THE STRENGTH OF THE WEAK INTERACTIONS

Lincoln Wolfenstein

Department of Physics, Carnegie Mellon University, Pittsburgh, Pennsylvania 15213; email: lincolnw@andrew.cmu.edu

Key Words  weak interactions, neutrinos, CP violation, nuclear weapons

PACS Codes  01.60.+q, 14.60.Pq, 11.30.Er, 13.25.Hw

Abstract  A career devoted to the study of weak interactions and fundamental symmetries is summarized. Subjects include the induced pseudoscalar coupling in muon capture, the hypothesis of a superweak interaction, the oscillation of neutrinos in matter, and a parameterization of the CKM matrix of particular importance for B physics. Also discussed are the origin of the Aspen Center for Physics and activities related to the dangers of nuclear weapons.

CONTENTS

INTRODUCTION .................................................... 1
SYMMETRIES FOUND AND LOST: C, P, AND T .................. 4
CP VIOLATION AND THE SUPERWEAK INTERACTION .......... 6
ASPEN CENTER FOR PHYSICS ................................ 9
THE NEUTRINO, WEAKEST OF ALL ......................... 10
AMBULANCE CHASING AND OTHER DIVERSIONS .......... 11
ON TEACHING PHYSICS AND THE METHODOLOGY OF SCIENCE 12
BEYOND THEORETICAL PHYSICS ............................ 14
COLLEAGUES AND STUDENTS ............................... 15

INTRODUCTION

I came to the University of Chicago in the fall of 1940 uncertain as to whether I should study physics or economics. My ninth-grade science teacher had suggested that physics was not for me because I was not good in the lab; he had never heard of a theoretical physicist. After one course in economics I declared myself a physics major.

With the onset of World War II there were no vacations, and I finished with a master’s degree in the spring of 1944. My thesis with Marcel Schein concerned the spatial distribution of large atmospheric cosmic ray showers (1), which members of his group were studying at Echo Lake. My main memory is that Schein kept...
trying to correct my spelling of “spatial,” changing it to “special.” My slide rule calculations were very crude; I knew nothing of the elegant formulation carried out by Moliere because it was published in the Zeitschrift für Physik, which, of course, was unavailable.

My war work was carried out back in my hometown of Cleveland, Ohio, at the laboratory of the National Advisory Committee on Aeronautics (NACA, later converted to NASA). I was analyzing the flow of air through the compressor and turbine of a jet engine. I knew practically nothing about hydrodynamics, but neither did the aeronautical engineers I was working with. We sat together and studied the classic textbook of Prandtl and Tietjens; I actually had an advantage since I knew vector calculus. The results of my work were finalized by Chung-Hwa Wu after I left and published as a NACA Technical Note.

At the end of 1945, with the war over, I was eager to return to graduate work. I was accepted by Harvard, Columbia, Cornell, Chicago, and Caltech, although the letter from Caltech warned of the housing shortage and suggested my wife might find a job as a servant so that she and I could occupy the servants’ quarters. The best offer was a DuPont fellowship at Chicago and so I returned there in the fall of 1946. It was now very different from before; the leading professors were Enrico Fermi and Edward Teller, who had come from Los Alamos. Fermi, of course, had been there in 1942, but I had been totally unaware of his presence or of the first nuclear reactor he was building under the stands of Stagg Field. As I tell my students, I used to walk right past the place where the reactor was being built on my way to lunch at the Ellis Co-op; had I known, at least I would have walked on the other side of the street. In 1946 there was also a brilliant group of new students: Murph Goldberger and Geoff Chew, who convinced Fermi to take them on as theory students; Owen Chamberlain, Al Wattenberg, and Jack Steinberger, who did experimental work with Fermi; and Frank Yang, who was really a student of Frank Yang.

I ended up working with Edward Teller on nuclear reactions involving polarized protons. At that time no such experiments had been done, but it was hoped that such experiments could elucidate the spin dependence of nuclear forces. During the summer of 1947, Bob Sachs came to spend the summer at Chicago while Teller was away at Los Alamos. He had written a paper with Eisner on the angular distribution of reactions involving particles with spin for the case in which all the particles were unpolarized. Because I was interested in extending this to the case of polarized particles, I studied it carefully and found that the proof was incomplete; the result was a short paper with Sachs completing the proof (2). I had many interactions with Bob Sachs over the next 50 years, but that was our only paper together.

Although most of my work involved reactions of protons on complex nuclei, toward the end I looked at proton-proton and neutron-proton scattering. Using phase shifts that had been numerically computed based on the tensor force, I calculated the left-right asymmetry due to the polarization of the incident beam. In April of 1948, I presented this result in a 10-minute talk at the American Physical Society meeting in Washington, DC. Julian Schwinger, who was chairing the
session, commented at the end that, of course, my results were wrong since there was no polarization effect if the only spin dependence was the tensor force. Back in Chicago, I found that although the effect obviously vanished in the first Born approximation, it was nonzero in the next order. Because I had used numerical phase shifts, presumably these second-order effects were included.

In Washington I also met Fred Seitz, who offered me a position at Carnegie Tech in Pittsburgh. I still had to prepare the final draft of my thesis, and in December of 1948 I returned to Chicago for my thesis defense before a committee of Teller, Fermi, and Gregor Wentzel.

