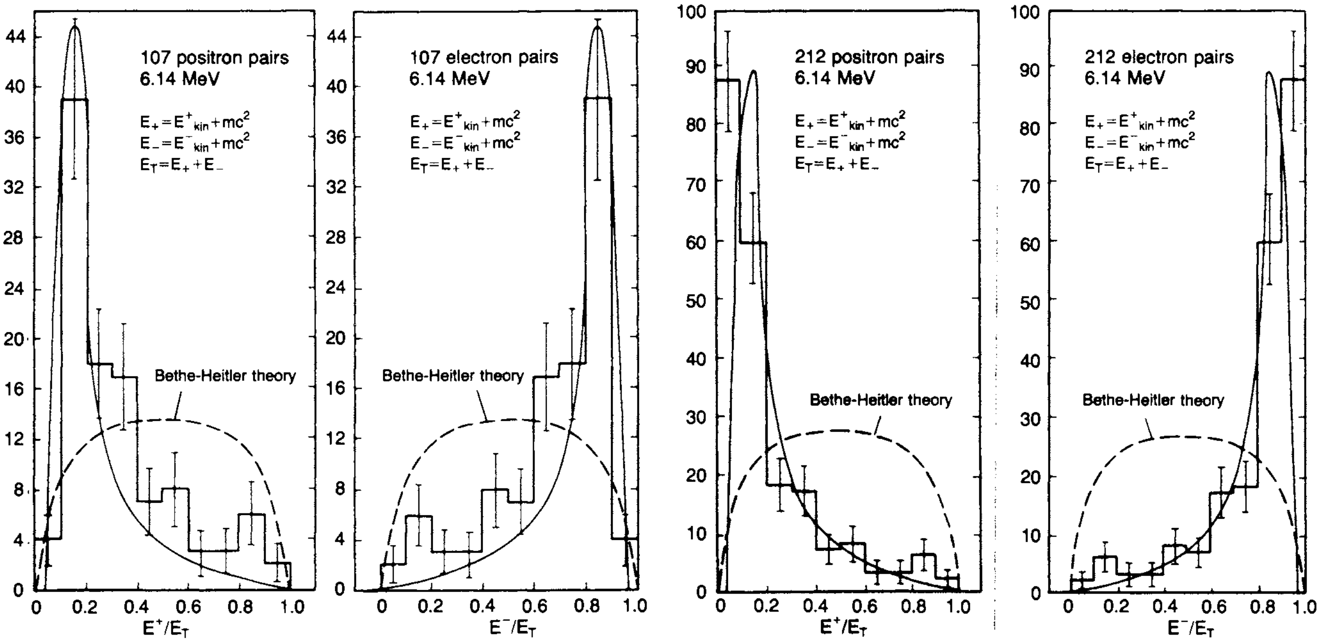
I was still doubtful because I thought, perhaps he's done something bogus. But it wasn't bogus, he had actually worked very cleanly. It was, however, my job as a physicist and head of the institute to pay attention to make sure that nothing stupid is submitted to me. So I said to him, "Work for a while on something else, and then after some time passes do a new analysis." He did that, but got the same result again. In the meantime, he had analyzed them so much that his measurements could now be summarized. They fit my theoretically expected curve rather well, not exactly, but well enough that we had to say that it's completely out of the question that the Bethe-Heitler theory is true.

There was, nevertheless, a small surplus at higher energies with regard to my theory. In this area, it didn't fit altogether well. But I had a degree conferred on El-Ela, and I told him that we had to see how we can continue the experiment. A few days before he left, he was still here with his wife. She was also a physicist and at least as clever in physics as he. She worked as an assistant at the University in Egypt. He told me that his wife still didn't have her doctorate and asked if she could continue the work. Naturally, he realized that the whole thing was still going well.

I replied, "Your wife should prove that it can't be otherwise. And for that, there is one possibility, that a portion of the positrons remain in the target because they have too small an energy, then fundamentally we're still getting too many quasi-Compton electrons since we see Compton electrons in the pictures as well as pairs, half and half approximately. We can now quasi-identify the pair electrons that appear as widowed pair electrons, because their positrons remain in the gold layer, since the authentic Compton electrons must have on average a greater energy. And that is what your wife

FIGURE 2

Energy spectra of pairs observed at Kiel Geesthacht



At left are the energy spectra positrons and electrons emitted by 6.13 MeV gamma quanta in a 25 μm-thick gold foil. The discrepancies between the theory and experiment with the energy-richer positrons and the energy-poorer electrons, can be ascribed to the fact that formation of pairs, by which the energy-poor positrons were retained within the gold foil, was found not to happen.

