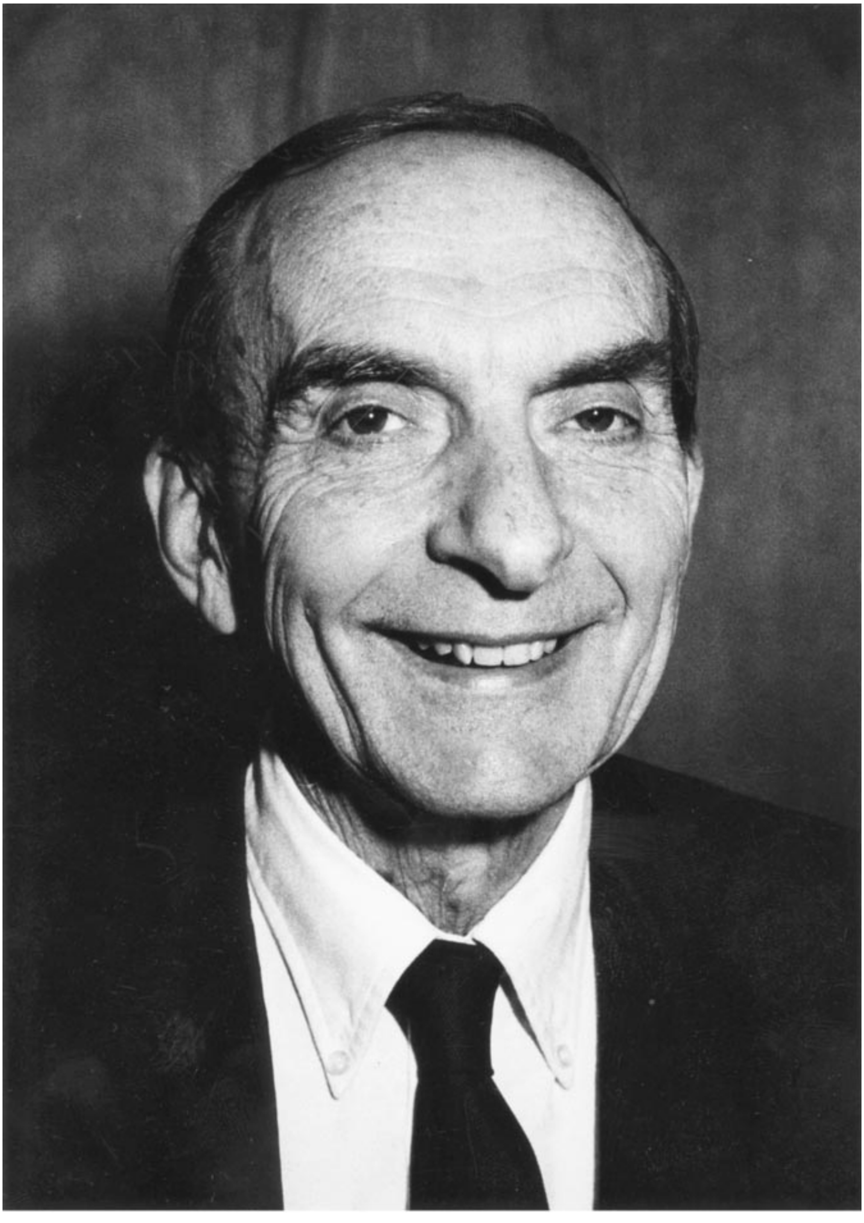

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FLY WITH EAGLES

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■ **Abstract** My training in many areas of research in theoretical physics derived from what I learned from the "eagles" I flew with. Let me enumerate them. First of all, when the Navy sent me to the University of Wisconsin in January 1944 to become an electrical engineering officer, I met Gregory Breit, who practically adopted me as a son. I learned from him to drag a problem bleeding through the street until it cried for help and gave up. My political indiscretions during my young life forced me to flee to England from Joe McCarthy, where I ended up in the inspiring theory group of Rudi Peierls. Peierls taught us to drive immediately to fundamentals. When I began collaborating with Hans Bethe, the first thing I learned was why he had never had long-term collaborators. I had to wait until he was more than 70 years old in order to have any chance of keeping up with him. He worked like a bulldozer, heading directly for the light at the end of the tunnel. Most important is confidence. He starts each day with a pile of white paper in the upper left-hand corner of his desk and fills it with calculations at a more or less even rate, although he's happy to stop for lunch. I found this to be an amazingly effective procedure to imitate. From my training with Rudi Peierls, his closest friend, I was well prepared to work with Hans. The twenty-odd years I've collaborated with him have been exciting and productive.

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EARLY LIFE

I was born in Brookings, South Dakota on July 22, 1926. My father had come to South Dakota State College in 1899, a year after obtaining his PhD in mathematics from the University of Chicago. He had grown up on a farm in Missouri and his brothers remained farmers. At age 17, he had been sent to the University of Missouri with the plan that he should spend one year training to become an accountant. He turned out to be good at mathematics, and one thing led to another. At South Dakota State College, he was Dean of General Science by the time I came along. Dean Brown's family were expected to excel in the Brookings schools. Teachers who had been there a long time called me Cecil, after my oldest half-brother (and half-cousin). Those who had been there a shorter time called me George, my $4\frac{1}{2}$-year-older brother. Growing up as a son of Dean Brown, a model of propriety, was highly constraining.

Our closest family friends, the Huttons and the Humes in the department of agriculture, were German-oriented. The Huttons had immigrated from Germany. Dr. Hume had studied animal husbandry in Germany. I developed an affinity for Germans that stayed with me all my life. The children of the Huttons and Humes pretty well matched in age those in my family, except for me. I was much younger, and my older siblings claimed that I was spoiled and indulged.

When I was six years old, I asked Dr. Hume to his amusement, "What is the Universe?" I've been trying to find an answer to that question all my life.

Brookings was a town of five thousand people, peaceful and pretty boring, especially in the summer when most of the college students were away. My six older brothers and sisters had all taken their bachelors degrees at South Dakota State College (both of my brothers went on to PhDs in chemistry from Brown University), but I was determined to go elsewhere and saved money through part-time jobs in order to do so.

The war was on, and in the summer of 1943, just before I became 17, I quit high school and started college, taking accelerated courses with the cadets of the Army Special Training Program. In the next months I tried to enlist in the V12 navy training program, which sent those accepted to universities for special training. One time I failed the physical because of flat feet and an overbite. The second time I failed for being underweight and color blind. Learning that I was color blind turned out to be most useful.

In the autumn of 1943, I applied to enter the Universities of Minnesota and Iowa. Turned down by the former because I had not completed high school, I naturally accepted the offer of Professor George Washington Carver to study at the University of Iowa, and I went there for the spring term of 1944. Professor Carver had been interested in the study of color and had the selfsame Japanese tests for color blindness that I had failed when applying to join the Navy. These consisted of 50 circles filled with extended dots of various colors, spelling out numbers. In a typical one, a color blind person would see a 6, whereas one who could distinguish colors would complete it to make an 8. I was determined to never again fail a test for color blindness, so I took the booklet of color plates home with me and memorized the number I should say when I saw another one. This was not difficult, because when I saw a 6 it was clear that it could only be completed to an 8. The possibilities were limited. Also, since I was only marginally color blind, I found that I could train my eyes.