Fred Seitz left after my first year at Carnegie Tech and Ed Creutz became the department chair. A major activity was the construction of a 450 MeV synchrocyclotron in Saxonburg, about 20 miles from campus. Once it started operation, I used to ride out in the wagon once a week for seminars and discussions concerning the experiments on nucleon and pion scattering and pion capture. Creutz’s chief theory adviser was Eugene Wigner. I gathered he was not too enthusiastic about my work when I received a letter from a small school in the west stating that Wigner had informed them I might be looking for a new job.

As it turned out, I remained in Pittsburgh from 1948 on except for various sabbaticals and leaves at CERN and various universities. In 1963 I received an offer of a professorship at Yale, but by that time I was settled with my family in a fine house not far from campus and New Haven did not entice me. My sister, a well-known psychologist, was shocked that I turned down an offer from a real university. In 1967 Carnegie Tech did change its name to Carnegie Mellon University, but I’m sure that did not change her opinion.

At Carnegie Tech a graduate student, Bernie Cohen, who was doing experiments at the University of Pittsburgh cyclotron, talked to me about the forward peak in some nuclear reactions. These reactions were thought to be described by the statistical theory of the compound nucleus, in which the compound state was independent of the incident particle and so naively the decay would be isotropic. I then wrote a paper (3) showing that the conservation of angular momentum could give a forward peak in the statistical theory, but the angular distribution would be symmetric about 90°. Much more work was done on this at MIT by Feshbach and his student Hauser (4). The real explanation of the forward peaks was given by the Butler stripping theory.

Much of my work continued to concern polarized protons, culminating in an Annual Review article in 1956 (5). Of particular interest were experiments measuring the dependence of the final polarization on the initial polarization. These were labeled “triple-scattering experiments” because one scattering was needed to polarize the initial beam and a third scattering to analyze the final polarization.

I presented preliminary calculations on triple scattering in a 10-minute talk at the Washington APS meeting in April 1954. Two months later, I was surprised to receive a letter from Clyde Wiegand, who had been in Washington and who worked with Chamberlain and Segré at the University of California Berkeley 184-inch cyclotron. His letter included experimental results that showed my
triple-scattering parameter gamma as a function of scattering angle for proton-proton scattering. By that time I had changed my notation from my preliminary work, so I wasn’t sure which parameter they had measured. As a result of this work, I was invited to Berkeley for the spring semester of 1955, where I gave a seminar course on polarization as well as sharing a large course on modern physics with Bob Karplus.

In 1957 I attended the Rochester High Energy Physics conference. These had been set up by Bob Marshak to concentrate on the physics at high energies in contrast to traditional nuclear physics. However, a small woman who had worked only in nuclear physics strode up to the microphone at the beginning of one session; what was she doing here? “I am here because of the strength of the weak interactions,” she explained. It was Madam C.S. Wu and she described her experiments that had discovered parity violation. From that time on, nearly all my research has concerned the weak interaction, which has provided a strong foundation for so much of elementary particle physics.

SYMMETRIES FOUND AND LOST: C, P, AND T

Discrete symmetries have played a major role in particle physics. I like to say that these symmetries were theoretical discoveries rather than postulates. The fundamental starting point was a postulated law of interaction, a Hamiltonian or a Lagrangian density. The original example in field theory was quantum electrodynamics (QED).

This is illustrated by my own encounter with charge conjugation symmetry $C$ in QED. Around 1950 Dick Drisko, a student of my colleague Bert Corben, was given the problem of calculating the decay rate of the singlet state of positronium into three photons. The normal decay of the singlet state was into two photons, but the triplet state had to go to three photons because the two-photon decay was forbidden by angular momentum (Yang’s theorem). The calculation of the rare singlet decay to three photons seemed like a good exercise for the student.

After some detailed calculations, Drisko returned with the answer: zero. Geoff Ravenhall, a postdoc at the time, and I thought there must be some symmetry at work and soon discovered that it was charge conjugation. The Dirac operator $C$ in the single-particle theory was well known, but it was not really understood that there was a unitary transformation $C$ under which QED was invariant. It was this idea we tried to explain and exemplify in our paper (6). We even presented an explicit representation of $C$ in terms of creation and annihilation operators, for which we were indebted to Gian-Carlo Wick, who derived this for us. At the end of our paper we considered the extension of this symmetry to the interactions of some new vector bosons that were being postulated at the time. We pointed out that if the interaction were vector ($V$), like that of the photon, then the new boson would have $C = -$, but if the interaction were axial vector, it would have $C = +$. We did not note that if the interaction were $V\cdot A$ then $C$ would be violated.
It was at around the same time that I became involved with time-reversal symmetry $T$. In my paper on proton polarization experiments, I had stated that two observables were equal: $(a)$ the polarization normal to the scattering plane of the outgoing proton when the initial proton was unpolarized and $(b)$ the left-right asymmetry of the outgoing proton when the initial proton was polarized. Although this was easy to show for the case of a spin-zero target, my colleague Julius Ashkin pointed out that it was far from obvious for the case of proton-proton scattering. Using different approaches, we both reached the conclusion that this result required the assumption of $T$ invariance.

