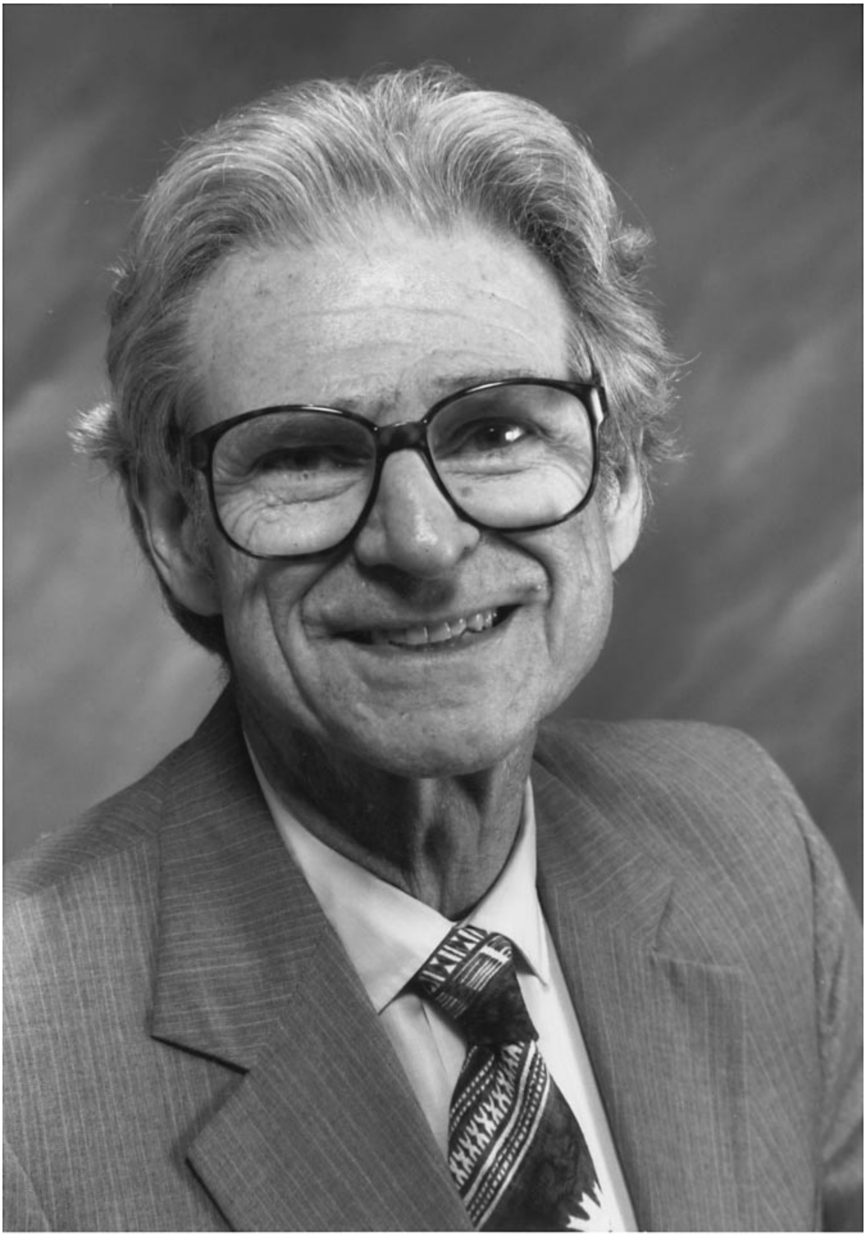

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A GENERALIST LOOKS BACK

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■ **Abstract** I fled with my parents from Hitler's Austria to Australia and studied physics at Sydney University. I obtained my Ph.D. in quantum electrodynamics with Rudolf Peierls at Birmingham University and came to Cornell to work with Hans Bethe. I have stayed at Cornell ever since, and I have essentially had only a single job in my whole life, but have switched fields quite often. I worked in nuclear astrophysics and in late-stellar evolution, estimated the Initial Mass Function for star formation and the metal enrichment of the interstellar medium. I suggested black hole accretion as the energy source for quasars, worked on molecule formation on dust grain surfaces, and was involved in 21-cm studies of gas clouds and disk galaxies. I collaborated with my wife on the neurobiology of the neuromuscular junction and with one of my daughters on the epidemiology of tuberculosis.

INTRODUCTION

A generalist is a person who learns less and less about more and more subjects, until he knows nothing about everything. I am getting close to that endstate, and this essay will trace my evolution toward it. The definition above really refers only to successive generalists—a professional basketball player giving up his profession and turning to baseball, for instance. I will also have to deal in some cases with simultaneous generalists applying expertise from one field of science to a problem in another field.

Most essays in this series have stressed some unifying theme, some primary goal, or some preferred scientific method. Presumably, most successful scientists are golf-players, i.e., they map out their own strategy beforehand and can then write a unified story. I belong to the minority of scientists who are ping-pong players, i.e., they react to influences from others on a short timescale without much systematic planning on their own. I therefore cannot write a coherent scientific narrative (and if I could, only a fraction of it would be astrophysics anyway) but will deal mainly with sociology: A ping-pong player reacts to outside players, but has to decide which of several possible influences to react to, so decision making is important. It may be natural for an autobiographer to brag about his past, but I brag more about the few right decisions I have made than about scientific achievements. I will also try to mention some of my failures in both areas (they may be more instructive to young players than the successes), but I am probably glossing over the most important ones (just as the characters did in the old Japanese film, “Rashomon”). I should also apologize for citing so many of my own papers and so few others.

SCHOOLING AND PARENTS

I was born in Vienna in December 1924, spent some time in Hungary, but returned to Austria for elementary school in Vienna. My first academic event came at the age of 10, when I failed my entrance exam into Gymnasium, the eight-year academic high school system in Austria. My mother claimed it was just due to the authorities being anti-Semitic. They were indeed anti-Semitic, but I felt I had flunked mainly because I read Karl May (Wild West adventure books especially for German boys) instead of studying for the exams. Gymnasium was essential for anything else you might want to do later in life in Austria, and I learned my lesson, at least temporarily: I had to learn Latin and other academic subjects on my own for one year, took another exam and got into second year of Gymnasium. Having to work very hard that year was actually an exhilarating experience, but my concentration did not last long without an external stimulus. We lived in a rather nonintellectual neighborhood in Vienna; there was no tracking in school of any kind, and I was soon the top of my class in all subjects in my rather mediocre gymnasium class. I soon returned to reading Karl May, had no preference for one academic subject over another, nor any ambitions for my future. This serenity (or indolence) was interrupted three years later by a traumatic event—the annexation of Austria to Nazi Germany in the spring of 1938.

In September 1938, all Jewish children were thrown out of the regular schools and only a very small fraction were able to go to a single Jewish high school in all of Vienna for the next academic year. The selection of the students was academic, so each of my new classmates had also been at the top of their class in everything the year before, and it was an exciting school atmosphere. Suddenly I was no longer the top of my class in anything except math and science. I also noticed that one half of the kids who were beating me in Latin, history, and other subjects, were girls—this school was one of the rare places where purely academic selection replaced male chauvinism. This exciting experience only lasted three months for me and was replaced by excitement of a different kind: On November 10, 1938, the infamous Kristalnacht, someone tried to arrest me even though I was only 13 years old (but I was proud of looking older). My parents took me out of school that day, and I had to go into hiding, even though we did not know why they had tried to arrest me. I did not attend any school again until we fled from Hitler and arrived in Australia in June 1939.

My stay in the Jewish school demonstrated that I was more gifted in some subjects than in others, before I had thought about which subjects I actually liked best.

It is curious that during my hiding period, when I assessed that my probability of surviving to adulthood was small, I started thinking seriously what I might do if I did become an adult after all. Although I had not formulated any preferences, I decided it was safer to choose a profession according to one's abilities, rather than one's preferences. Much later I found out that I loved opera but could not hold a tune, so singing would not be my profession; of academic subjects, I found anthropology more interesting than the physical sciences, but I wasn't very good with people.