Armed mentally, I made the trolley trip to the Cedar Rapids Navy Office and took the Eddy test to qualify as a recruit for the naval radio technician program. Two weeks later I returned to hear the results of the test. The officer in charge informed me that today was the last day of recruiting and that I would have to enlist immediately. The alternative was to wait to be drafted into the Army. I had the foresight to get my parents' written permission, so I signed up. With the clothes I had on and the books I was studying, I went immediately to Ames, Iowa, for a physical exam. That same evening I was off to boot camp at the Great Lakes Naval Training Center north of Chicago with folders of 120-some other recruits. Since I had passed the Eddy test, I was a first-class seaman and they were only lowly apprentice seamen. Therefore, I was in charge of the group. That night I phoned my mother: "Mom, I'm in the Navy." (See Figure 1.)

Boot camp was hard, because I was put on so many 0–4 AM watches that my eyes actually focused in different directions when I opened them at night. I had

Figure 1 The author at age 17 upon enlistment in the Navy.

trouble sleeping the short time before or after the watch. And I was on KP[1] for a long time. But somehow it ended. Before the end we were given a chance to take a written exam for officers' training. I must have done very well on the exam (I knew the answers to all the questions) because they hustled me through the physical—the one for all naval officers including aviators—with all my physical defects (except that I no longer tested color blind), and I was sent to the University of Wisconsin to be trained as an electrical engineering officer.

I found engineering easy. Later we were allowed to take physics courses, which I did. In the autumn term of 1944 I took Gregory Breit's graduate course on theoretical physics. Preceding his lecture I had an hour of calisthenics on the top floor of the armory across the campus. Physically shaking, I slid down the banister, struggled up Bascom Hill and down the other side, and, five or ten minutes late, flung my peacoat and book bag on a chair in Gregory's course. This was a course with meat in it! It was also difficult. I got a 42 (out of 100) on the final, but then found out that I'd done second best in the class, the top grade going to Johnny Powell, who had had some experience before.

GREGORY BREIT

Gregory was lonely, as almost all the male students and many of the college professors were away in the war effort. In the early days of the nuclear war effort Gregory had been in charge of clearance. I gathered that his idea of clearance was to keep all the classified research sent to him and not give it out. In any case he was not later taken into the program, but he consulted on ordnance for the Naval Training Laboratory at Aberdeen.

He conducted a weekly seminar in theoretical physics but had a difficult time finding speakers, so I volunteered. He gave me the Pinocchio papers (by M Conversi, E Pancini & O Piccioni), which analyzed muons, some of which were stopped in iron. I remember that one of the main points was that the muons could be bent by the much larger field **B** in the iron magnets, rather than **H**. In any case, Gregory instructed me patiently about the papers. When the seminar came, he prompted me, in order to bring out the main points he had taught me. Others at the seminar mistakenly thought he was criticizing my presentation and afterwards said to me, "Boy, you sure told Gregory!" In any case, Gregory took a great interest in my training, and I began to get an idea of what research was like.

With the atomic bomb, the war was over and we were given the option of remaining in officers' training, getting a degree and commission. But in that case we would have to stay in the Navy for a certain number of years. I was not eager to make my career in the Navy and shipped out. After some weeks at a transit center in Chicago Pier I was sent to Bremerton, Washington, to participate in mothballing ships. Since I was officially back in the radio technician program, my

[1]Kitchen Police.

main duties were connected with the radio shack, scraping paint and repainting with rust-resistant paint. Because very few regular Navy men were in the radio technician program, we were frozen in, and I was not demobilized until a year after the war ended. Quite naturally, I went back to Wisconsin to work with Gregory.

I had seen no difficult side of Gregory while in the Navy—he had always been very kind to me. He had a canoe, and in September 1946 he invited a German student and me to go canoeing with him across Lake Mendota. First he put the German boy in front. Gregory was small but powerful, and the man in back steers the boat. He paddled hard, keeping the boat turning, and the German boy couldn't keep it straight. As soon as we reached the other side of the lake, the German boy remembered he had an appointment, leaped out of the boat and took a bus back. I'd seen what Gregory had been doing and muttered imprecations to myself as I took over. The calisthenics involved in officers' training had been pretty tough. Also we'd been out rowing whale boats at 6:00 in the morning during that time. With all my might, I could keep Gregory from turning the boat. I felt that he had new respect for me. He said nothing as we carefully stowed his boat away. Many years later, after he had retired from Yale and was living in Buffalo, we were talking together on a Saturday afternoon. (He was not happy about his situation in Buffalo and often phoned me to complain about it.) That day he asked, "Do you remember when I took you canoeing in Wisconsin?" I said yes. "I knew you had some stuff in you then." This was the only time we ever discussed the canoe trip.

Gregory was offered a professorship at Yale, beginning January 1947. Shortly thereafter he came down to my desk in the cellar and asked whether I would like to go to Yale with him. I said, "Yes." He asked, "Don't you want to think about it?" I answered, "What is there to think about?" Wisconsin would have no one of his caliber to work with. So a whole gang of us, Gregory and his students, tripped off to Yale in January 1947.

Of course Yale University was all very grand, but there was really no one of Gregory's stature in the physics department and I was disappointed in the graduate courses. Gregory had organized most of his students into computational projects, especially computing Coulomb wave functions for nuclear scattering processes. I was not enamored of punching Monroematics, the hand computers of that time, and I told him so. Probably others of his students shared my feeling but didn't dare to tell him. He asked, "How can I pay you if you don't work?" I replied, "You're not paying me. I'm on the GI bill." He thought for some time and then told me that he had a very good problem for me, to give a relativistic description of the proton as well as electron motion in hydrogen, using his 16-component equation. The only trouble was that I had not yet taken a course in quantum mechanics. He suggested that I begin by reading Dirac's 1928 paper deriving the Dirac equation. As often in my research, I learned to manipulate quantities before I really understood them, and when I did take a course in quantum mechanics I derived the Pauli equation as a nonrelativistic approximation to the Dirac equation.

I wrote four lovely papers with Gregory on the effects of nuclear motion on the fine and hyperfine structure of hydrogen and on the effect of the finite size of the proton. Of course, nobody knew then what the size was, so we parameterized it. On my own, I derived a new virial theorem for the Dirac equation and included it in my thesis without telling Gregory about it. He came rushing up to my office one night after reading my thesis, surprised by that last paper, and immediately said that I must publish it in the *Proceedings of the National Academy of Science*. He had published a similar relation there and he would submit it.