We then wrote a nice paper on the subject. In general, there were 16 different possibilities for the dependence of the scattering on the two spin operators. Eight of these violated parity $P$, and three others violated $T$. Only one of the remaining possibilities was relevant for the two experiments mentioned above. To detect other operators required the triple-scattering experiments I came to write about later. At the same time as our paper, Dick Dalitz published a paper with the same result. Later, when symmetry violation was rampant, this equality was used to test the validity of $T$ in proton-proton scattering. In 1953 I took the occasion of a visit to Europe to meet Dalitz in Birmingham. My main memory is of a trip to Stratford with Dick in his beaten-up car. When we stopped for a traffic light, a policeman came up and looked over Dick’s car and said with a very straight face, “Have you reported this accident, sir?”

And then, in the summer of 1956, there appeared the suggestion by Lee & Yang that $P$ might be broken in the weak interactions. I remember the International Theoretical Physics meeting in Seattle in September 1956 where they spoke. Wigner was in the front row and did not like the idea at all. I also learned there, in conversations with Lee and Yang, about the two-component neutrino. Back in Pittsburgh an experimentalist from the University of Pittsburgh, Lorne Page, asked me about the polarization of beta electrons; somehow this consequence of $P$ violation was not discussed in the original paper of Lee & Yang. I did the elementary calculation showing the large possible longitudinal polarization, and Page started an experiment using positronium formation as a detector. We sent an abstract to the April 1957 meeting of APS. By the time of the meeting, $P$ violation had been established, and there were many experiments on the polarization of beta electrons.

In the fall of 1957 I took the first of my four sabbaticals at CERN. At that time CERN was just beginning, and when I arrived the theory group was located in temporary buildings at the airport. My attention was now focused on the weak interaction. I concentrated on the muon capture reaction, stimulated by lectures I had heard from Henry Primakoff. My first attempt was to calculate $P$-violating effects in muon capture by a proton; the results were not of much practical interest because I did not realize the importance of the hyperfine effect, which soon emerged in a paper by Bernstein, Lee, Yang, and Primakoff. While we all applied the universal $V-A$ theory to muon capture, I looked at the possibility of pion corrections. Originally Yukawa had attempted to explain weak interactions...
in terms of pion exchange. Once the pion couplings had been measured, it was possible to see how pion exchange worked for muon capture; this was done by Leite-Lopes (11), who found that the result was only $\sim 5\%$ of the measured rate. Nevertheless I realized that this pion exchange had to be taken into account; it produced an induced pseudoscalar interaction that interfered with the dominant $A$ interaction to reduce the rate by 20%. After struggling to make sure I had the correct sign and discussing the problem with Viki Weisskopf, then head of the theory division, I published my paper (12).

Back in Pittsburgh, my student Glenn Manacher and I looked at the pion-exchange effect in radiative muon capture, where it is possible to get closer to the pion pole. I reported these results at the 1960 Rochester conference, and Manacher carried out a detailed analysis in his 150-page thesis, which was never digested for publication. Two years later I was asked to review a paper submitted by Opat (13), a student of Primakoff, with much the same results as Manacher’s. I recommended that Opat cite Manacher’s thesis, but that because the thesis was not generally available and hard to read, this paper should be published. In later analyses of muon capture data it became customary to assume the standard $V-A$ coupling and then deduce the pseudoscalar $G_p$ coupling from the data. I thought this was misleading (14) because, in the absence of new physics, $G_p$ was determined very accurately from the pion-pole calculation. The radiative muon capture in hydrogen was carried out only recently with the result (15) that the derived $G_p$ was almost 50% too high. If this result is correct, it would be a sign of new physics that might contribute effectively to $V$, $A$, or $P$. Unfortunately this difficult experiment may not be repeated in the near future.

CP VIOLATION AND THE SUPERWEAK INTERACTION

The summer of 1964 was a very busy time for me. Wilma and I were getting ready to leave in early September for a year in Geneva with our four children. The fourth, Mimi, was not due until a month before we were to leave. Nevertheless a paper by me entitled “Violation of $CP$ Invariance and the Possibility of Very Weak Interactions” arrived at Physical Review Letters on August 31 (16). Despite an unenthusiastic referee report, it was published. So both Mimi and the superweak theory were born that summer.

The discovery of a small violation of the $CP$ symmetry was announced in the spring of 1964. The result was not particularly welcome given the success of the standard $V-A$ theory. A variety of theoretical possibilities emerged. My own attack on the problem took off from a paper by Bob Sachs. He suggested that $CP$ violation occurred in the decay amplitude for $\Delta Q = -\Delta S$ semileptonic decays and that by combining these with the normal decays one got a $CP$-violating $\Delta S = 2$ term in the mass matrix. I did not like this idea. In the first place, the evidence for such $\Delta Q = -\Delta S$ decays was unconvincing and indeed proved to be wrong. More important were technical objections to Sachs’ calculations.
Instead I considered the possibility of a very small $\Delta Q = -\Delta S$ current coupled directly to the standard $\Delta Q = \Delta S$ current. This would yield an effective $CP$-violating $\Delta S = 2$ term that was very weak and no significant $CP$ violation with $\Delta S = 1$. The specific origin of such a very weak interaction didn’t matter for its consequences. All $CP$ violation would depend on a single parameter that described the mixing of the $CP$-even state into $K_L$. One consequence mentioned was the lepton asymmetry in $K_L$ decay.