Apart from ability, my eventually going into the physical sciences was almost inevitable: Sons of professional sword swallowers often become sword swallowers, and my father was a physicist. Even my mother was a physicist—at any rate she had a Ph.D. in physics, although she stopped working after I was born. Although I seemingly did not have a close relation with my father, he had had an interesting career, and he had a strong influence on me. He was born in Galicia, as were many Austrian Jews, but moved to Vienna to study at the university. As a young student he was a close friend of Erwin Schrödinger. There even was a rumor that Schrödinger lost his virginity while borrowing my father's apartment, somewhat like the Jack Lemmon movie "The Apartment." My father was poor but had his own little apartment; Schrödinger was rich, but etiquette required that he live with his family (Moore 1989). They maintained some contact even after Schrödinger got his Nobel Prize. My father had an early interest in mathematics but also in applications, and he wrote a very early textbook on *Mathematical Methods for Scientists and Physicians*. Had he been a gentile, he would probably have ended up as a university professor, but Austrian anti-Semitism made that difficult. Industry was more liberal at the time, and he joined Elin, an Austrian electrical concern outside of the European cartel, and became the director of a factory making incandescent lamps. The main challenge was not making the lamps, but avoiding patent fights with the cartel by inventing alternative patents. This cartel/outsider rivalry indirectly saved our lives in 1938–1939 when an Australian outsider hired my father to start a lamp factory in Sydney, in competition with the Philips-led cartel. This enabled us to flee from Hitler and start life again in Sydney. Ironically, a few years later my father switched and joined Philips.

My new life in Sydney began at fourteen, just as World War II started and I was classified as a "Friendly Enemy Alien" (German passport, but Jewish). Even with the war going on, Australia was an ideal place for a teenager to grow up, and I had a happy life. I was accepted into Sydney Boys' High School, a selective high school with some tracking, and found it an excellent and liberal school. Academically, I had a problem at first—the numbering of school years was different from Austria, with the end of high school after fifth grade instead of eighth. Because I seemed bright (although I knew very little English), the school took a chance and put me in fourth grade—if it all worked out, I would graduate at sixteen instead of eighteen in spite of the half year of school I had missed! I had to make up one or two years' worth of schooling in all subjects, mostly on my own but with just the right amount of encouragement and advice from my teachers. I started in June 1939, and I certainly had to work harder the next 18 months than any time before or after.

My hard work in my mid-teen years paid off; in fact, I overcompensated a bit (as often happens), and I graduated from high school with honors. I had a great time and only regretted that I did not learn how to date girls, but I figured I could catch up in college. Having had to work hard in all academic subjects, instead of concentrating on one favorite, already predisposed me toward being a generalist. It also taught me how to put in the minimum effort for a successful conclusion, which is useful if you're working on several problems simultaneously. On the other hand, all this also predisposed me toward being a little bit sloppy, which is not so good. At any rate, I was able to start at Sydney University at the age of sixteen, majoring in mathematics and physics as my parents had expected me to.

My undergraduate years provided enjoyable distractions, especially bushwalking in the outback during major vacations and some minor involvement in left-wing scientific politics. However, these years also gave me a good basic education and were uneventful except for two excursions. I got interested in Chemistry, had summer jobs in chemistry labs, and almost switched my major. In the end I stayed in physics, but retained a love for chemistry. The other distraction had to do with Australia's war effort in radar, which university students majoring in the sciences were supposed to help with, instead of joining the army. As a consequence, I was involved in research and development in radiophysics, and what we would now call plasma physics, but I did not return to this field for about 15 years. Radiophysics pointing toward radio astronomy was in the air in Sydney in 1945, with some very able people at the Radiophysics Lab of CSIRO, just across the street from my physics department. I did not make use of the exciting opportunity there, but instead went into quantum electrodynamics, another field that was in the air in Europe and in the USA, even though not in Australia. I started with a pedagogical masters thesis on Field Theory—a modest effort but enough to get me a prestigious scholarship to go to graduate school in Great Britain.

Here I faced my first challenge to make the right decision in spite of overwhelming advice for the opposite. With my scholarship I could have gone to Oxford or Cambridge, which of course had great prestige overall. Furthermore, Cambridge had Dirac, one of the greatest figures in quantum electrodynamics, and most of my advisers were urging me to go there. Fortunately, a few people I trusted most, including Bert Corben in Melbourne, assured me that Rudolf Peierls had the best school for theoretical physics in Europe—even though he was at the less prestigious red-brick university in Birmingham. Fortunately, I followed this correct advice and went there.

QUANTUM ELECTRODYNAMICS: BIRMINGHAM TO CORNELL

I started in Birmingham University in the Fall of 1946 in Peierls' school, housed in temporary and uncomfortable quarters. Altogether, I was surprised to find that living in England in the postwar years was much more strenuous than the war years themselves had been in Australia. In spite of the food rationing and other discomforts, being a graduate student with Peierls was a glorious experience. He had a stable of bright young people and guided them in many topics—not just quantum electrodynamics, but nuclear theory, statistical mechanics, and solid state physics as well. Rudi Peierls was a great role model for becoming a generalist and even the vacations were a broadening experience. My overseas scholarship was financed and administered by "The Commission for the Exhibition of 1851" and this organization also helped to farm us students (possibly thought of as "the deserving poor from the colonies") out to the stately homes of England for vacations. These stays were all enjoyable, but one in Wales was also memorable—with Charles and Dorothea Singer—early and brilliant historians of medicine. Medicine as an academic venture seemed fascinating to me from then on.

The early postwar years in England were also memorable for observing the bigshots in theoretical physics at conferences that Peierls organized. It was particularly interesting to watch the first time Werner Heisenberg met Peierls, Bethe, Frisch, Oppenheimer, Pauli, Dirac, and others after the war. It was a strained atmosphere, but (unlike remarks in a recent play) none overtly refused to shake Heisenberg's hand (although some people who happened to be in another part of the room when he entered did not have to shake hands). I was a bit puzzled that Pauli, who normally made a point of being rude to people, was singularly deferential to Dirac. In any case, Pauli's rudeness was only the cover for a very kind man: When I asked him a physics question once to which I really needed the answer, he first shook his head sadly and said, "I don't know how people can be SO stupid," but then gave me the real answer patiently and in detail. I also made some pilgrimages to Cambridge—Dirac was impressive at answering questions, even though he would not have been a good thesis adviser. Incidentally, I had already met Tommy Gold, Fred Hoyle, and Herman Bondi in the late 1940s. My scientific interests did not yet overlap theirs, but their fertile and bubbling minds were most stimulating, and the trio became both role models and friends later on.

During the three years in Birmingham, I had already showed a tendency toward generalism by doing some work outside of quantum electrodynamics. One example was a paper on nuclear induction signals (Salpeter 1949), which was not only pedagogical, but modestly useful and has been quoted, or used in lab courses, off and on for some years later. By contrast, the paper based on my whole Ph.D. thesis is probably my least quoted paper (even I am not citing it here)—and for a good reason. My thesis was designed to take the singularity in the electrodynamic self-energy of the electron seriously and to calculate it rigorously by starting with a finite size and then proceeding to the limit of a point electron. In the meantime, it had become important to do a practical calculation of the Lamb shift in the hydrogen atom, in spite of the singularity, and Hans Bethe had done this in a simple, though approximate manner (Bethe 1947). Essentially, he just compared a free electron with one inside the atom and was able to circumvent the singularity. This beginning of renormalization theory was followed by two more rigorous and elegant formulations, one by Schwinger at Harvard and the other by Feynman at Cornell (plus later synthesis by Dyson). These elegant formulations were beyond my capacity as a graduate student, but my thesis work had put me in an excellent position to have done Bethe's simple calculation; I had an excellent opportunity to do it first but did not take it—I simply just did not think of it! This was my first, but not last, instance of missing the boat, and my thesis was out of date before it was out in print.