An activity during my Yale years that I will not discuss much here, chiefly because it also concerns other people, was my short-lived membership in the Communist Party, chiefly a group of graduate students. I had been the Yale student representative to the Progressive Party Congress in Chicago, which nominated Henry Wallace as a third-party candidate. Violence, going on all about me, to the Communist Party simply egged me on to join them and my closest friendships at Yale were with other members, who were very idealistic. During a long hot summer when they were all gone, I, by default, gave the report of our student branch to the Connecticut state Communist Party convention. Yugoslavia had been expelled from the Comintern and our student branch had questioned how the communist parties of twenty-odd countries could change their views overnight from a virtuous to a dastardly Yugoslav government. I voiced our concern. The representative from the national Communist Party spent a good share of his summary talk in castigating me, and I found myself expelled from the party for, as I remember, left-wing deviationism.

I did not want to give information to the FBI about other people, let alone myself, so I concluded that it was time to get out of the country. This was late 1949 and my GI bill support ran until the next summer, when I would receive my PhD. I wrapped up my thesis and announced its defense. In one of the classrooms on the upper floor of Sloane Physics Lab, I filled the blackboards on all four sides with my calculations using Breit's 16-component two-body relativistic equation. Professors other than Gregory stuck their heads in and then disappeared, realizing that they would not understand my thesis work. Thus I had a comfortable defense with Gregory and his students; the latter did not understand my work but were compelled to attend.

I wrote to various universities in England, figuring that I should go to a country where I could speak the language. Max Born replied from Edinburgh that he was getting too old to supervise research. PMS Blackett wrote from Manchester that they had no room. I received a three-penny folded airmail return from Rudi Peierls in Birmingham, who answered, "Come ahead." So off I went. I was lucky to squeeze by because in 1950, preceding McCarthy's entrance on the scene that summer, Mrs. Shipley at the State Department denied all applicants with left-wing backgrounds passports to travel aboard. This was, of course, quite illegal, but stopped only in 1957, as I describe later.

February 4, 1950, I arrived in Southampton on the USS America. The customs officer greeted me. I showed him the letter of invitation from Peierls and he said, "I hope you won't try to stay more than one year. I fancy we have enough theoretical physicists in England." Then began a wonderful ten years during which I was brought into the extended Peierls family and I learned from Rudi his way of doing theoretical physics. I could never do it as carefully and accurately as he wanted but his training prepared me well for his closest friend, Hans Bethe. Peierls' autobiography, in which I figure chiefly because of my gardening skills, is entitled *Bird of Passage*. He was my second eagle, personally completely different from Gregory Breit, but one of the best physicists of our century.

RUDOLPH PEIERLS

Eugen Roth wrote in one of his poems, "Der Mensch schaut in die Zeit zurück und sieht, sein Unglück war sein Glück."[2] While my Yale friends were being thrown out of their jobs for past political indiscretions, I was settled in the most stimulating theoretical group in the world. I didn't even have the burden of making decisions about personal problems. Genia Peierls did that for everyone in the group. I gratefully accepted her advice. I could do research the way I wanted to learn how to do it, and I had brilliant colleagues. Jens Lindhard was visiting from Copenhagen for the year and found me "digs" in the same house where he boarded, a reasonable walk from the university.

I had been intrigued by the fact that Breit's relativistic two-body interaction had limitations. He had found out that it should be used only as an expectation value, not to higher order. In fact, he had found out that when it was used to second order, terms of order α^3 appeared in the fine structure of helium, which did not agree with experiment. I was determined to have a go at deriving a relativistic two-body equation from quantum electrodynamics. Freeman Dyson was a postdoc with Peierls in 1950 and he encouraged me to try. Geoff Ravenhall had just completed his thesis with Peierls. In expanding the electron-electron interaction in powers of the retardation, he had found a new term. It was different from the Breit interaction and I did not believe it. The climate for attacking the problem was right; I had interested experts around.

I was able to derive a Breit-like two-body relativistic equation by first eliminating the scalar and longitudinal photons in favor of the instantaneous Coulomb interaction. Using pair theory for the rest of the interaction, Geoff and I found that Breit's troublesome α^3 terms disappeared and resurfaced only at α^4 order once the energies of the virtual pairs were included in the denominators in perturbation theory. Then we came back to the Breit equation and stared in disbelief. Since it was only a two-body equation, negative energy states of the electrons were empty. Along with Peierls, we asked what would happen if two electrons interacted, one dropping into a negative energy state and the other going off with positive energy so that the summed energy was that of the initial state. We decided that the wave function would quickly "go down the drain." This might appear to be a useless

[2]The man looks back in time and sees that what he thought was his ill fortune was actually his luck.

effect, because it was clear that the negative energy electron states are filled, i.e., that pair theory must be used. However, in many-body calculations it is difficult to incorporate pair theory. On the other hand, we realized that in Hartree-Fock calculations of atoms, only one electron at a time is allowed to change state, so that our predicted catastrophe would not occur in such calculations.

Indeed, only nearly 25 years later, when Larry Wilets and others started to improve atomic Hartree-Fock calculations by including two-body correlations, did it happen. The wave functions started disappearing! Joe Sucher, professor in particle physics at the University of Maryland, realized what was happening. He published a *Physical Review Letter* (1), "Continuum Dissolution and the Relativistic Many-Body Problem," in 1985. He constructed a simple model to show that continuum dissolution occurs if the negative energy states are left empty, and remarked that the effect was first noted by Brown & Ravenhall in 1951 (2). Following Sucher, the continuum dissolution acquired the name Brown-Ravenhall disease. It turns out to be easy to block the continuum dissolution by introducing projection operators forbidding transitions to negative energy states. The correctly calculated effects from virtual pairs turn out to be small.

I went on to calculate the electron-electron interaction between two K-electrons in heavy atoms (3), finding to my surprise that the retardation was zero—the electrons were in stationary states!

Rayleigh scattering turned out to be a surprise, in fact such a big one that Peierls included my work in his book *Surprises in Theoretical Physics* (4). Bob Wilson at Cornell had measured (5) the coherent electron scattering at 1.33 MeV in ^{208}Pb and found it to be below Bethe's impulse approximation calculation at backward angles. He attributed this to the presence of Delbrück scattering, 180° out of phase with the Rayleigh scattering.

In a paper with Woodward (6), I showed that impulse approximation got worse with increasing gamma-ray energy, rather than better. The point was that momentum had to be picked up from somewhere in order to scatter the gamma rays. If from the atomic wave function, as in impulse approximation, the amplitude would go as $(\Delta q)^{-3}$ because of the asymptotic behavior of the atomic wave function. The momentum could, however, be picked up from the Coulomb potential of the nucleons at a cost of $(\Delta q)^{-2}$, so that the latter would predominate for large values of the momentum transfer Δq.