During my stay at CERN, I filled a notebook with ideas about $CP$ violation. Some of these were later summarized in an article I wrote for *Nuovo Cimento* (17). In January T.D. Lee arrived at CERN for a meeting; he brought with him a draft of a paper with the authors listed as Lee and me. He explained that he had come up with the superweak idea, but when he was writing his paper, Gian-Carlo Wick had informed him of my paper. Thus he thought this more general presentation of the idea should be coauthored by us. My first reaction was that he should publish his paper; I had already published mine. However, during the week he was at CERN, we had a number of discussions modifying various parts of the paper, and so a joint paper was actually published (18). Unfortunately my files do not contain the original paper he brought, so I cannot compare the original and final versions.

The way to disprove the superweak theory was to identify $CP$ violation in the decay amplitude. This could be done by observing a difference in the $CP$ violation in the $\pi^0\pi^0$ decay channel as compared to $\pi^+\pi^-$. This difference was given by the parameter christened by Yang & Wu (19) as $\varepsilon'$. In 1967 an experiment at CERN showed a large difference, so at a meeting in Moscow in the winter of 1968 I announced the death of the superweak theory (20). It turned out that the experiment was incorrect and the death announcement premature. It took 35 years after 1964 to get agreement between experiments at CERN and Fermilab on a nonzero value of order $4 \times 10^{-6}$. Although this showed that the original superweak idea was wrong, it was still possible that all $CP$ violation was due to some very weak new physics, given the very small value.

When the spontaneously broken gauge theory became the standard model in 1973 it was written in terms of four quarks; charm hadn’t been discovered yet, but the GIM (Glashow-Iliopoulos-Maiani) mechanism (21) required it to exist. In this form the theory was $CP$ invariant and so failed to explain the $CP$ violation in $K$ decay. I think one reason people didn’t worry much about this was because $CP$ violation could be explained by some new physics at a higher mass scale that would yield the superweak interaction.

A variety of papers considered ways to introduce $CP$ violation into this new standard model. Of particular interest to me were the Weinberg proposal of three Higgs bosons (22) and Lee’s discussion of spontaneous $CP$ violation (23). It seemed fitting to put $CP$ violation in the Higgs sector, which was the most mysterious piece of the model. Over the years I have written various papers about $CP$ violation in the Higgs sector, some with my student Jiang Liu (24) and others later with postdoc Yue-Liang Wu (25).
However, already in 1973 one little-read paper suggested in one paragraph that if there were six quarks instead of four, then CP violation was possible in the quark mixing (26). I only became aware of this idea several years later, when I refereed a paper from CERN that discussed the consequences of this model (27). With the discovery of evidence for a fifth quark b around 1979, this became the standard Kobayashi-Maskawa model for CP violation.

In this model, CP violation is due to one phase in the $3 \times 3$ CKM (Cabibbo-Kobayashi-Maskawa) matrix that takes the three down quarks into the three up quarks in the weak interaction. The key to the phenomenology of this model became apparent to me with the determination of the unexpectedly long lifetime of the $B$ meson. I realized that the CKM matrix had a hierarchical structure, and I wrote a parameterization in powers of the sine of the Cabibbo angle, which I labeled $\lambda$. The CP violation entered only at order $\lambda^3$ (28). This has become a quite standard way of writing the CKM matrix.

An important consequence was that there were two elements $V_{ub}$ and $V_{td}$ that would have large phases, and as a result there could be large CP violation in the $B$ system. The first thing you could measure would be the phase of $V_{td}$ because this would show up in $B-\bar{B}$ mixing. This is the quantity now called $\sin 2\beta$, which has been measured by BABAR and BELLE; its large value is the first semiquantitative verification of the CKM model. I labeled this the $\varepsilon$ parameter for the $B$ system because it was an effect of mixing and so could still be blamed on something like a superweak theory. The next thing to be measured would be the phase of $V_{ub}$, which you would measure by detecting the phase (now called $\gamma$) in a $b \rightarrow u$ decay such as $b \rightarrow \pi \pi$. I called this the $\varepsilon'$ experiment for the $B$ system. I wrote my conclusions about $B$ physics in the fall of 1983 in Santa Barbara, but the paper got lost somewhere and only got published almost a year later (29).

Even though people had struggled for more than 30 years to measure $\varepsilon'$ for the $K$ system, few appreciated the importance of what I called $\varepsilon'$ for $B$. Therefore, almost 20 years later, I wrote another little paper with a student emphasizing this (30). Demonstrating that the phase $\gamma$ was not zero, even if you didn’t measure it very well, would be the final death for anything like a superweak theory.

The determination of the magnitude of the element $V_{cb}$ required a theoretical calculation of the semileptonic $B$ decay to charm. This was done in a paper by Isgur, Grinstein, and Wise in the quark model (31). I wondered how dependent this was on the quark model and realized that the important point was that the overlap between the initial $B$ and the final $D$ or $D^*$ should be very close to unity at the zero recoil point. I wrote a paper based on this with my student Altomari (32). This had the germ of what became heavy quark effective theory. We also noted that a form factor that Isgur et al. had set to zero could be very important. Both of our papers were rejected by Physical Review for various reasons, but summaries appeared as Letters (33).

A question that has concerned me for many years is that of what I call “long distance effects” that are omitted in typical quark calculations. One example (34) was the very small mass difference calculated for the $D-D^*$ system, which resulted from delicate cancellations in quark model calculations. Another was the result
for the leptonic decay asymmetry in the $B$–$\bar{B}$ system. Bjorken had similar doubts, and although we did not completely agree, we managed to collaborate on a paper about this (35). Of particular interest for many years has been the strong final-state phase shifts in exclusive hadronic $B$ decays (36). Unfortunately, in all these cases, the best I could do was to say that everybody else was wrong; I didn’t have the answer either.