In spite of missing out on the big time in Birmingham, I had done enough so that I had the opportunity to go as a postdoc or research associate, either to Oppenheimer at the Princeton Institute for Advanced Study, or to Bethe at Cornell University. This gave me my second chance to make the right decision in spite of opposite advice. This was the heyday of Oppie and his high-powered group at the Institute, and I was assured that two years in this intense atmosphere would condition me to return to a good job in England (a two-year stay seemed to be the norm, as for Ladies Finishing Schools, although Peierls' wife Genia had already told me "within a year you will marry an American girl and you will never come back permanently"). There was then advice to choose Princeton, from many people but fortunately not from Peierls, and I myself had no doubt that I wanted to go to Cornell. Bethe was a close friend of Peierls, had a very similar scientific temperament, and had visited the Birmingham department a few times. The graduate student offices were lined up in a row like a railroad car, and Hans would go from one to the next, like a chess Master playing simultaneous games, and give advice. The advice was instant, but low-key, and I was sure I would find Bethe's group of theorists at Cornell a relaxing but stimulating place. I arrived at Newman Lab of Nuclear Studies in October 1949, one month late, because I had had trouble getting a U.S. immigration visa. I consequently got the worst of the offices for first-year postdocs (it even had a crack on the outside wall). To give away the punchline early in this essay—I have essentially had only one single job for my whole life, and I stayed in that office for exactly 50 years (at the end of 50 years the crack had not got smaller nor larger). Maybe the invariance of my geography led me to change fields often.

In early 1950, I met Mika Mark, an entering graduate student in psychobiology, and as Genia Peierls had predicted, we got married in June. Some years after her Ph.D., she switched to neurobiology and biophysics, which gave us an opportunity to collaborate, but in the 1950s she mainly helped me in decision making, because she had more common sense than me. With Richard Feynman and Freeman Dyson passing through Cornell, and with a certain rivalry between Feynman and Harvard's Julian Schwinger, Cornell was a hotbed of quantum electrodynamics, and I was involved off and on for a few years. (For one horrendous calculation of fourth-order vacuum polarization, three of us did the same calculation independently to check each other—one, a graduate student, Michel Baranger, the other two, Freeman Dyson and myself. Dyson and I got the same answer, the graduate student got a different answer—and his was the correct one.) However, I have to describe next one particular paper and one decision very soon after that.

RELATIVISTIC BOUND STATES TO NUCLEAR ASTROPHYSICS

The early 1950s saw the beginning of elementary particle physics and high energy theory, including speculations about very strong, hypothetical, attractive forces, including a theory proposed by Fermi & Yang (1949). One extreme example of strong attraction would be the bound state of two Fermi-Dirac particles where the binding energy is so large as to cancel (fully or almost) the sum of the two rest mass energies. For such an artificially low-mass composite particle, a combination of special relativity and quantum mechanics would be needed. Such a combination for bound states did not yet exist, but Hans Bethe saw in 1951 how to develop a formalism, starting from techniques Feynnman had invented recently for quantum electrodynamics of scattering problems. Hans got me to work out the details and we soon had a plausibility argument for a new equation that incorporates relativity into bound state problems. It was a rather elegant equation, involving a mysterious relative time coordinate, and potentially useful in the future but not easy to manipulate. It soon became known as the "Bethe-Salpeter Equation," even before it was published (Salpeter & Bethe 1951), and I got a lot of attention from the theoretical physics profession. You might think that I am particularly proud of that paper and that I was all set to stay in the field of high energy theoretical physics and quantum electrodynamics for life. In reality I am proud, not of that paper, but of my decision to slowly get out of that field altogether.

I am proud of my realization in 1951–1952, in spite of my seeming success, that I just did not have the right abilities or temperament for quantum electrodynamics/high energy/elementary particle theory. This field required abstract mathematical thinking and a mind devoted to rigor, whereas I had a quick but sloppy mind. Even the Bethe-Salpeter equation itself was really invented by Hans—I had only filled in some details—and I could see that I would not get very far with applying it in the future to challenging, fully relativistic problems. My unsuitable temperament was illustrated at a colloquium I gave at the Princeton Institute for Advanced Study, on possible applications of the equation but with only a plausibility argument for it instead of a rigorous derivation. The whole audience felt that such a derivation was required and somebody, who looked young enough so that I thought he was a high school boy there by mistake, got up and (together with Francis Low) outlined the rigorous derivation of the equation on the spot. He was not quite a high school student, he was Murray Gell-Mann, and he got the Nobel Prize for other things a short while later, but he produced the derivation with rather little effort (Gell-Mann & Low 1951). However, the main warning for me was not so much that I had not given a derivation myself, but that with my temperament I had felt a plausibility argument, rather than a derivation, was good enough.

Over the next few years I still wrote some papers on quantum electrodynamics, especially on some precision atomic physics calculations that used low-brow applications of the Bethe-Salpeter equation (e.g., Salpeter 1952a). Hans and I also wrote a rather detailed monograph on Quantum Mechanics, which still sells one or two copies a year worldwide (Bethe & Salpeter 1957). Having written meticulous papers with highly accurate calculations (including the fourth order correction paper with Baranger & Dyson) would give me self-confidence later on, but I needed a change. For my scientific temperament I needed a field that was more controversial, more open-ended and new, where quick was useful and sloppy did not matter too much because it would all change soon anyway. Hans Bethe had invented such a field a few years earlier—nuclear astrophysics—by showing how the conversion of hydrogen into helium provided the energy source for the sun and other main-sequence stars (Bethe 1939, Bethe & Critchfield 1938). There were star types other than the main-sequence and plenty of elements other than helium, so nuclear astrophysics was sufficiently open-ended with plenty of uncertainties. Willy Fowler, a nuclear experimentalist at Caltech's Kellogg Radiation Lab, was doing the appropriate measurements, and he had the foresight to invite some young theoretical physicists to explore the theoretical aspects of nuclear astrophysics. Although I continued to stay at Cornell for the main academic year, I started to spend summers at Kellogg Lab, beginning in June 1951. To demonstrate my maturity in 1951: My first impression was of Willy's immense old age—he was having his 40th birthday when we arrived that summer—but his leadership qualities came through pretty quickly after that.

Some areas in nuclear astrophysics were fairly straightforward, and I worked on some of these in that summer and over the next few years—for instance, on the detailed completion of the proton-proton chain (Salpeter 1952b). However, the real challenge was to explore nuclear reactions that start from seemingly inert helium in red giant stars, after main-sequence stars have exhausted the hydrogen. I started to work on that also in the summer of 1951. This was a more controversial subject, which brings me to a particular paper, published the following year, plus its aftermath a few years later.

HELIUM-BURNING: A TALE OF TWO RESONANCES

Hans Bethe was a different role model for different situations, but for an open-ended and controversial situation the advice was three-pronged: (*a*) Be prepared to switch fields; (*b*) use only the minimum mathematical technique necessary; (*c*) in the face of uncertainty, be prepared to use conjectures and shortcuts and take risks—in other words—have CHUTZPAH! For my next paper, I had learned lessons (*a*) and (*b*), but not yet (*c*).

Nuclear astrophysics was needed for two related but different problems: (*a*) The source for energy production in red giant stars and other types of evolved stars and, (*b*) building up all the heavy elements in evolved stars and getting the isotope ratios right. The second of these was most exciting in the long run, partly because of the controversy as to whether the Big Bang Cosmology could produce all the heavy elements in the first few minutes (e.g., Gamow 1946) or whether evolved stars could, as is required by the Steady State Theory (reviewed by Bondi et al. 1995). In spite of its importance, this problem was too daunting for me (there are many more isotopes than there are types of stars), so I merely looked for energy-producing reactions in red giant stars. Stellar evolution theory was in its infancy, but calculations by Schwarzschild & Sandage, by Hoyle and by some others, (e.g., Sandage & Schwarzschild 1952), were already showing that red giant stars, in spite of their cool surface, have a hot and dense interior consisting mainly of helium. Be^8 was known not to be stable, so something more complicated than a single two-nuclei reaction was needed. Fortunately, my host Willy Fowler and his experimental colleagues had shown recently that Be^8 is almost stable, i.e., that its meta-stable groundstate provides a resonance level at a positive but quite low (and known) excitation energy for a pair of alpha-particles. Hence, one can form C^{12} from He^4 without needing an explicit three-body reaction, i.e., the meta-stable Be^8 state is in thermal equilibrium with helium and can then absorb another alpha-particle from the tail of the thermal distribution.