This meant, as we knew then, that the main contributions to Rayleigh scattering came from closer and closer to the nucleus, as $(\Delta q)^2$ became larger. This phenomenon was rediscovered many years later and was called color transparency, although it had little to do with color. It resulted simply from smaller and smaller parts of the wave function becoming important as the magnitude of the momentum transfer was increased.

With Peierls and other collaborators, I then calculated the elastic scattering of gamma rays (Rayleigh scattering) at a series of energies covering those of then easily available natural sources, taking the Coulomb interaction into account to all orders in $Z\alpha$; i.e., using the Furry representation. Initially, I hitchhiked across the country to Cambridge to do the calculations Friday night through Sunday on their first electronic computer, EDSAC I, which stored numbers to the base 10 as traveling blips in mercury tubes. We programmed digitally on punched tape. The total storage of the machine was 1024 bytes, with $\sim$150 used for initial instructions. All in all, it was about as good as an inexpensive programmable hand computer these days, but sufficient to carry out my calculations.

From my experience with the Furry representation in Rayleigh scattering, I derived a method for calculating the Lamb shift in heavy atoms. This was outlined in a 1959 paper (7) with Jim Langer, now past-President of the American Physical Society, and Glen Schaefer, now deceased. Our technical difficulty was in removing the divergences in a manifestly noncovariant situation so that the finite terms we calculated were meaningful. The method was correct and was used by several investigators for many years, but my own numerical calculations with collaborators were inaccurate and probably somewhat in error. Other investigators set these right much later.

Why did I work on quantum electrodynamics and atomic physics when I could have learned so much condensed matter physics from Peierls? I had set myself a series of questions to answer from my graduate work, and Peierls encouraged me to work in these fields, especially after Dyson left for the United States, in order to keep his group broad. However, with the Lamb shift I had answered those questions and I changed to nuclear physics, chiefly because I planned to go on sabbatical to Copenhagen, where nuclear physics was then sovereign.

A small problem I had in contemplating Copenhagen was that I did not possess a US passport. Initially, in 1949, I had received a passport just a few months before Joe McCarthy came on the scene. My passport was valid only for England and only for one year. When I turned it in for renewal after one year, the American authorities kept it. This caused me problems because the British wanted to stamp it yearly when they gave me permission to live in the country. The net result was that I had neither a passport nor permission to stay in England for several years. How did I manage to stay there anyway? I spent every Saturday morning planning strategies, which I tried out on Rudi Peierls, appealing my case all along the line to American authorities and then writing to the British Home Office, telling them what I had done. They always replied, asking me to let them know the outcome of my appeal, signing the letter, "Your humble servant . . . ." No one in the British Home Office really wanted to make a decision. Finally, the Home Office asked me to reapply for my US passport and tell them the results. Peierls thought this was a reasonable request, so I did so in 1955. The British Consulate in Birmingham forwarded me the request from Washington that I submit an affidavit about my activities in the Communist Party. Peierls thought that the British Home Office would consider this a reasonable request, so I prepared the affidavit, with great care because it was a sworn statement. Some time later a request came back for another affidavit supplying names of everyone I had known in the Communist Party. Even though I had long been expelled, many of the members I had known were highly idealistic people. I didn't wish to involve others. I told the US Consul in Birmingham that such an affidavit, like the first, was a very serious matter and I needed to consider it carefully; could he please give me a copy of the request? Since the matter was highly sensitive, he banished his British secretary from his office and copied the request by hand for me. I took it back and gave it to Peierls to lock up in his safe. I knew that the British would never expel me from the country for refusing to comply with such a request. Indeed, Secretary Burton of the University wrote to the British Home Office, saying that my request for permission to live in England had been in limbo for several years and demanding that a decision be taken. After some time, I was granted leave permission. In 1956 Dr. Nathan, friend of Einstein and executor of his will, had carried a case similar to mine through the US Supreme Court—he had been denied a passport. The Court ruled that the State Department could not deny passports to US citizens. So when I reapplied I got my passport back and I could go on sabbatical to Copenhagen in 1958.

My sabbatical was somewhat hard on me, in that I had begun working in nuclear physics in a standard many-body approach I'd learned in Birmingham. The work of Aage Bohr and Ben Mottelson was near its peak, terribly successful in reproducing and predicting experiments. Some of the connections I had begun looking for came with Phil Elliot's work showing that collective rotations could be described by configuration mixing of single-particle orbitals organized in a nuclear $SU(3)$.

Back in Birmingham, I was promoted to Senior Lecturer and on to Reader. Peierls had tried to promote me to full professor in 1959, but the powers that be insisted that I go up through the ranks. The promotion to full professor was to come in 1960. But by then I had a competing offer, a letter from Niels Bohr offering me a professorship in the Nordic Institute for Theoretical Physics (NORDITA).

In the summer of 1959, which I spent at the University of Minnesota, I was asked to give the theoretical talk at the Gordon Photonuclear Conference because the theoretical "guru," Joe Levinger, was unable to attend. Along with Mark Bolsterli, I made the schematic model of the giant dipole state in nuclei (8). The chief problem of the photonuclear people had been how to get the giant dipole state up to the high experimental energy. Mark and I constructed it from single-particle excitations, basically as the plasma oscillation in many-body physics is constructed. But we had to use shell-model wave functions. As chief theorist at the photonuclear meeting, I was allotted an hour. I worked through the schematic model in 10 minutes. The audience sat hushed. Everyone realized that we'd solved the main problem, and we recessed for the remainder of the hour, most of us going for a walk. This work gave me an appetite for constructing collective motion out of single-particle excitations. By that time Landau's papers on Fermi liquid theory had appeared in the West, and these gave a beautiful formalism for constructing many-body theory. The problem faced by nuclear physicists was to employ the shell-model orbitals.

I moved to Copenhagen in the summer of 1960. It was quite natural that I should take up the problem of constructing the nucleon-nucleon interaction to be used between nucleons in shell-model states. Copenhagen was at its zenith in nuclear physics, with the Bohr-Mottelson collective model explaining more and more nuclear phenomena, especially excitations, and more and more data flowing in daily. It seemed that all data flowed to Copenhagen.

Before leaving I want to talk more about the Peierls' "school." Descriptions of the Peierls' Birmingham school of physics during the 1950s, written by those who participated in it, stress the stimulation in research and the family feeling created by the Peierls, especially by Genia Peierls, who was a dynamic phenomenon indescribable by mere words. After I had spent 1950 shivering in British "digs" with Jens Lindhard, the Peierls took me into their house as boarder. The house was a gigantic 12-room building that the Peierls could only have bought because of the British leasehold, i.e., it reverted back to the trust that owned it in $\sim$40 years. My room was diagonally across from the dining room and upstairs. Genia with her loud voice would simply call me for breakfast across the entire house, starting the walls rattling. Genia loved parties, keeping the entire group of theoretical physicists telling their stories or playing charades until 2:00 or 3:00 in the morning, long after buses stopped. As her team guessed each correct word, helped by an elaborate set of gestures from their member operating from the enemy camp, Genia would erupt with deafening screams of delight.