ASPEN CENTER FOR PHYSICS

In 1961, my colleague Michel Baranger and I discussed the possibility of having a place where theoretical physicists could get together in the summer. Most of us had research grants that gave us summer salaries to do research and it would be very profitable for such research if we could spend time with physicists from other institutions. Michel discussed this with Mike Cohen at the University of Pennsylvania, who had a similar idea.

George Stranahan, a graduate student working with Dick Cutkosky, suggested Aspen would be a good place; he had contact with Robert Craig, the director of the Aspen Institute for Humanistic Studies, who was favorable to the idea of having an associated science component. Furthermore, George’s family had a foundation that could help fund our first building, now known as Stranahan Hall. And so the Aspen Center for Physics was born.

For the summer of 1962, we wrote to all our friends and a total of 42 physicists came during the summer. I drove out with my wife and four children, and we settled into a large house at Fourth and Francis just a short distance from the new center. In our large living room we hosted a string quartet concert and a talk by Jim Farmer, the head of the Committee on Racial Equality. Houses rented for about $1000 for the whole summer.

Little did we imagine that 40 years later there would be 500 physicists during the summer and that $1000 would cover two weeks’ rent. By 2002 our fine house on Fourth and Francis had been torn down to be replaced by a still bigger one, and the hillside where we had seen sheep in 1962 was covered with condos.

For many years the Center was run on a shoestring; the National Science Foundation was very unwilling to give support. Gradually the value and importance of the Center was recognized, and for many years now support from the NSF has been coming, although the new buildings have depended on private and foundation support. Fermilab was attracted to Aspen for its Program Advisory Committee meetings, which were in June, and so for a number of summers I started at one end of town and then went to the other.

I have spent many summer weeks in Aspen, and many of the physicists I know throughout the world I first met there. In 1972 I met Manny Paschos; we were both thinking about the detection of the neutral currents predicted in the new Weinberg-Salam gauge theory of weak interactions. Together we found a nice formula relating the reactions of neutrinos with those of antineutrinos (37). The Paschos-Wolfenstein relation is still used in the analysis of data (38).
10 WOLFENSTEIN

It was at Aspen that I started a collaboration with Ernest Henley on $P$ violation in certain nuclear reactions. We then started working on a book together that would be a second volume to one he had written with Hans Frauenfelder; like so many book projects, it never was completed.

In 1978 Murray Gell-Mann, Pierre Ramond, and Dick Slansky were discussing the question of neutrino mass in the $SO(10)$ grand unified theory. They came up with the idea of the seesaw formula, which I found very interesting, and I invited Dick to come to Pittsburgh and give a talk about it. Somehow the three of them never managed to write a paper about it, but it became famous anyway. That may have started my long interest in the origin of neutrino mass.

THE NEUTRINO, WEAKEST OF ALL

In August of 1973 I arrived at CERN for my third one-year stay. The big news was the Gargamelle evidence for weak neutral-current events initiated by neutrinos. There were many discussions concerning the validity of the evidence, and these were enlivened by messages from Carlo Rubbia asserting there were no neutral currents at Fermilab.

When it became clear that neutral currents were here to stay, I became very interested in whether they really were the neutral currents predicted by the spontaneously broken gauge theory of Weinberg and Salam. One particular question intrigued me: Were the neutrino interactions really diagonal in flavor? How could you tell that the neutrino you didn’t detect coming out had the same flavor as the incoming neutrino? The answer I found was that if you followed the beam a long distance in matter, there would be a gradual transformation of the beam to a new flavor if the interaction changed flavor. This would be analogous to the phenomenon of optical activity for light, in which the plane of polarization was rotated. The key concept was the index of refraction for neutrinos.

I actually applied this idea to neutrinos going through the Earth. For such neutrinos, if the neutral current changed a mu neutrino into a tau neutrino, on average the emerging mu neutrino flux would be cut in half (39). This is exactly what was found 20 years later by the SuperKamiokande experiment, but analysis of the energy dependence shows this is not due to off-diagonal neutral currents.

I also applied this idea to the problem of the deficiency of electron neutrinos from the sun. After writing up this solution of the solar neutrino problem, I discovered in discussions with people, particularly my former student Daniel Wyler, that I had left something out. There was also a contribution to the index of refraction diagonal in flavor for electron neutrinos as a result of the charged-current scattering. The mixing due to the off-diagonal neutral current was therefore suppressed, and this was not a good solution to the solar neutrino problem. However, this led to a quite new consideration, which formed the second part of the paper I eventually published (40). Assume there are no off-diagonal neutral currents but there are neutrino oscillations due to neutrino mass and mixing, as discussed by Pontecorvo and others. For neutrinos going through a large amount of matter, the mixing of electron neutrinos can be greatly suppressed or greatly enhanced by the index
of refraction. In thinking of the application to the sun, I focused on the vacuum oscillation example of Gribov & Pontecorvo (41), in which the oscillation length was comparable to the Earth-sun distance. In this case, the only effect of matter was to suppress the oscillation inside the sun so that oscillations began at the surface. It was almost seven years later that I received a letter from Mikhaeyev & Smirnov in the Soviet Union, describing their application of my paper to solving the solar neutrino problem (42). It took me a while to understand it, but I did suggest in my reply the application of the adiabatic approximation. Their emphasis was on the “resonant enhancement” of the mixing, that a small mixing angle could become large in matter. My original paper had shown that, for constant density, such an enhancement would occur for a particular energy. What was new was that, in the case of continuously varying density, such an enhancement could occur for a range of energies. This was labeled the MSW (Mikhaeyev-Smirnov-Wolfenstein) effect by Peter Rosen. It now appears that the “large-mixing-angle MSW effect” is the correct solution to the solar neutrino deficit. In this case the enhancement is not as dramatic as originally envisioned; if the matter effect is negligible (which is true for the lowest-energy solar neutrinos), one gets the vacuum survival probability of about 0.6, whereas when the matter effect is important (for the $^8$Be neutrinos), the survival probability is about 0.3.