I calculated the rate for this indirect conversion of helium into carbon (much more rapid than the direct triple-alpha reaction without the beryllium resonance would have been) in the summer of 1951 and published it in the following year (Salpeter 1952c). I noted in that paper that my calculated rate could easily be too low by a factor of 1000, say, if there should be an appropriate resonance level in C^{12}, but I did not have the chutzpah (or guts) to do anything about it: My energy production rate for red giant stars required a central temperature that was within the rather uncertain range given by stellar evolution theory at the time; my calculation would lead to most of the helium being converted to oxygen and neon instead of carbon, but I just did not have the guts to think of resonance levels that had not been found yet! A short while later Fred Hoyle demonstrated both chutzpah and insight by using the known abundance ratios of C^{12}, O^{16}, and Ne^{20} to show that there JUST HAD to be an appropriate resonance level in C^{12}, and he was able to predict its energy (Hoyle 1954).

Willy Fowler and his colleagues soon looked for Hoyle's predicted resonance level and found it just where it should be (Cook et al. 1957). This made a believer in theoretical nuclear astrophysics out of Willy and has been a great object lesson to many ever since. However, history tends to oversimplify, and the spectacular prediction of the carbon resonance level has obscured the fact that the beryllium resonance level also was needed to increase the rate enormously. This level, and many others, was the experimental achievement of the Kellogg Radiation Lab (including Charlie and Tommy Lauritsen, Ward Whaling, and many others in addition to Willy Fowler) and a few other experimentalists elsewhere. One reason why experimentalists do not get enough credit may be that most of the review articles are written by theorists. The two resonances and the work of Ernst Öpik also illustrate the danger of being in the wrong place too early: Öpik was an Estonian but worked much of his life in a prestigious, but somewhat isolated, Irish Observatory. He actually published a paper on the triple-alpha reaction before mine or Hoyle's (Öpik 1951), but (*a*) got too low a reaction rate because he did not know about the beryllium resonance, and (*b*) his work was completely unknown in Britain and the USA until 1953. Even after that, Öpik did not get as much publicity as this remarkable, although gruff, man (and generalist) deserved. I also suffered the consequences of being 20 years too early in another topic: I wrote a paper on the burning of primordial deuterium during the contraction of a proto-star onto the main sequence for an interesting meeting in Belgium that I unfortunately was not able to attend (Salpeter 1953). This paper had two defects: (*a*) I missed out completely on the Hayashi Phase of contraction (Hayashi 1964), which drastically alters the contraction phase, and (*b*) I did not know the value of the primordial deuterium abundance. Deservedly, this became one of my least quoted papers even though deuterium burning became of interest later on.

THE INITIAL MASS FUNCTION AND THE ENRICHMENT OF THE INTERSTELLAR GAS

In the early 1950s I thought of my nuclear calculations as straight physics, to be applied to astrophysics mainly by others. Nevertheless, I was learning some real astronomy from experts, largely on visits to Princeton and Caltech, especially from observers like Baade, Greenstein, and Sandage in Pasadena and from theorists like Schwarzschild and Spitzer in Princeton. One special educational experience for me was the Ann Arbor summer school, or Michigan symposium on Astrophysics, in the summer of 1953. This summer school was the most formative meeting ever for me (and possibly for others as well; see Gingerich 1994), probably because it both had bigshots as role models and also had us youngsters lecturing to each other (reminiscent of Oppie's motto "what we don't understand, we explain to each other"). Walter Baade was the star of the show, explaining the two Stellar Populations and also introducing us to galaxies. George Gamow was equally impressive talking on everything, an important role model for me for being a generalist (fortunately his example of drinking Vodka from a water pitcher already at lunchtime was not followed by us youngsters). I also remember fondly Leo Goldberg and Allan Sandage; also George Batchelor, who lectured on turbulence. I myself talked on hydrogen and helium burning, but also on reactions between C^{12} and O^{16} in more evolved stars at about 10^9K, as well as subsequent photodisintegration. I had already mentioned this qualitatively in print (Salpeter 1952c), but I never followed it up with detailed calculations.

Just before the 1953 summer school our first daughter, Judy, was born and soon after it we went for one year to the newly founded Australian National University (ANU) in Canberra. Although nominally a one-year trip to the ANU, I was offered the Chairmanship of the new Theoretical Physics Department there. The main emphasis of this department would have to be High Energy Theory, and I would have a dilemma. In fact, I started spending more and more time in 1954 at Mt. Stromlo Observatory, which was quite close geographically, learning more about observational stellar astronomy and also hearing about galaxies from Gerard and Antoinette de Vaucouleur.

The Burbidges, Fowler, and Hoyle (Burbidge et al. 1953), as well as Al Cameron (e.g., Cameron 1958), were showing how heavy elements are built up in the deep interior of massive, and very massive, stars in various late stages of evolution, including supernova explosions. However, a practical question was how much enrichment of the interstellar gas there could have been from the breakup of these stars, given the fact that massive stars are very rare today. Walter Baade had made clear (mostly orally, rather than in print) the distinction between Stellar Population II (formed when the Galaxy was young) and Stellar Population I, with stars having been born continuously over the lifetime of the Galaxy. Observations had already been made for globular cluster stars in Stellar Population II (Arp et al. 1952), but we and the interstellar gas are situated in the Galactic Disk where Stellar Population I resides. Heavy elements today in the interstellar gas came mostly from stars that are already dead and the question was how many more stars were born and have died than the few that are still alive today, separately for different stellar masses.

To calculate how many massive stars were born and have died over the lifetime of our galaxy, the "initial mass function" for star formation (or the "birthrate function") was needed. This in turn required three ingredients: the lifetime of a star as a function of its mass, the mass-luminosity relation, and the luminosity function for Population I main-sequence stars. Each of these three was known only extremely approximately in 1954, but I had learned my lesson about the importance of chutzpah: I used what little I could find in print on each of these three functions. A clean separation of main-sequence stars from other stellar types was a bit difficult at the time, but the situation was made easier by the fact that stars spend much more time on the main-sequence than on the (interesting) later evolutionary phases. I fudged over the distinction between stars in the galactic plane versus those in the whole disk and over the time variation of the star formation rate from the young and gas-rich galaxy to the present-day gas-poor galaxy. With these two approximations in hand, I finally got a crude estimate of the Initial Mass Function, of the amount of heavy elements in the interstellar gas, and also of what fraction of stars should be White Dwarfs today. Of my 300-odd papers overall, the one I am most proud of was published in 1955 (Salpeter 1955).

I am proud, rather than ashamed, of the sloppy approximations I made in that paper, but I was also lucky that the several errors (corrected later by M. Schmidt and others) mostly canceled each other, rather than adding up. The result was also a particularly simple powerlaw for the Initial Mass Function, which is close to predicting logarithmic scale-invariance for the distribution of mass among different stars (a similar, but not identical, relation holds for the distribution of people into villages, towns, and cities). Developments (or lack thereof) in the 45 years since have been good for me personally, but bad for the science: I had hoped that my Initial Mass Function was only an average for today and that the actual function would vary strongly from quiescent to active regions and would also vary from the young galaxy to the galaxy today. Some such variations probably exist but are too weak for universal agreement, so "the Salpeter function" is still of some use today and gets quoted, but the theory of star formation has suffered greatly by not having clear-cut observational variations, which would have to be predicted by a correct theory.