When Rudi came home from the University at night she questioned him about every detail of every conversation he had had with anyone during the day. When such a conversation had concerned a personal problem, he would usually came back to the relevant person the next day and say, "I've thought more about the matter we discussed yesterday. . ." and then convey Mrs. Peierls' advice. On the weekend Genia would phone the wives of group members and advise them as to what they should do to cope with the many problems they encountered in Birmingham. We were all poor, and shivering in the inadequate heating, but we were happy and the group dynamics produced remarkable results.

When I went to Birmingham in 1950, Freeman Dyson was, as I said earlier, a postdoc there. Dick Dalitz, Wladek Swiatecki, and Geoff Ravenhall had just finished their PhDs. The entire group, including graduate students, was only a dozen people. Many of the Europeans went back and became professors in their home countries. Among the Birmingham PhDs prominent in America are Jim Langer, Elliott Lieb, and Stanley Mandelstam.

Those of us on the staff did undergraduate teaching, but the strong part of the Birmingham teaching were the courses for graduate students, postdocs, and staff. Lectures would be given throughout the year, often by Peierls, but initially by Dyson when I arrived in Birmingham, covering different subfields of physics. They were designed so that in the three years a British student spent in graduate school he would be exposed to all the subfields of physics, except for general relativity, which was not popular in Birmingham. The result was that the "products" of the Peierls school were well versed in most of theoretical physics and were easily able to change subfields. Very few of them are now working in the field in which they were trained, but they are almost universally successful.

Rudi Peierls (Figure 2) was so informal and actually shy, letting Genia convey most of their feelings, that, although impressed by his intellect, I did not

Figure 2 The author with Rudi Peierls and Vikki Weisskopf at Peierls's retirement celebration, Oxford, 1979.

properly credit his contributions to physics. But I kept encountering them in my life following Birmingham.

In 1995 Ronny Peierls phoned me the night after his father died. He asked me to phone Hans Bethe. Rose Bethe answered the phone and I told her, but I was too overcome to continue. A bit later Hans phoned me back and we talked about Rudi for some time. In closing, Hans said sadly, "A big part of my life is gone—and yours too." Rudi Peierls not only taught us how to do physics, but he and Genia taught us how to interact and deal with people creatively and positively.

COPENHAGEN

Whereas Bohr and Mottelson had built up the extremely successful "Copenhagen school" in nuclear physics (the more famous prewar Copenhagen school still permeated the buildings of the Bohr Institute and NORDITA), I wanted to understand how the nucleon-nucleon interactions in the nucleus built up the structure and excitations. It was clear that this was to be a thankless job, because the schematic interactions, delta function plus quadrupole-quadrupole interaction, worked so well in describing nuclear phenomena that I knew I would get no credit for a "microscopic" derivation. Nonetheless, I wanted to give the work a more "fundamental" foundation.

Adherents of the Bohr-Mottelson school came from all over to revel, experimentalists to fit the data. I played "loyal opposition," questioning each *ad hoc* assumption, and added liveliness to the discussions. Our Monday morning meetings with the experimentalists became raucous when I questioned the way they connected their data. After one such session Phil Siemens said to me, “We had a very heated debate today, as heated as it could be with odds of 25 to 1!” Of course, I was the 1. In all fairness to Aage Bohr and Ben Mottelson, they did not resent my criticisms and often remarked on the positive role I played in questioning applications of their formalism.

Nuclear theory distinguished itself from most other many-body theory by the extremely strong repulsion felt by two nucleons at short distances, often taken to be an infinite hard-core interaction, which we now know to originate from the exchange of massive vector mesons. Moszkowski & Scott had proposed a delightfully simple way to handle the short-range interaction (9). That was to integrate the Schrödinger equation, with the wave function starting from zero at the hard-core radius, out to the radius at which the phase shift would be zero if the potential outside of it were thrown away. But instead of throwing it away, the long-range remaining piece of the interaction was to be used as the effective interaction. This “Moszkowski-Scott separation” worked well because nuclei and nuclear matter are rather dilute, so that if two particles are close together, a third particle is unlikely to be in the vicinity.

I set a Norwegian student, Kristofer Kolltveit, and a Finnish student, Alpo Kallio, to work on this problem. The Kallio-Kolltveit potential became extremely successful, considering its simplicity.

During a year of leave from NORDITA spent at MIT, 1962–1963, I shared the Feshbach/Weisskopf suite of offices with Murph Goldberger and Murray Gell-Mann (Feshbach and Weisskopf were away). Regge poles were giving way to quarks on the particle physics scene, but I was still working out the many-body theory of nuclear matter, realizing that although the two-body tensor force contributed a lot to the binding energy in second order, higher-order effects were small. MIT had a group of assistants who did computations on the electronic machines. I designed a Monte Carlo calculation of the total effects of the tensor force, in which the integrand went to ∞ at one point in a six-dimensional integral, although the integral was finite. My computer assistant managed to draw random numbers so as to hit very close to ∞; I explained carefully that she should begin again, starting with the last set of random numbers she drew. Then everything worked out.

Princeton offered me a professorship in 1963 and I decided to take it up in 1964. After all my earlier political troubles it seemed that moving to Princeton as a full professor was a fitting way to return to the United States. The lore in Princeton, as in Copenhagen, was that the nucleon-nucleon interaction in nuclei resulted from such strong interactions that it was impossible to work it out, starting from the elementary nucleon-nucleon interaction measured in scattering experiments. When I told Eugene Wigner, in response to his questions about what I planned to do, that I wanted to work out this interaction, he replied, “That would take someone cleverer than I.” I replied that I was certainly not as clever as he, and that probably by “working out the nucleon-nucleon interaction in nuclei” I meant a much less accurate calculation than he would envisage, but I was determined to do what I could do. Dawson, Walecka & Talmi (10) had handled the hard core in the nucleon-nucleon interaction by calculating the *t*-matrix (called *G*-matrix in nuclear physics) for the two-nucleon scattering and then using it as an effective interaction in nuclei. Igal Talmi was not happy with their results, because he knew that as one added neutrons to the $f_{7/2}$ shell in calcium, the term quadratic in the number of nucleons had to be repulsive, whereas their calculation gave it as attractive. I knew from Copenhagen that effects on the interaction from nucleons polarizing the nuclear medium were important. My first morning at Palmer Physical Laboratory, I listened to a suggestion that Ben Bayman, then assistant professor there, had proposed to George Bertsch, a young graduate student. I showed him that the idea probably wouldn't work out. George had sat listening, then in the middle of the discussion suddenly walked out without saying anything. I looked questioningly at Ben, who said, "George saw that it wouldn't work out, so why waste any more time on it?" The next morning George came to my office and asked for a problem. I proposed that he work out in ^{16}O the correction to the two-body interaction that came through polarization of the nucleus, to lowest order, essentially including the nucleon particle-hole bubble. This would be the lowest-order effect from the Bohr-Mottelson interaction via vibration exchange. The calculation involved quite a lot of angular momentum recoupling, which Ben Bayman or I would have done by group theory, so I estimated that George would be kept busy for months. To my astonishment he brought in the results the next Monday. The polarization correction had all the right properties to satisfy Igal Talmi—the quadratic term was repulsive—and later we showed that most of the Copenhagen quadrupole-quadrupole interaction came from this polarization bubble diagram.