I naturally became interested in the origins of neutrino masses and mixings. For Zee’s model of Majorana mass (43), I calculated the pattern of masses and mixings (44). Using these results I calculated the probability of neutrinoless double beta decay. The answer was zero.

The calculation involved a weighted sum over the mass eigenvalues, and I realized that the cancellations were due to the fact that one of the eigenstates had the opposite CP eigenvalue from that of the others (45). Majorana neutrinos are CP eigenstates in the absence of CP violation. This also proved interesting in an analysis of possible radiative neutrino decay carried out by my student Palash Pal (46).

I was visiting John Bahcall in Princeton in February 1987 when news of the supernova neutrinos arrived. It was one of those thrilling moments you never forget. Later I visited Michigan and they showed me the circles from the Cherenkov cones in IMB (the Irvine Michigan Brookhaven Experiment); they didn’t look much like circles to me, but they were the best you could expect from a 20 MeV electron. Clearly supernovae were a wonderful place for matter effects on neutrino oscillations, and I couldn’t resist writing a little letter about it (47), although Terry Walker and Dave Schramm had been more clever and written their paper in 1986 (48).

AMBULANCE CHASING AND OTHER DIVERSSIONS

Theoretical physicists spend a certain amount of their time rushing to find possible explanations for anomalous experimental results that are very preliminary. I refer to this as “ambulance chasing,” an attempt to profit (in terms of publication numbers) from what turns out to be an accidental result.
I have done my share of this activity. In 1972 it appeared that the decay rate for \( K_L \) to \( \mu^+\mu^- \) was anomalously low; in fact, it was lower than the calculated rate from the absorptive part of \( K_L \) to two gammas followed by \( \gamma-\gamma \) to \( \mu^+\mu^- \). To get some destructive interference, G.V. Dass and I invented a \( CP \)-odd new interaction. Later the experimental result increased and the problem disappeared.

In 1976 K.V.L. Sarma arrived in Pittsburgh for a visit with news of some strange events observed in a detector in the Kolar gold mine. Sarma had been a postdoc at Carnegie Mellon working with Dick Cutkosky, and we had remained friends. We concocted various scenarios with new particles and discussed their consequences, but the Kolar gold mine results were never duplicated in later experiments.

My most recent example concerned the first observations of \( CP \) violation in \( B \) decays by the BABAR experiment. The parameter \( \sin 2\beta \) appeared to have a value lower than that allowed by the standard model. Within a few days, my former student João Silva and I produced a paper discussing the implications of this result. Our paper and two others on the same subject appeared in the Los Alamos preprint collection less than a week after the result. As more data accumulated, \( \sin 2\beta \) settled down to a value in perfect agreement with the standard model.

In 1973 the Cambridge Electron Accelerator (CEA) gave results indicating that the hadronic cross section in \( e^+e^- \) annihilation was continuously rising in the region of 4 to 6 GeV. In retrospect this was obviously the onset of charm production, which was required by the standard model. However, the CEA was shut down before any further experiments. In 1974 at CERN, Gabriel Karl, Nicola Cabibbo, and I considered the implications of a continuously rising cross section and a possible unitarity bound on this. It was quite irrelevant, but it was fun working with these two very nice fellow theorists. In 1974, on a Monday morning in November, news arrived from SLAC of the discovery of a very narrow resonance from \( e^+e^- \) annihilation. The strange feature was that its decay into hadrons by strong interactions was only about six times greater than its electromagnetic decay. This led me to consider the possibility that this was a light weak vector boson. Of course it soon became clear that this indeed was the \( c\bar{c} \) bound state.

Looking over my list of publications, I find that only \(~5\%\) are such wild goose chases. I never tried to explain cold fusion or monojets. Very few weird results have proven true; perhaps the weirdest that did was \( CP \) violation, which I still pursue after all these years.

ON TEACHING PHYSICS AND THE METHODOLOGY OF SCIENCE

I never enjoyed teaching the introductory physics course for engineers and scientists. The emphasis in the text and in the tests was on solving a set of numerical problems. Naturally the students spent their time learning formulas and methods of guessing which formula fit which problem. That was not physics to me.
I always think of Tom Kuhn’s comment (49) that science was taught in the schools like religion. If we teach the students that they must believe what we tell them or otherwise they will get a bad grade, then we cannot compete with the church, which tells them that if they do not believe they will go to Hell.

I chose instead to teach a physics course for nonscience students. Here I can choose the topics to cover and emphasize how we came to believe the theories of modern physics, all of them counterintuitive. I start with the struggles of Kepler as described in Arthur Koestler’s *The Sleepwalkers* (50) and the wonder of Galileo as he looked through the telescope. I try to engage them in dialogue concerning what to believe, given the evidence of that time. I remember one girl raising her hand and saying “Please, teacher, just tell us what to believe.”