My dilemma between accepting the Theoretical Physics Chair at the ANU and returning to Cornell was resolved fairly quickly: At the ANU I would have to concentrate on high-energy physics, but by now I was hooked on astrophysics and even on astronomy. Furthermore, male chauvinism was slightly less severe at Cornell than at Australian universities, especially later on after my wife had switched from psychology (the area of her Ph.D.) to neurobiology. In September 1954 we returned to Ithaca—myself to a tenured associate professorship in physics and my wife to two rush-jobs before returning to biology research in 1956: (*a*) she learned enough architectural skills to design a house and acted as contractor for its construction; (*b*) she had a second baby.

A GENERALIST'S DIVERSIONS: PLASMA PHYSICS AND HIGH PRESSURE STATISTICAL MECHANICS

I did not overlap in time with Sir Arthur Eddington, but my thesis advisor Rudi Peierls did, and he corrected a wrong paper of Eddington's (Eddington 1935; Peierls 1936). As I discuss in more detail in a Chandrasekhar memorial (Salpeter 1996), Eddington's error was in part due to his realization that Coulomb forces in the ionized stellar interior (i.e., plasma physics) have to be included in principle, in addition to gravity. It was mostly a red herring in Eddington's time, but this admonition led me to an early and approximate treatment of electron-screening corrections to stellar thermonuclear reaction rates (Salpeter 1954). In this field also, later developments have been good for me and bad for the science. Modern treatments of screening have been controversial, and results have tended to oscillate around mine, so that my old paper still gets quoted! However, by now it is pretty clear—at least for the sun where several inequalities are not very strong—very extensive numerical calculations will be needed for very precise results, and computers are just about up to that task now. The importance of plasma physics and statistical mechanics for stellar interior calculations was also clear to Evry Schatzman, who wrote an even earlier paper than mine on electron screening (Schatzman 1948), and to Al Cameron. Cameron wrote the first paper on pycnonuclear reactions, the real cold fusion (Cameron 1959), a topic that also inspired one of my later graduate students, Hugh Van Horn (Van Horn & Salpeter 1969).

My second encounter with plasma physics, a few years later, also got me involved in ionospheric research and came about by a comedy of errors. In 1958, Bill Gordon had proposed the Air Force fund the construction of the Arecibo Ionospheric Observatory in Puerto Rico for incoherent backscatter of radio waves from the free electrons in the ionized upper atmosphere. He pointed out, correctly, that the electrons would mostly follow in time the density fluctuations of the massive, slow positive ions, but he did not give a rigorous derivation (Gordon 1958). Somebody at the Advanced Research Projects Agency (ARPA) wanted to check up by using one of the early rigorous plasma theory papers by Bohm & Pines. That paper had replaced the positive ions by a fixed uniform background density, for simplicity in treating the free electrons alone. In this hypothetical world, there would be no ion density fluctuations at all, so the electrons could not follow the nonexistent ions, and the ARPA official concluded (incorrectly) that the backscattered intensity would be much smaller than Bill Gordon's estimate. I was associated with ARPA as a member of the JASON division and agreed to look into the controversy. I soon found out that most of the backscatter would indeed be qualitatively as Bill Gordon had predicted, and I was able to allay ARPA's misgivings. The controversy was laid to rest further when two other groups also published independent papers to this effect. However, the diversion about free electrons without positive ions got me curious about the smaller, but interesting, electron backscatter effects that do not involve the ions at all. I showed that this backscatter would give a frequency shift related to the plasma frequency, a kind of Raman scattering (Salpeter 1960a). I believe this paper turned out to be my most-quoted paper ever, not because of the ionosphere but because this kind of backscatter is a convenient way to measure the electron density in laboratory plasmas, and the number of plasma experimentalists is very large. A little while later, an expert graduate student, "Rip" Perkins, succeeded in measuring this kind of Raman scattering from the ionosphere's plasma oscillations (Perkins et al. 1962).

Plasma physics, and related subjects such as interplanetary scintillations, also fascinated many of my graduate students and some became much more professional as plasma physicists than I was, e.g., Rip Perkins, Nick Krall, Allen Boozer, Dick Lovelace, and Jonathan Katz. Besides plasma physics, the statistical mechanics for calculating high pressure equations of state was of interest, not only for the really high pressures in white dwarfs and neutron stars, but also for the intermediate high pressures in the Jovian planets. Some of the papers in these areas may seem a bit abstract (Salpeter 1960b, 1961; Zapolsky & Salpeter 1969), but had applications to real objects.

Statistical mechanics is also involved in various neutrino emission processes, both in the sun and in highly evolved stars. Shaviv and I wrote a few papers relevant to the solar neutrino controversy (e.g., Shaviv & Salpeter 1971), but this topic was covered more extensively by John Bahcall and his colleagues. Neutrinos from highly evolved stars, however, were a favorite Cornell topic with many competing emission processes to be considered. There were papers from Phil Morrison and a number of my graduate students, ex-students, and visitors, including Beaudet, Chiu, Petrosian, Silvestro, Stabler, and Zaidi (e.g., Beaudet et al. 1967).

MAPLE SYRUP, ADMINISTRATION, AND NATIONAL SERVICE

Starting in the 1960s I occasionally got job offers for chairmanships and other administrative positions elsewhere. Cornell salaries were rather low, and I was only a rank and file Professor, so these offers involved a substantial raise in salary. My wife and I once made a list of what we would do with the extra money, but most items were a mixed blessing, e.g., with two cars instead of one we would have to get registrations and snow tires twice as often. The only improvement with no drawbacks at all would be buying pure maple syrup instead of the cheaper imitation syrup we had used in the past. We decided that we could afford maple syrup even on a Cornell salary, have used it ever since, and never again thought of income as a determining factor. Of course, when considering an administrative job the real questions are in any case not the money, but how stimulating the challenge is and whether you are up to the job. Unfortunately I was not and am not gifted in such matters, but fortunately I realized that fact before the Peter Principle took over. Consequently I turned down all these offers, but I often suggested people who had the right abilities and sometimes they even were appointed.

My only administrative exception was my directorship of Cornell's Center for Radiophysics and Space Research for eight years, rather late in my career. Fortunately, by that time Tommy Gold and Yervant Terzian had established a smoothly working modus vivendi for astronomy and related fields and other people (e.g., E. Bilson, P. Gierasch, and Y. Terzian) did most of my job for me. My major achievement came when a professor wanted a salary for his secretary above the top of the range—I managed to invent a new job category, so there was no salary range to worry about!

When I returned from Australia in 1955, there was some worry about a missile gap, and the defense establishment was looking for technical consultants. By that time the Joe McCarthy hysteria was mostly over and, in spite of my left-wing "pinko" youth in Australia, I was able to get my security clearance. After some time consulting for private industry, I became a member of JASON, a group of young professors with their fulltime job at a university during the academic year but doing classified defense work during the summers. In contrast to my administrative ineptitude, I was fairly good at technical National Service, especially at analyzing work already done by others for the defense department and at evaluating claims. My JASON activity that I was and am most proud of had to do with anti-ballistic missile defense (ABMD) in the 1960s. Various schemes for ABMD had been proposed and my evaluations debunked a number of them, and I wrote a thick report that saved the defense department a lot of money. However, it was much more difficult to combat the atmosphere of "dishonesty without outright lies" that pervaded the ABMD community then (and now): I even spent a week on Kwajalene Atoll in the Marshall Islands where the ABM radars were, and are, located. It took me almost the whole week to discover all the information about the incoming missiles that the radars had been given, contrary to the propaganda I had been fed initially. Almost forty years have passed, but false or deceptive claims in favor of ABMD were remarkably similar then as now. I believe CNN televison had a problem recently, similar to mine on Kwajulene, to discover just what information the ABM radar and missile was given beforehand at a claimed ABM success. I resigned from JASON during the Vietnam war, but kept my security clearance. I became a member of a panel, appointed by the American Physical Society to evaluate directed energy weapons for ABMD during the Reagan Administration.