The stage was then set for the full calculation of the nucleon-nucleon interaction in nuclei, the Kuo-Brown interaction (11). Tom Kuo had come as a postdoc to Princeton and had been advanced to assistant professor. He combined the ingredients of *G*-matrix, a clever treatment of the tensor interaction, and George Bertsch's polarization diagram in order to calculate the full in-medium interaction. We felt that the results were spectacular. Opinions of others were less flattering. Particle physicists considered this a messy business (and still do). Other nuclear physicists were largely skeptical and set about to calculate corrections. Bohr and Mottelson could not believe that we had a quantitative solution without including their vibrational induced interaction, the bubble sum, to all orders.

Many heated discussions followed. I leave the problem here, saying only that now whenever anyone in the world wants to make a complete quantitative calculation of nuclear properties or states starting from fundamental interactions, such as the Monte Carlo calculation at Caltech led by Steve Koonin, he or she contacts Tom Kuo to get the matrix elements of his effective interaction. He is fully occupied traveling all over the world, furnishing matrix elements to other investigators.

My family wanted to live in Copenhagen, rather than Princeton, and a new rule came that faculty at Princeton could only take leave every few years, except for government service. I pointed out that NORDITA was a cooperative institute involving five governments, but Princeton would make no exception for me. In 1967 CN Yang, who had moved from the Institute for Advanced Study to Stony Brook, came back to Princeton and asked me to go for a "walk in the woods" with him. He asked me to come to Stony Brook and set up a nuclear theory group there. I had come to know and respect Frank Yang at Princeton, so even though Stony Brook looked like a construction site at that time, I took his invitation seriously. I worked out a scheme in which to divide my time between Stony Brook and NORDITA, taking leave without pay from one institution while I was at the other. In 1968 I made the transition, taking my graduate students back and forth with me for the next 18 years until I retired from NORDITA. Nearly all of my many American graduate students acquired Danish wives in the process. I want now to elaborate on the themes I worked on, continuing over many years, describing my motivation, rather than simply enumerating projects.

Before doing this I should say that Stony Brook gave me the resources, including several faculty lines, to build up a nuclear theory institute. Frank Yang was helpful at every stage in my endeavors. I had learned a lot from Peierls and from Copenhagen about group dynamics in institutes, and I set about building up a productive group, with lots of visitors. I took along to Stony Brook Akito Arima, who later became president of Tokyo University; Tom Kuo; and Andy Jackson, now professor at the Bohr Institute in Copenhagen. Although my personal interaction with Andy was occasionally stormy, we never disagreed about what was good physics. He exerted "quality control" over our group effort and a number of times stopped real blunders in my publishing. He is one of the most erudite and, in particular, articulate people I know. He wrote the motivation for my honorary DSc from the University of Copenhagen. I would like to think that I actually had worked in the inspired, directed way he described, rather than bouncing from pillar to post in wrong starts before finding the right way.

Going back to my year 1962–1963 at MIT, I had had many discussions with Arthur Kerman about nucleon-nucleon interactions. He had fiercely maintained that there was no need in anything we explained for a meson presence in nuclei: Of course, no one doubted that meson exchange was the origin of the two-body potential, but up to that point nuclear physicists just took mostly static two-body interactions and then charged along. As I remember a panel discussion ending a Photonuclear Gordon conference, Bill Bertozzi challenged,[3] "You lousy theorists, you can't even calculate the $\sim$10% correction to the static interaction needed to explain the simplest nuclear interaction $n + p \to d + \gamma$."

There had been a calculation of exchange currents in the three-body system by Villars (12), but Kerman challenged me, "For any exchange current you come up with I can furnish you with two or three more which will do the same thing." These challenges remained in my mind during most of the next decade. During this time, my friend and beginning collaborator Mannque Rho applied work from

[3]My quotation is probably not right in detail.

current algebra by Steve Adler to give the unique lowest-order expression for the operator to be used for the exchange current (which physically meant catching the pion responsible for the interaction in midair). With John Durso, I successfully applied the chiral invariance first introduced by Mannque into nuclear physics to calculate the intermediate-range nucleon-nucleon interaction (13).

On my way to the Gull Lake Conference organized by Michigan State University (MSU) in 1971, I stayed over Labor Day in the Kellogg guest house on campus. Because it was a holiday, no breakfast was served at the guest house, so I trundled over to the nearby Red Barn for breakfast, bringing a pad of graph paper. While eating I realized that the so-called seagull term in pion exchange between the neutron and proton in the deuteron depended only on the interparticle distance r_{12}. I could easily draw it, with the help of my hand computer, on the paper and count the squares underneath. I immediately saw that it explained most of the discrepancy to form the deuteron. After I returned to Stony Brook, Dan-Olof Riska and I did the calculation properly (14). The meson presence in nuclei (although the deuteron isn't much of a nucleus) had been discovered, and this led to a flurry of activity, showing that some reactions that could be measured by the electron accelerators could be forced far off-shell, where the corrections became as large as the main quantity.

Teaming up with Mannque Rho, who furnished the expression from chiral invariance and taught us how to "single count" and the dynamics, Dan-Olof Riska, who was in close touch with experiments and experimenters, enabled the exchange current development. Both have been marvelous colleagues in many works in which I participated. Mannque will patiently listen as I make a "derivation" of a new quantity, often by devious means, such as dividing zero by zero along the way. Then he puts the idea into a rigorous formalism and we have a new paper. Dan-Olof grabs any new idea and runs with it; shortly afterwards I have a new paper on my desk with our names jointly on it. At least in my view, these papers all have a purpose, following within a grand design, although my high rate of citations comes about chiefly from authors disagreeing with new concepts that we introduced.

Introduction of chiral symmetry into nuclear physics led naturally from the exchange currents into chiral restoration. Mannque and I were impressed by the Bég-Shei theorem (15) that the nature of the symmetry realization (Wigner-Weyl vs Nambu-Goldstone) is irrelevant in discussing the short-distance symmetry (in the context of the Wilson expansion). In other words, chiral restoration should proceed smoothly, at least in terms of properties of particles, and hadron masses should, aside from those Goldstone bosons, go smoothly to zero with increasing scale, either density or temperature. Our "dropping mass" theory, introducing Brown/Rho scaling, was published in 1991 (16). The nucleon effective mass was always known to decrease with density, as clearly seen in Walecka mean field theory. More recently, we have found that with increasing density the vector mean field tends to decouple, so that the in-medium nucleon rest-mass energy $m_n^\star c^2$ also decreases.