It was hard to find a suitable text. The one I liked best was Gerry Holton’s *Concepts and Theories in Physical Science* (51), but it covered too much material. In my course I tried to jump from Newton’s theory of planetary orbits to Bohr and quantum mechanics and in later years to Weinberg’s “first three minutes” (51a). The major problem, as I confessed to the students, was that each of these theories was the result of 50 to 100 years of struggle and they had three or four weeks for each.

I also tried a course on “science and society” for science students. It began with the nature of scientific knowledge, using books such as those of Kuhn (49) and Ziman (52). Not many students were interested. I think professional philosophers have given the philosophy of science a bad name. A chapter in Steve Weinberg’s book *Dreams of a Final Theory* is entitled “Against Philosophy,” although in fact his book is a wonderful discussion of the philosophy of science (53).

As I look at my own time as a physicist, I see again and again the struggle to accept new theories. When I began, the elementary particles were the proton, the neutron, the photon, the electron, and possibly the neutrino. Now the proton and neutron are no longer elementary and the “cast of characters” has grown to at least 16. It was very hard to accept the quarks as elementary particles, since they never existed freely. Could it be they were pseudoparticles like phonons? But then it would seem one would require a still more complex substructure for the nucleon. As I worked on various weak processes, I found that the only way I could describe them consistently was in terms of quarks; I had no choice but to believe in them.

For years cosmology seemed to me pure speculation. Then came the accidental discovery of the microwave background. As I introduce cosmology into my course, I emphasize that we look backward in time with the working hypothesis that the same laws of physics held in the past. I do not try to go much before primordial nucleosynthesis; I always explain that the name “big bang” was coined by Freddy Hoyle as a term of derision. With respect to attempts to find a theory of initial features that determine the present universe, I take a lesson (54) from Kepler’s attempt to find a theory of five planets with particular spacing. We now believe our solar system derives its features from the details of the chaos from which it formed, just as countless other stars with their planets originated. The details of our particular universe too may derive from some original chaos. Thus it may well
be that there is no particle physics explanation of the baryon asymmetry of the universe, and I remain skeptical about inflation.

BEYOND THEORETICAL PHYSICS

I never worked on nuclear weapons. My files contain telegrams from 1944 from mysterious places that I now recognize as Oak Ridge and Los Alamos suggesting I might work there, but by luck or chance I didn’t. I have visited friends in Los Alamos, but I have never set foot in the Laboratory.

The danger of nuclear weapons and the possibilities for arms control have been important issues for me. I feel that scientists have a responsibility to educate the public and the Congress on these issues. As a result, I have belonged to a number of organizations and written countless letters to newspapers. As an outsider with no special knowledge, I realized my influence was small. However, I never envied the insiders; as I wrote in an article about Oppenheimer (55), “his story serves to illustrate the tragedy of the scientist in the nuclear age, who finds himself compromising ethical principles if he wishes to play a role in the vital decisions concerning nuclear weapons; the nuclear scientist, as much as anyone and perhaps even more than others, finds himself a stranger and afraid in this new world of his own making.”

In 1961 physicists from the University of Pittsburgh and Carnegie Tech formed the Pittsburgh Study Group for Nuclear Information, which issued a report on the “Effects of Nuclear War on the Pittsburgh Area.” Its major purpose was to demonstrate the uselessness of proposed civil defense measures. A similar motivation led to the arguments against the Anti-Ballistic Missile Treaty in the late 1960s and Star Wars in the 1980s. A paradox I felt needed emphasis was the danger of nuclear weapons on the one hand but their uselessness on the other; I summarized this in a 1990 article I called “The Real Zero Option,” which was published as “Nuclear Addiction” (56).

In the late 1960s I became national secretary of the Federation of American Scientists (FAS). At that time there was no central office, and the activity was concentrated in the chapters. We appointed Jeremy Stone as the Executive Director in charge of a Washington, DC office. This grew and became very effective, although one consequence was the disappearance of the local chapters.

I also gave a variety of courses on nuclear weapons issues. I used as one text Brighter Than a Thousand Suns, a history of the nuclear era up to 1957 by the German pacifist Robert Jungk. This book was very controversial because it seemed to imply that the German physicists might have been more ethical because they didn’t make the bomb. I gave the students copies of reviews, a vitriolic denunciation by Ed Condon and a more nuanced analysis by Hans Bethe. For a number of years I gave the course in collaboration with different professors from the Humanities and Social Sciences College. My last collaborator was a former U.S. ambassador, James Goodby, who had worked for the State Department on a variety of arms control issues and who was briefly on the Carnegie Mellon faculty.
I have also been concerned about environmental issues and critical of economics professors who have a very probusiness point of view. In an article in the faculty newspaper, Focus, entitled “The Yellow Brick Wizards” (the business school was yellow brick), I wrote, “One day while walking home through Schenley Park I met one of the wisest of the Wizards...and asked what economists thought about issues like pollution. Those he explained were ‘externalities’...if I understand the word it means that pollution is external to the equations the Wizards use...In my experience when I leave a major term out of an equation I get the wrong answer.”