The unanimity of our panel, in spite of a wide range of political views amongst the members, was very gratifying. The fact that all panel members had security clearance gave more political credence to our report debunking ABMD (APS Study Panel 1987). A recent evaluation of our panel report makes it clear that a similar panel will be needed to look critically at current ABM plans, which seem at least as misguided as the two previous plans (Kubbig 2001).

A less satisfying piece of National Service was my membership on the National Science Board (the Board of Trustees for the NSF) from 1978 to 1984. That board was and is quite important (my only U.S. Presidential appointment), but I personally was not an effective member. During my tenure on the Board, the NSF short-changed astronomy in my opinion, especially by abandoning a NRAO project for a large millimeter wave radio astronomy dish. This was partly due to some unjustified badmouthing of observational cosmology in the literature, and I could not even neutralize these false accusations in the eyes of the Board and of the NSF. These six years illustrated for me a general feature of the academic life: Most of the time one has enough flexibility to work only on those things one is gifted for, but occasionally one has to tackle a job that highlights one's own inadequacies.

THE NEUROMUSCULAR JUNCTION

Fairly soon after our second daughter Shelley was born in 1955, my wife Miriam "Mika" Mark Salpeter went back to experimental research in neurobiology. Electron microscopes were becoming practical tools by the end of the 1950s and would be useful in studying synapses and other neurobiology structures that were rather too small for light microscopy. In the 1960s, Mika became an expert electron microscopist, and nominally a research associate in Cornell's engineering college, where Dale Corson was particularly sympathetic to female liberation, whereas biology was almost as male chauvinist as psychology (or Australian universities) before Cornell's Biology division was formed. My wife became particularly interested in the neuromuscular junction (NMJ), where the nerve releases quantal packets of about 10,000 acetylcholine molecules (ACh) each into a narrow space (the "primary cleft") facing the muscle surface. This surface has a high density of receptors for ACh on it. The ACh molecules diffuse in the cleft, get bound once or twice to the receptors, or get destroyed by esterases or unbind again, and so on. This process is rather reminiscent of the chain of nuclear reactions in a stellar interior, but here the main question is just how many doubly bound receptors cause ion channels to open up after some time delay. Such miniature endplate current pulses (MEPC) can be studied individually by electrophysiology, and their summation after a nerve impulse also triggers the muscle contraction.

Mika worked on various aspects of the NMJ throughout her life, because it is simpler and easier to manipulate than synapses in the central nervous system (CNS). In particular, a very stable snake toxin can be attached to the ACh receptors and can also be made radioactive. So, at least in principle, the receptors can be localized and their site density can be measured and also decreased in a controlled manner, and the reduced MEPCS can then be studied. My wife turned this principle into practice, especially by perfecting the technique of electron microscope autoradiography, where a photographic emulsion is first put on top of a thin biological section with radioactive receptor/toxin complexes (after some exposure time, the developed photographic grains are then visualized by electron microscopy). She elucidated many aspects of the NMJ (how a MEPC develops, the degradation and turnover of the receptors, the effect of myasthenia gravis, etc.), received several NIH career development awards, the Jacob Javits Distinguished Career Award, and was a professor in the Section of Neurobiology and Behavior and its Chair for two terms. I was involved in only three aspects of her multi-faceted career.

1. My first involvement may not sound very academic: Life was not made easy for a working mother with two children in those days. We usually had household help but there were emergencies, and I sometimes had to stay home and babysit. This had the advantage of my getting closer to my two daughters and of helping my wife's career indirectly, but what about its effect on my own career? Dealing with two children and working simultaneously tended to make my work even more qualitative than it would have been otherwise. This may have accentuated my sloppiness, but it also made it easier for me to switch fields rapidly and to work on several problems simultaneously.

2. The electron microscope has such a good intrinsic resolution that the scattering in the emulsion of the electrons released by radioactivity is the main bottleneck to localizing the source in autoradiography. My mentor and role model, Hans Bethe, had developed the theory of such scattering a long time before, and it was easy for me to calculate the scattering distribution function and the resolution for the autoradiographic technique. This was only one input into my wife's quantification of the technique, but it resulted in a long series of joint papers between 1969 (e.g., M.M. Salpeter et al. 1969) and 1978 (different distribution functions had to be calculated for different radioactive isotopes).

3. By the early 1980s, the electrophysiology of MEPCS under various conditions had got pretty quantitative (risetime, amplitude, falltime, shape, etc. could be measured), and it was interesting to carry out theoretical simulations of the time development of the current pulse. As mentioned, all this diffusing, binding, and unbinding is reminiscent of the nuclei in the interior of evolved stars. For a simplified geometry (nerve and muscle surface replaced by plane sheets), the development can be modeled by differential equations, and such calculations were carried out by research associate Bruce Land (Land et al. 1984). The actual space between nerve and muscle has a complicated three-dimensional geometry and a Monte Carlo code, "M Cell" for various simulations was developed by Tom Bartol and Joel Stiles to investigate the effect of geometric peculiarities (e.g., Stiles et al. 2001).

My wife Mika died tragically and unexpectedly in October 2000, and I had to take over running her lab. I also became the Principal Investigator for the remaining two years of her National Institute of Health research grant—not an easy task but at least made easier by my being a generalist already. The Monte Carlo code and the computers have become fast enough that one can now do an intensive parameter study. Such a study may sound esoteric, but two of the parameters (cleft width and receptor site density) are altered by a disease like myasthenia gravis, and a third parameter (esterase abundance) can be manipulated by a potential treatment for this disease. A parameter study can thus give some guidance for treatment variation. Tom Bartol and Bruce Land are just tooling up to do a similar (but lengthier) parameter study for CNS synapses.

BLACK HOLE ACCRETION

I attended the first Texas symposium in Dallas in December 1963 and was immersed in a lot of discussion and speculation on the recently discovered quasars. There were no detailed, realistic, rival models for the energy source of a quasar, but accretion of some matter onto some kind of black hole seemed to me like one plausible possibility. Soon after the Dallas symposium I explored one particular scenario for some hypothetical dense clusters, about ten times more massive than ordinary globular clusters but with similar velocity and location in disk galaxies like our own. While crossing the Galactic Disk, these objects would accrete interstellar matter and form massive black holes that would then accrete further interstellar gas (Salpeter 1964). What I did and did not do in that paper, and subsequently, may be a useful lesson for young players in the future, and I will describe it in some detail.

I am proud of three features in my 1964 paper: (*a*) I had to modify the Bondi-Hoyle-Lyttleton theory of accretion for motion through a uniform gas where standing shock fronts have to be considered (this exercise also kindled my interest in interstellar gas and dust on the one hand and in hydrodynamics on the other). (*b*) I needed to know the smallest radius of a circular orbit around a black hole where the orbit is still stable in General Relativity. It was long before Saul Teukolsky started a General Relativity group at Cornell, but I asked professional general relativists elsewhere for the answers. I got no answers, so I calculated it myself for that paper. However, to detract just a little from my glory, I have to add a qualifier: I mentioned my problem to Richard Feynman (who was not a relativist either) at a cocktail party and, without a moment's hesitation and without putting down his glass, he told me exactly how to start the calculation. (*c*) I showed that the increasing mass of the black hole, after reaching a critical value, would increase exponentially with a constant growth factor.

I am less than proud of having guessed wrong on two counts: (*a*) I had thought of condensed objects in the outer regions of disk galaxies, but densities are higher in the core of a galaxy, so a central location for black holes would have been worth exploring and is now known to be correct. (*b*) I guessed in 1964 that there would be many theoretical papers on black hole accretion very soon, with little observational information for a long time, and I just stopped working on the subject. In reality, there was only some independent and simultaneous Russian work (Zeldovich 1964) and then rather little interest until Lynden-Bell's paper on centrally located black holes in 1969 (Lynden-Bell 1969) and subsequent work by Martin Rees. My underestimating the potential of this subject also led to the worst advice I ever gave to any graduate student.