The simplest confirmation of dropping vector meson masses should have been in quasielastic electron scattering. Our predictions (17) were that both ω- and ρ-meson masses should decrease $\sim$20% by going from $\rho = 0$ to ρ_0, nuclear matter density. In fact, the experiments are working out this way, but with complications. Luckily, before the linear accelerator in Saclay was turned off in 1990, data on positron scattering by ^{12}C and ^{208}Pb were taken at both forward and backward angles. This enabled comparison of the positron data with the electron scattering data (18), showing that the very simple effective momentum approximation was accurate. This was an old approximation in which the momentum of an electron or positron is shifted by $\pm Ze/\langle R\rangle$, where $\langle R\rangle$ in the denominator comes from an average of $\langle 1/r\rangle$. Much more complicated focusing corrections had been used earlier, but the combined electron and positron data make a convincing case that they were wrong and that the effective momentum approximation is adequate. Once the Saclay linear accelerator was shut off, most of the research workers employed there went to other machines and sorting out the above matters was left to Joseph Morgenstern. Although he was retired, he was allowed to continue to use the facilities. I am extremely glad that he stayed on and sorted matters out.

A strong case for a decrease in ρ-meson mass was made by the CERES experiments carried out at the SPS at CERN. In Pb + Pb collisions a hot and dense fireball was formed for a long enough time that a substantial number of the short-lived ρ-mesons could decay while still in the fireball. Because their masses were decreased by the high density and temperature, they decayed into e^+e^- pairs of lower energy, giving an excess of these in that region. This excess was measured by the detectors.

Perhaps the most interesting drop in effective masses is that of the K^- meson, where the main decrease occurs because of the movement toward restoration of the explicitly broken chiral symmetry with increasing density. Never mind the pretentious words; the simple fact is that the mass of the K^- meson decreases with increasing density. I'll return to this in discussing the work I did in astrophysics in collaboration with Hans Bethe.

HANS BETHE

I first met the Bethes in 1951, while boarding with the Peierlses. Hans and Rose Bethe could be easily put up in the voluminous Peierls house. Later, when I could go back to the United States, I always stopped at Cornell, where I benefited greatly from Hans's criticism of my theories. I always had lots of ideas, and chiefly needed someone to shoot down those that wouldn't work.

Not until many years later did I work with him. On April 1, 1978 we picked the Bethes up at the airport in Copenhagen. Hans had sprained his ankle on Mount Pion in Turkey, and I told him that that was a bad omen for his work on the pion-nuclear many-body problem, where each higher-order term calculated was as large as all of the lower-order terms put together. Hans asked, "What should we work on?" I replied, "Let's work out the theory of supernovae." He said that he didn't know anything about them and I replied that I knew that the workers in the field had the nuclear physics all wrong, and that I knew how to correct it. In fact I'd written a research paper with Lattimer & Mazurek (19) showing how. But I said, "I need an expert on explosions." Hans admitted that he knew something about them. (He had been head of theory in the atomic bomb program in Los Alamos under Oppenheimer.) We delivered the Bethes to their apartment, and then, before going home that afternoon from the Institute, I left a computer printout of a large star that Stan Woosley had evolved up to the point of collapse.

I came in the next morning and went to see Hans. He said, "The entropy of the iron core is very low, less than 1 (in units of k_B^{-1}) per nucleon."

"So what?"

"That means that the iron core will collapse without being held up until the iron nuclei merge into nuclear matter. There isn't enough entropy for them to break up."

"But Hans," I expostulated, "all supernova calculations on the supercomputers since the war show that the collapse is held up at about 1/1000 of nuclear matter density."

"They're wrong!" Indeed, in May of 1978 we had a topical meeting on the collapse of stars and resulting supernovae in Copenhagen, and everyone began tabulating the entropy. The point is that before my paper with Lattimer & Mazurek (19), no one had put in enough excited states of nuclei, which soak up the entropy without producing pressure.

So we were off on a merry chase, which produced BBAL (20), called "Babble" by Willy Fowler. That paper settled once and for all the collapse of stars. I felt like a graduate student (and Hans treated me like one during working hours) taking a crash course in thermodynamics, but avidly soaking up all of Hans's tricks for simplifying calculations. Suddenly astrophysics became an important activity at NORDITA, and also at Stony Brook, where Jim Lattimer and Madappa Prakash joined me to make up a strong nuclear astrophysics effort.

We settled the collapse of large stars and we thought we would clear up the subsequent explosion quickly. Alas, no satisfactory successful supernovae calculations exist to this day. But we're here, and most of the material our bodies are composed of had to be formed in the collapse of stars, so we know that supernovae do work.

Each January since BBAL, Hans and I (at first just the two of us, later with our wives) have shared an apartment or condominium on the west coast, at Santa Barbara, Santa Cruz, or more recently most years at Caltech. We have lived and worked together, having long, interesting discussions and conversations. We now know each other well enough so that one of us has only to begin a sentence for the other one to know what follows. We talk on the telephone a lot the rest of the year.

I do not have space to describe the twenty-some papers we've written together. That's not necessarily bad, because our recent work on black holes is not generally accepted by astrophysicists and astronomers, and I would prefer to write about it later, once it is—as Hans and I are confident it will be—accepted. Let me make an important remark characterizing my work with Hans. Each paper we write is a building block, and we add other blocks on top of it. Rarely do we have to go back and modify earlier work.

In 1985 we wrote a *Scientific American* article, "Theory of Supernovae" (21). In January 1987 we went on to start working on the theory of relativistic heavy-ion collisions, since we thought we had done everything we could do with supernovae. As I left Caltech I shook Hans' hand in farewell and said, "Now it's time for a supernova explosion." On February 23 one occurred! I began feeling that there must be a God, to plan 150,000 years earlier such an explosion to go off just two years after we wrote a popular review article, and one month after we'd run out of work!

Neutrinos were detected from the explosion for about 12 seconds; the optical display occurred, and we waited expectantly to "see" the neutron star. Each January I asked observers at Caltech when we could see it. They always replied that it would take another year for the debris to clear out. I decided I should be able to work out the time when we should see it from the ambient matter accreting onto it, with its binding energy converted into X rays. I did this with a senior, Joe Weingartner (22), and we found that we should have seen the neutron star in 1988.

I phoned Hans one evening and told him, "You know, 1987A has gone into a black hole." He disagreed and listed all the reasons why a neutron star was there. I argued with him. Half an hour later he phoned me saying only, "You're right." We published our 1994 paper saying so (23), suggesting that it had gone into a black hole because it exceeded our maximum neutron star mass of $1.5M_\odot$ The groups that had calculated the mass of the compact core resulting from the explosion of the $18M_\odot$ progenitor of SN1987A always got a core mass of $1.4-1.6M_\odot$ Why we had a maximum mass of $1.5M_\odot$ takes some explanation.