At right are corrected energy spectra of positrons and electrons emitted by 6.13 MeV gamma quanta in a 25 μm-thick gold foil. Their lines are in full agreement with a theory that the halving of energy and momentum is exactly satisfied by the mutual interaction of quanta with the anti-electrons of the Dirac sea. The discrepancies of the analog spectra in which the "widowed" electrons are not yet eliminated, are now gone.

In both figures, the histograms show the observed values. The solid lines show the values expected from the strict Dirac conception; the dotted lines show the distribution predicted by the Bethe-Heitler theory.

Source: *Fusion,* 1991, No. 1

should do. She should take the data from Geesthacht, leave out the positrons and measure the energies of *all* the electrons, the pair electrons and the Compton electrons together."

His wife then did fantastically well. First she had remeasured some of the pairs, to see if Abu El-Ela had measured correctly, and she got practically the same as he. She remeasured almost the all of the group that he had, because she got so good and could measure quickly, even better than he. Finally, she measured the electrons quite by themselves. And what emerged?

We could now consider: If a quantum comes along and knocks an electron out of a gold atom, it could only give its full energy to the Compton electron if it flies off in a straight line. This Compton electron must have the full energy of 6.3 MeV. Pair electrons, in contrast, that fly around as widowed electrons, could have only approximately 5.7 MeV. That must be so because the positrons take away some energy, even something more than their rest energy of 0.511 MeV. In brief, the widowed pair electrons always have at least 500-600,000 less electron volts of energy than the Compton electrons.

The electron measurements by Mrs. El-Ela produced a double peak. She measured the electrons with a margin of error of between 20,000 and 50,000 electron volts, and got a wonderful peak at 6.3 MeV. The curve then fell off, climbed again, to then form a second maximum at approximately 5.4 MeV. I could have hugged Mrs. El-Ela! What she measured was wonderful! After a few months' pause—she was working in the meantime on questions in theoretical physics—she analyzed the material again. And once again she got the double peak.

That was the decisive proof that my theory was working. Now we could distinguish the pair electrons from the Compton electrons. The valley between the two energy peaks was so deep that they could be easily separated. Mrs. El-Ela had simply taken the widowed pair electrons, which could not be distinguished from the Compton electrons, and combined them with the pairs that her husband measured. Accordingly, the frequency distribution agreed with my theory so well that it really couldn't have been better! All that is now in my book.

Mrs. El-Ela also received her doctorate, and returned to Egypt. She took her measurement data with her, but left the pictures here, and we finally analyzed a part of them again with an improved electronic procedure. Mrs. El-Ela knew nothing of that, and we merely asked her to send up the corresponding values once more. I sat at my table in Kiel with her values, and compared them with the new measurements that had been told to me by telephone from Geesthacht. Although none of the Egyptian measurements fit exactly with the new, when the given error range was considered, then they fit in every case within their margin of error. That was the best control for Mrs. El-Ela's work!

After that, so I thought, people will have to believe our work. It is the best that has been done in this area up to this point.

That proved that we really need the conception of the Diracian anti-world. At least, it shows that this anti-world cannot simply be ignored. If calculations are made as though the anti-world exists, then we get something right. If calculations are made as if it doesn't exist, then we get something wrong. I can't say any more than that.

Q: What should be done further at this point?

Bagge: We made measurements at 6 MeV. What I would like to have done is to determine the energy dependence of the energy deficit of pair production. That must be measured at higher energies, in accelerators. For example, the American experiment done by Fowler, Delsasso, and Lauritsen must be done again. They had quanta of 17 MeV, and they could measure the whole thing with a thinner layer, perhaps gold layers of 20 or 15 μm thickness. Then we would be quite certain that we will get the positrons.

And measurements should be done along the entire energy spectrum. Please consider the following: Today, there is hardly any work done on the large accelerators in which there are not huge energy differences between what is observed and what is really expected theoretically. The energy deficits we measured at 6 MeV could play a role in that. Other physicists must now make measurements that involve the relevant possibilities for that. Fowler et al., for example, could do that immediately. However, there is one thing they must do: They should not fill their Wilson chamber with air, because that has too great a braking effect; they must fill it, as I did, with helium. I did that from the well-thought-out reason that braking of positrons and electrons in helium is less by a factor of four than in the air. And the factor of four makes a great deal of difference for the precision of the measurements. The experimenters in America simply must do this experiment again.