I also expressed my concern over the university’s ties to the Department of Defense (DOD), and particularly over the establishment of a big DOD center called the Software Engineering Institute. In a letter to the local paper, The Pittsburgh Post Gazette, I wrote, “The university should serve as a center for critical analysis of the arms race and as a catalyst for efforts to end it; instead Carnegie Mellon has chosen to become a participant.” Shortly afterward, I was surprised one day by a call from actor Charles Haid, who identified himself as Renko from Hill Street Blues. He and other former Carnegie Mellon drama students had been invited to a gala celebration for the university, but they were concerned about this DOD center and had been referred to me by a newspaper reporter. They decided to come to campus and gave a press conference explaining why they refused to participate in the celebration.

COLLEAGUES AND STUDENTS

During my 55 years in Pittsburgh, I have had many very fine colleagues. Among the theorists were Gian-Carlo Wick, who arrived in 1951 as a refugee from the University of California loyalty oath and left for Brookhaven in 1957; Walter Kohn, who arrived as a nuclear theorist but left as a condensed matter theorist, although—as I wrote him when he won the Nobel Prize—it turned out he was a “closet chemist”; and Leonard Kisslinger, who replaced Michel Baranger in nuclear theory and who proved a stalwart member of our department. There were also some whom I first met when they were undergraduates: Jim Langer, who eventually left for Santa Barbara; Ray Sorensen; and Dick Cutkosky, who was my main theoretical colleague until his death in 1993.

Of all my colleagues, the most wonderful was Julius Ashkin. He was a superb physicist, both theoretical and experimental. He was an elegant teacher; I still have notes from his courses in fading dittoed copies, and Barry Holstein gives much credit to these notes in his textbook on quantum mechanics (57). Above all he was a great human being. I only wrote the one paper with him, but I discussed much of my physics work with him. His early death at the age of 61 was a blow to us all.

My former students are now distributed around the world, and I try to stay in contact with many of them. A few excellent students, like Jiang Liu and João Soares, left physics after productive years as postdocs, but most of the best students continue to work in physics. Darwin Chang is a leading theorist in Taiwan, professor at Tsing-Hua University. Palash Pal, who coauthored a very good book
on neutrino mass with Rabi Mohapatra (58), is a professor in Calcutta. Lalit Sehgal has been for years a senior researcher at the Technische Hochschule in Aachen. Daniel Wyler is a professor in Zurich. João Silva, with whom I continue to collaborate, is a professor in Lisbon and coauthored with two former postdocs, Gustavo Branco and Luis Lavoura, a textbook on CP violation. Barry Holstein helped to form a very strong theory group at the University of Massachusetts in Amherst; in this group is his former student John Donoghue, who counts as one of my “grandstudents.” I think I have learned as much from all my students as they have learned from me, and so I would like to dedicate this review to them.

The Annual Review of Nuclear and Particle Science is online at http://nucl.annualreviews.org

LITERATURE CITED

3. Wolfenstein L. Phys. Rev. 82:690 (1951)
55. Wolfenstein L. *Dissent*. Jan.–Feb.:81 (1968)
## CONTENTS

**FRONTISPIECE, Lincoln Wolfenstein**  
xi

**THE STRENGTH OF THE WEAK INTERACTIONS, Lincoln Wolfenstein**  
1

**THE SOLAR \textit{hep} PROCESS, Kuniharu Kubodera and Tae-Sun Park**  
19

**TRACING NOBLE GAS RADIONUCLIDES IN THE ENVIRONMENT, Philippe Collon, Walter Kutschera, and Zheng-Tian Lu**  
39

**THE Gerasimov-Drell-Hearn Sum Rule and the Spin Structure of the Nucleon, Dieter Drechsel and Lothar Tiator**  
69

**THE THEORETICAL PREDICTION FOR THE MUON ANOMALOUS MAGNETIC MOMENT, Michel Davier and William J. Marciano**  
115

**THE BROOKHAVEN MUON ANOMALOUS MAGNETIC MOMENT EXPERIMENT, David W. Hertzog and William M. Morse**  
141

**THE NUCLEAR STRUCTURE OF HEAVY-ACTINIDE AND TRANSACTINIDE NUCLEI, M. Leino and F.P. Heßberger**  
175

**ELECTROMAGNETIC FORM FACTORS OF THE NUCLEON AND COMPTON SCATTERING, Charles Earl Hyde-Wright and Kees de Jager**  
217

**PHYSICS OPPORTUNITIES WITH A TeV LINEAR COLLIDER, Sally Dawson and Mark Oreglia**  
269

**DIRECT DETECTION OF DARK MATTER, Richard J. Gaitskell**  
315

**BACKGROUND TO SENSITIVE EXPERIMENTS UNDERGROUND, Joseph A. Formaggio and C.J. Martoff**  
361

**GENERALIZED PARTON DISTRIBUTIONS, Xiangdong Ji**  
413

**HEAVY QUARKS ON THE LATTICE, Shoji Hashimoto and Tetsuya Onogi**  
451

**THE GRIBOV CONCEPTION OF QUANTUM CHROMODYNAMICS, Yuri L. Dokshitzer and Dmitri E. Kharzeev**  
487

**GRAVITATIONAL WAVE ASTRONOMY, Jordan B. Camp and Neil J. Cornish**  
525
CONTENTS

INDEXES
Cumulative Index of Contributing Authors, Volumes 45–54 579
Cumulative Index of Chapter Titles, Volumes 45–54 582

ERRATA
An online log of corrections to Annual Review of Nuclear and Particle Science chapters may be found at http://nucl.annualreviews.org/errata.shtml