Kaplan & Nelson (24) had originally found that the kaon mass decreased in dense matter. I realized that in neutron stars the kaon mass did not have to go to zero, but if it came down to the electron chemical potential (essentially Fermi energy), electrons would change into kaons, there being plenty of time in stars for strangeness to be broken. The kaons, which are bosons, could then go into a zero-momentum condensate, and this would greatly soften the equation of state, allowing a neutron star mass of only $\sim 1.5M_\odot$ as Thorsson et al. (25) calculated. They had several sets of parameters, but I relied on their calculations for the set obtained by Lee (26), who carried out a calculation in chiral perturbation theory through one-loop order.

I spent the autumn of 1993 in Santa Barbara attending a workshop on hot QCD. The companion program was on dense stellar systems, which I also attended and found extremely interesting. Although not directly in that program, the common envelope evolution of a neutron star in the envelope of an expanding red giant companion seemed particularly interesting to me because the neutron star could accrete mass from its companion. In response to my paper with Bethe giving the maximum neutron star mass as $1.5M_\odot$, people had said, "Of course neutron stars are formed with masses $\lesssim 1.4 M_\odot$ in their evolution." However, in binary evolution leading to double neutron stars, after the first neutron star is formed it must go through common envelope evolution with the expanding (evolving) companion, and my idea was that there, in different situations, it could accrete varying amounts of mass. Thus, if we did not see neutron stars of masses greater than $1.5 M_\odot$ they must have gone into black holes, as we assumed SN1987A to have done.

In reading I noted that astronomers, in their binary evolution, limited accretion to the Eddington limit of $\dot{M}_{\rm Edd} = 1.5 \times 10^{-8} M_\odot$ per year for a $1.4 M_\odot$ neutron star. At this rate of accretion, the radiation of the binding energy, $\sim$20% of its rest mass, of matter falling onto the neutron star produced enough pressure to hold off any greater rate of radiation. This was too low an accretion rate to produce any significant increase in neutron star mass during common envelope evolution. However, Chevalier (27) had shown that if the accretion rate were $\dot{M} > 10^4 \dot{M}_{\rm Edd}$, then the matter accreting onto the neutron star would trap the outgoing radiation and carry it back onto the neutron star in the adiabatic inflow. I was familiar with a similar situation in the collapse of stars, where neutrinos are trapped (at much higher densities) and confined to the star. I immediately looked at envelope densities in the red giant companion and saw that accretion would proceed at a rate of $\sim 10^8 \dot{M}_{\rm Edd}$. This meant that $\sim 1 M_\odot$ would be accreted by the neutron star in common envelope evolution, enough to send it into a black hole. Thus, the standard scenario for evolving binary neutron stars ended up with a low-mass black hole of mass $\sim 2.4 M_\odot$ and a neutron star mass of $\sim 1.4 M_\odot$ formed in the final explosion. I wrote this paper (28) up during 1994 and submitted it to the *Astrophysical Journal*, wondering what the referee would say about my first venture into binary evolution. The manuscript went to one of the world's experts on these matters, Dipankar Bhattacharya, who wrote, "This result is a very important one, and is likely to have a major impact on the evolutionary models of X-ray binaries and recycled pulsars . . .." So I was off into the field of binary evolution.

Of course, we do see binaries of neutron stars, so I had to invent the double helium star scenario for evolving them. In it the masses of the two progenitor stars have to be within 5% of each other, so that they burn helium at the same time. Then the common envelope evolution is that of two helium stars in a common hydrogen envelope, which is expelled. The consequence is that the two neutron stars will be very close in mass. The double helium star scenario is just 1/10 as probable as the original standard evolutionary scenario, so we would have ten times more low-mass black-hole neutron-star binaries than double neutron star ones.

Just before the end of our stay at Caltech in January 1996, Kip Thorne came by our office and told us that estimates for gravitational waves to be seen by LIGO (Laser Interferometer Gravitational Wave Observatory) were based on merging double neutron star binaries. What would merging black-hole, neutron-star binaries contribute? From my earlier work I knew that there were ten times more of the latter and that they had higher masses. I said to Hans, "So this is a project for next January." "Oh, no," he said. "I want to begin on it now." (1996 was the year he turned 90, so I thought that this was a good time for him to begin.) Hans didn't know anything about binary evolution, so I sent him an approximately 20-page article by Meurs & van den Heuvel (29) that developed standard binary evolution. A few days later I got back a fax, in which Hans had rederived their mass results in less than two pages. He wrote, "I haven't done it with as high accuracy as they, but I seem to get the same results." So we carried out "Evolution of Binary Compact Objects that Merge" (30). We could do the binary evolution analytically, with simple mathematics. Peter Eggleton, who has built most of the computer codes for stellar evolution, was referee. I said to Hans, "I have never received a more complimentary referee report." He responded, "Nor have I." Our chief result was that the mergers of low-mass black-hole neutron-star binaries should increase the LIGO detection rate by a factor of 20 over that from binary neutron stars.

We went on from there to the evolution of high-mass black holes, a theory of gamma-ray bursters as being powered by the rotation energy of high-mass black holes, etc. But I've already used up the space allotted to me. Anyway, most of our results and theories in astrophysics are not accepted by the community; we suggested major changes in "accepted wisdom" in almost everything we wrote. Hans Bethe is 20 years and 20 days older than I and still going strong. If I can live a good fraction of that increment longer, I can write a supplementary report on how we fared after our theories have been confronted with the many observations to come.

AFTERWORD

In the foregoing I wrote about events I participated in that might be interesting to the reader. In addition to my research, three other activities gave me great satisfaction:

1. Graduate students and teaching. According to my own count, I supervised the research of 66 PhD students, most of whom are professors somewhere in the world. "Supervised" may be too strong a word, because often those students also did research with others during their graduate career. Mostly, I just started my graduate students on interesting problems, and then "hung on for the ride" as they accelerated, becoming excited about their work. Each developed his or her special way of attacking problems. My contribution was to get them started and then to offer criticism along the way.
2. Building a group. Our nuclear theory group at Stony Brook speaks for itself. Organizing a group that (most of the time) works well with itself is like directing an orchestra. It is very satisfying when the members play well together.
3. My work in editing. When I went to Copenhagen in 1960, I told Leon Rosenfeld, who had started the journal *Nuclear Physics* in 1956, that I would be happy to help him in the editing. He immediately went off for most of the academic year on an around-the-world trip, leaving the journal to me and a technical assistant, Ad Compagner, who later became a professor in Delft,

Holland. It was natural, when Leon died in 1974, that I take over as editor. I was later joined by more supervisory editors, now numbering seven.

With Dirk ter Haar of Oxford, I started *Physics Letters* in 1962 and *Physics Reports* in 1971. We had the good sense to add Maurice Jacob to the editors of the latter. He put it on the map immediately by getting many good reviews from members of CERN. I am still an editor of this journal.

I've enjoyed editing. I can still scan a sheet of manuscript quickly and pick out typos. It takes longer to check the physics.

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