In 1965, Bruce Tarter had started on his own to work on the spiraling inwards of an accretion disk around a black hole, a subject surrounded by many uncertainties. I had told Bruce to stop working on such an unprofitable topic and to do a thesis on the interaction of X-rays with the interstellar medium, where one could hope for observational data soon. His work in this field was indeed well-received, (e.g., Tarter & Salpeter 1969), he became the director of the Livermore National Laboratory at a rather young age and generally did not suffer too much from my bad advice. Nevertheless, it took quite a while for others to get well beyond Bruce's unpublished work on accretion disks, and he might have had a more exciting youth without my timid, bad advice. Graduate students are too young to know what is impossible, so they will sometimes achieve the impossible. Such miracles may be rare, but a professor should think twice before discouraging his student.

THE INTERSTELLAR MEDIUM AND HYDRODYNAMICS

By the middle of the 1960s stellar structure and evolution had become a mature subject, but the interstellar medium was becoming a challenging field. By that time Tommy Gold, with his fertile imagination, had joined Cornell University and interstellar molecular hydrogen was one of his interests. We had early theoretical estimates of molecule formation on the surfaces of dust grains and of the abundance of H_2, before this molecule could be observed directly (Gould et al. 1963). Improvements to these calculations were carried out a few years later by Dave Hollenbach, Bill Watson, and Mike Werner. We were proud of the innovations in our 1963 paper, but even this paper was not innovative enough in one aspect: We did not stress that enormous variations could be expected in gas density and ultraviolet flux from one region to another, so we mainly discussed average conditions that lead to rather small predicted molecular abundances. We thus missed out on the extreme conditions that can lead to almost completely molecular clouds. John Bahcall and I speculated not just about interstellar matter, but also about intergalactic gas, and we predicted fairly sharp Lyα absorption lines in quasar spectra (Bahcall & Salpeter 1965, 1966). We guessed slightly wrong that HI would be clumped inside galaxy clusters, but at least it was a start. We were also able to show from observations that the fine structure constant, $\alpha = e^2/hc$, of quantum electrodynamics has changed by less than 5% per 10^{10} years, but that is a far cry from today's accuracy. Another, rare excursion far outside our own Galaxy in the 1960s had to do with cosmologies that used a cosmological constant to give a coasting period in the expansion (e.g., Petrosian et al. 1967b).

The upgrade in the 1970s of the Arecibo Radio Telescope enabled observations of interstellar HI through its 21-cm line and saw the beginning of my long collaboration with two radio astronomers, Yervant Terzian and John Dickey. In much of this work I only provided the theoretical underpinning (e.g., Dickey et al. 1979), but occasionally John Dickey managed to train me to do some actual observational work. The various phases of hydrogen in the Galactic disk and halo interact with interstellar dust grains, and the late 1970s also saw a lot of work on the physics of these grains, especially by Bruce Draine (e.g., Draine & Salpeter 1979).

Hydrodynamics impinges on astrophysics in many topics and the handover problem from radiative to convective heat transport in a star is one of them. An example of my long collaboration with Giora Shaviv is a paper on this topic (Shaviv & Salpeter 1973). Questions of solubility, nucleation of droplets, and gravitational separation of helium and hydrogen in the major planets are another topic (Salpeter 1973, Stevenson & Salpeter 1977). Energy production from gravitational layering is still a controversial question for Jovian planets and for white dwarf stars, as illustrated in some later work by Dave Stevenson. One speculative paper on hypothetical balloon animals floating and convecting in Jupiter's atmosphere was even published in an archival journal, in spite of references to some unusual forms of mating (Sagan & Salpeter 1976).

I have collaborated with Franco Pacini, on pulsars and exotic stars, since about 1968 (e.g., Pacini & Salpeter 1968). I also have benefitted greatly from my frequent trips to Arcetri Observatory (in Florence) and from stays at Cornell by many of Franco's younger Arcetri colleagues, including A. Natta, M. Salvati, G. Giovanardi, F. Palla, E. Corbelli, and P. Lenzuni.

The late 1970s also saw a lot of work on planetary nebulae where controversy and hydrodynamics overlap, and some work by Vic Mansfield on supernova remnants (Mansfield & Salpeter 1974). A similar overlap occurs in a series of papers on accretion onto neutron stars, starting with Shapiro & Salpeter (1975). In the 1980s and 1990s, my interests overlapped with those of Ira Wasserman, who was by then on the Cornell faculty. One result was a number of papers with two chiefs and one Indian, i.e., collaboration by both of us with one graduate student at a time. However, in some cases the real boss seemed to be the Indian, rather than either of the two chiefs (e.g., Bildsten et al. 1992).

GALAXIES AND DARK MATTER; NSF ASTRONOMY GRANTS, AND EPIDEMIOLOGY OF TUBERCULOSIS

As mentioned, by the late 1970s Arecibo was an ideal place for studying emission from HI gas, and the Virgo cluster of galaxies is conveniently located in the sky for the Arecibo fixed dish. I could not resist getting involved in Virgo cluster HI emission observations, at first with just one graduate student at a time, starting with Nathan Krumm and George Helou. We had some fairly early data on the nonplentiful gas in Sa galaxies (e.g., Krumm & Salpeter 1977) and then concentrated on the outermost HI disks of spiral galaxies and on the question of flat rotation curves in galaxies. Constant HI rotation velocities outside the star-containing galactic disk are very exciting because they indicate the presence of dark matter in this region of no stars and little gas—or else indicate some version of MOND, an alternative to Newtonian dynamics (e.g., Milgrom 1983). However, one can also get fooled by some fairly substantial sidelobes that the Arecibo beam had before the most recent upgrade. Instead of advising my graduate students to be doubly careful, as a professor should, I was sometimes gung-ho and urged rapid publication, which a professor should not do. As a consequence, at least one of our papers claimed a larger HI radius for some galaxies than is actually the case. Later observations by many groups on other disk galaxies still showed that the HI gas extends well beyond the stellar disk in many disk galaxies, so they are still exciting for studying dark matter halos and for the MOND controversy. Nevertheless, my indiscretion has meant that my armor has clanked somewhat ever since, just as Sir Gawain's armor did after his indiscretion in *Sir Gawain and the Green Knight*. I also employed the two chiefs and one Indian mode in the 1980s and 1990s with radio astronomy graduate students, making sure that the other chief was a genuine radio astronomy professor.

Because I was already an aging professor in the 1990s, it was in any case beneficial for each graduate student to also have a younger chief who would be around much later on: It is not always appreciated that, even quite a few years after a Ph.D., help from an ex–thesis-advisor is very useful. I have had enjoyable collaborations with Professors Terzian and Dickey on external galaxy observations, and a collaboration with Terzian is still continuing on observations of galaxy pairs that also give information on dark matter, although not uniquely (e.g., Nordgren et al. 1998). Another fruitful and continuing collaboration (with emphasis on dwarf galaxies) is with Professor Lyle Hoffman who combines a successful research career with teaching in a fairly small College.

The controversies on the distribution in the universe of dark matter (apart from its physical nature) has of course led to much theoretical work. I have had some fruitful collaborations with Jane Charlton and Edvige Corbelli on the related questions of Lyman-alpha clouds and the ionization-recombination equilibrium of hydrogen (e.g., Charlton et al. 1994, Corbelli et al. 2001). One dark matter controversy relates to the cosmological formation of large-scale structure and here the main stream and I diverged during the 1990s: Inspired in part by the success of three-dimensional stellar interior calculations, and encouraged by the ever-increasing speed of computers, the main stream theorists were carrying out elegant numerical three-dimensional structure formation calculations. These calculations became more and more precise, but still had to be based on a number of assumptions. I, on the other hand, was confused by what the assumptions should really be and was content with writing up toy models, conjectures, and speculations—some of them slightly outrageous. In a paper in honor of Antoinette de Vaucouleurs (Salpeter 1993), I even suggested that the outskirts of disk galaxies (dark matter and partially ionized hydrogen) might extend so far out that the rotation period is about one Hubbletime, so that the outer disk or Halo of Andromeda and of the Milky Way almost touch and provide some Lyman-alpha absorption. In the same paper I also speculated on "vanishing Cheshire Cat galaxies," where supernova-driven galactic fountains mostly evacuate the interior of a low surface density galaxy of its gas (Salpeter 1993). Only "the grin of the Cheshire Cat" is just barely detectable in the distant outskirts as gas.

My theoretical astrophysics grant proposals to the NSF were turned down in two successive years, partly because of the qualitative nature of my calculations. Fortunately, Fred Hoyle and I won the Crafoord Prize for nuclear astrophysics jointly in 1997; I am able to support my own theoretical work financially from a Crafoord Fund, and I have made no further NSF grant proposals since. It is probably reasonable not to give federal grants to old codgers and to leave more for main stream scientists, but I hope the main stream will at least listen to all the old codgers from time to time: The emperor's new clothes are usually described by the very young, but occasionally the very old might remind the main stream of some omissions. Possibly because of emphasis on elegance and beauty, most of the discussion in the 1990s of structure formation concentrated on regular galaxies and on giant galaxy clusters, both of which have beautiful, well-defined centers. This drew attention away from irregular structures like the Local Group and slightly larger galaxy groups—even though they far outnumber galaxies with a well-defined center in that luminosity range (not to mention dwarf irregular galaxies with ill-defined structure). When I talked to two junior (and young) NSF program officers, what hurt was not that I did not get the grant, but that these two sounded as though they themselves read papers only from the main stream and not from old codgers. I hope I misunderstood their attitude.

I had come full circle since I switched away from quantum electrodynamics because it was too mature a subject for my scientific temperament. Theoretical astrophysics had now become similarly mature, and I needed another subject that is more open-ended and controversial. Medicine and infectious diseases are of course not new, but the mathematical epidemiology of such diseases is still an open field, largely because there are so many uncertainties. My daughter Shelley is a physician and public health officer in California, where tuberculosis is a big problem. She is interested in the analysis of decisions between prophylactic chemotherapy, applied when an individual only has a latent infection and does not yet have the active disease, and other measures, applied only when patients have an active case of tuberculosis and can then infect others. The reproductive number of the disease is the average number of people who are first infected by a single person with the active disease and then themselves proceed from latent infection to active tuberculosis. This number is important for various cost-effectiveness studies, and we were able to estimate it for the USA from CDC data on active tuberculosis for the last 50 years and from skintest results for latent infection (Salpeter & Salpeter 1998).

My switch to epidemiology was not as radical a change as you might think—humans coughing tuberculosis mycobacteria into the air at different ages requires similar mathematical treatment as stars of different lifetimes disbursing heavy elements into the ISM do. It was easy to introduce a continuous distribution function for the delay time from latent infection to active tuberculosis that led to an improved calculation for the reproductive number. It is an eerie feeling to apply standard astrophysics techniques to medical problems and come up even with politically relevant results. For instance, we showed that the total probability for activation from latency is the same for all ethnicities prevalent in the USA, even though the infection rate and the active case rate is much larger for Asians than Whites. The next big epidemiological challenge for tuberculosis is its disastrous interaction with HIV/AIDS, especially in southern Africa where both diseases are increasing rapidly. Calculations here will be useful in the future but are hampered by sociological, as well as medical, uncertainties—e.g., do tuberculosis patients with HIV go to fewer parties so they do not cough up mycobacteria as much?

ODDS AND ENDS

Given the unstructured nature of my scientific career, I can't come up with a summary or resume either, but can add a few tidbits and gripes. Besides the summers with the nuclear physics group at Caltech, I have enjoyed a few sabbaticals and other visits, including a stay at Mount Wilson and Palomar offices in Pasadena in 1959—my one period surrounded by real optical observers. At an even earlier stage, I enjoyed overlapping with Vera Rubin (who was an undergraduate at Cornell), and Walter Baade and Jesse Greenstein were always an inspiration. In 1960 I spent time at my Alma Mater, Sydney University, with Stuart Butler, Harry Messel, and Bernie Mills. In 1961 (and again in 1969), I was an Overseas Fellow at Churchill College in Cambridge at the College's very beginning. The Fellows' meetings were most instructive, especially the superb administrative technique of Sir John Cockcroft, the first Master. The Fellows were young, the Master was very old and was often in the minority on issues but, without knowing how, we Fellows usually ended up voting with him. The trio of Herman Bondi, Fred Hoyle, and Tommy Gold provided both friendship and role models. The lively debates on Steady State versus Big Bang were always stimulating, with me favoring the Steady State. Many of us tried to salvage the Steady State from the onslaught of new data from the microwave background (e.g., Hazard & Salpeter 1969), but the data finally won. The champion for the Big Bang, Sir Martin Ryle, was also quite impressive but I resented the excessive secrecy that he imposed on all his "young men" regarding their observational results—conversations would have been more fun without that inhibition.

I have usually been pretty easygoing myself (I have been called a "half-assed equivocator") and made no waves. Consequently I have often been rewarded with getting more credit than I deserve, but I have a gripe because more controversial characters get too little credit. Ernst Öpik, who did many things first, and the Barnothys, who preached early about gravitational lensing, were mostly disregarded. Carl Sagan, who asked many of the important scientific questions before other people, was not disregarded but had detractors about mistakes in his scientific work. These detractors claim they are fair in quoting the mistakes, but everybody has made mistakes, even the Archangel Gabriel (and certainly I), and the detractors do not mention the Archangel's mistakes (not even mine)—so they are really not fair.

In looking back at my career, I don't even know whether to be proud or ashamed overall, but I am proud and gratified about the many graduate students, postdocs, and colleagues I have worked with. Although I never met him, I am grateful to my academic grandfather Arnold Sommerfeld, Hans Bethe's teacher, and I hope that some of my distant academic offsprings will remember me also. (I already have some academic great-grandchildren, mainly due to Hubert Reeves who started training students early.) What colleagues and students remember you for is more important than the published writing you leave behind. On the personal side, I particularly value my close relation to my children and grandchildren.

My friends tell me that I should not apologize for this essay being self-centered—that it is only natural. I apologize a little for being more morose than I usually am, but I am still in a state of shock from the loss of my wife. Apart from that, I have enjoyed my life and my wife's legacy sustains me still.

I also have enjoyed switching fields and even being sloppy. I've already shown how my astrophysics background has helped me to work in neurobiology and epidemiology, but the opposite is also true—or at least would be if I were a little younger. One example where epidemiology knowhow might have helped is my vanishing Cheshire Cat paper (Salpeter 1993) plus a forerunner speculation (Gerola et al. 1983). There we speculated that supernova-driven galactic winds in some galaxies might not only evacuate gas but, indirectly through dynamic effects, also lower the density of stars. Modern observations of dwarf spheroidals (Blitz & Robishaw 2000) are somewhat reminiscent of these two papers, but more complicated symbiotic relations must be involved. Such symbiotic relations are the bread and butter of the epidemiology of infectious diseases where humans and germs co-evolve—humans in crowded ghettos develop some immunity to tuberculosis, chemotherapy is beneficial but mycobacteria develop drug-resistance (with even more rapid evolution for HIV/AIDS). Experience in such biological co-evolution may yet help understand how gas clouds and low surface brightness galaxies interact.

I sometimes get asked how real neurobiologists or epidemiologists look at me, and I can answer that by showing how military officers looked at me long ago when I worked for the defense department as a part-time amateur: To go to Kwajalene Atoll I had to fly military air transport. I was first given a highly inflated simulated civil service rank, which was then translated into a highly inflated military rank—on that plane I was a simulated Major General! I bragged about this after my return until a friend remarked "Ed, to me you're a Major General, to your mother you're a Major General, but—BUT—to a Major General you're no Major General!"

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