Kenneth M. Case

1923–2006

BIOGRAPHICAL *Memoirs*

A Biographical Memoir by
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KENNETH MYRON **CASE**

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Elected to the NAS, 1975

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*By Paul F. Zweifel,
Norman J. McCormick,
and Laurie H. Case*

Kenneth Case made major scientific advances while at Harvard University, the Los Alamos Scientific Laboratory, the Institute of Advanced Study, the University of Rochester, the University of Michigan, and the Rockefeller University, and as a member of Jason (a longtime collaboration of eminent physicists).

While still an undergraduate, Case did significant research as a participant of the Manhattan Project at Los Alamos. After finishing his Ph.D. at Harvard he embarked on a lengthy and very broad research career that involved theoretical physics, applied and mathematical physics, and contributions to national defense.

Early years

Case was born in Brooklyn, NY. His father came from a struggling family of Ukrainian Jews that had emigrated to escape the pogroms. An ambitious man, Case's father used his chemical engineering degree to found a paint-manufacturing company that became successful. Soon the family moved to Manhattan and an apartment overlooking Central Park.

Case's mother came from a more affluent and self-consciously cultured background particular to German and Austrian Jews. She had studied to be a schoolteacher and brought the full force of her training to bear upon her only child. As soon as he learned to read, she began systematic recitations of Shakespeare's plays, a project completed before he began kindergarten.

For grade school Case attended the School of Ethical Culture and later the affiliated high school, Fieldston. As an adult he relished in a rousing rendition of his alma mater's fight song—"Pure as a lily/Strong as a vulture/Rip rah ray for Ethical Culture!" Like many of his classmates, he was brought up as a Jew in the tradition of his mother's Viennese family. Their Judaism was interlaced with a passion for European culture and learning.

Case rarely spoke of his professional accomplishments, but toward the end of his life he expressed satisfaction with having calculated the yield of the first atomic bomb tested at the Trinity site. His estimate was between 15 and 20 equivalent kilotons of TNT. In the end it was measured at 18 kilotons.

His Hebrew was minimal. The family did not keep kosher but pork was avoided. Even as a boy, Case was faced with the morality of choice.

Growing up in Manhattan delighted him. He loved Broadway musicals and eating cheesecake at Lindy's. For Case, New York in the years before World War II was the epitome of romance. Yet he was equally happy at the family's weekend house in Connecticut, where he spent hours tending the vegetable garden. Gardening became a pastime that he would favor the rest of his life.

Case's parents had great ambitions for him. His precocity evident early on, they were determined that he have access to the best education. Aware of the quota system within the Ivy League, which kept the admission rate of Jews to 10 percent, his parents decided to change the family name from Cassoff to Case. He was accepted to Harvard, though it is impossible to know whether the name change had been necessary.

Harvard and Los Alamos

Case's plan was to study chemical engineering, like his father. But almost immediately he fell under the spell of physics. He entered Harvard in the autumn of 1941—being ineligible for the draft because of extreme myopia—and he remembered his time as an undergraduate there with affection, especially when some of his professors left for MIT to work on the development of radar and he and other able students were asked to help teach the lower-level physics classes, something he found delightful.

In late 1943 Case was approached about working on an unidentified project for the military. In January he and a fellow student, Frederic de Hoffmann, took a train to Lamy, NM, the closest rail stop to the remote town of Los Alamos. (De Hoffmann became one of Case's closest friends; in fact, they were best man at each other's wedding.) Case soon was introduced to Manhattan Project director J. Robert Oppenheimer, a meeting that proved to be indelible in his memory—at 21 years of age he found himself working with the most illustrious physicists of his day! Often the days were long, with table tennis and movies the major diversions. He also ate some of his meals with Klaus Fuchs, but without an inkling of the espionage afoot.

For the year and a half that Case worked at Los Alamos, he shared an office with Roy Glauber, a future Nobel laureate and also a fellow student at Harvard. Glauber described their work (1) in an email:

> *I remember [what] Ken and I were doing as part of Robert Serber's group at Los Alamos. We worked quite separately on fairly similar problems in neutron diffusion. They required finding stationary distributions in spherical media, usually a uranium or plutonium sphere surrounded by a heavy metal tamper in order to reflect as much of the neutron density as possible back into the sphere. The distributions [that] were exponentially multiplying in time could all be found in terms of equivalent static distributions with different parameters. We were not involved in the hydrodynamical aspects of what followed at all. There were many variants on this simple theme consisting of concentric shells…and I recall Ken's working on many of them. My own work was principally devoted to working out new methods of solving these problems using fancy versions of the Boltzmann equation and the like. Ken, I remember, did work some on slowing-down theory. I did not. The 1953 book by Case, de Hoffmann, and Placzek (2) was a return by Ken to work like what he did at Los Alamos and a partial abandonment of field theory.*

Case rarely spoke of his professional accomplishments, but toward the end of his life he expressed satisfaction with having calculated the yield of the first atomic bomb tested at the Trinity site. His estimate was between 15 and 20 equivalent kilotons of TNT. In the end it was measured at 18 kilotons. Later he said this "was exact as far as I am concerned." (3) Case's experience of contributing to the larger cause of national security had an enduring impact on him, influencing his decisions in the years to come.

With the end of World War II, Case and de Hoffmann returned to Harvard where they were summarily graduated. By the calculus of the university, their experience on the Manhattan Project amounted to two years of undergraduate education. Case immediately began work on his Ph.D. at Harvard under Julian Schwinger. He received his degree three years later, in 1948. His dissertation ("The Magnetic Moments of the Neutron and Proton") was published as a short letter to the editor of *Physical Review.* (4)

De Hoffmann received his Ph.D. under Schwinger at the same time as Case did (while Glauber's was awarded a year later). De Hoffmann went on to become the founder and first president of General Atomic (a branch of General Dynamics Corp.) and later the president of the Salk Institute.

Institute for Advanced Study and the University of Rochester

After finishing his Ph.D. in 1948, Case accepted a postdoctoral position at the Institute for Advanced Study (IAS), then under the directorship of Oppenheimer. This appointment lasted for two academic years, with the summers of 1949 and 1950 spent at Berkeley, CA, where he served as a research associate at the Radiation Laboratory.

During 1949, Case's first year at the IAS, he presented a paper at the annual American Physical Society meeting. (5) In this work he proved what came to be called "Case's theorem," which asserted that two different interactions (so-called pseudo-scalar and pseudo-vector couplings) gave the same result (to lowest order in perturbation theory). A problem arose, however, because Murray Slotnick had described his Ph.D. thesis at the same meeting the day before Case was scheduled to speak. Slotnick had found finite results for the pseudo-scalar case, but infinite results for the pseudo-vector one. (6) Oppenheimer was in the audience and challenged Slotnick's results as "violating Case's theorem." (It is unknown why Case was not present.) Richard Feynman also was there, and he spent the whole night repeating Slotnick's calculations using some of the new methods he was in the process of developing. He proved that Slotnick was correct and challenged Case the next day, after Case's talk. Feynman, using his new methods, had repeated in one night a calculation that had taken Slotnick six months, and he had done it with more generality—Slotnick assumed zero momentum transfer, but Feynman did not. (7)

The penultimate paragraph of Case's corrected paper (5) read: "Thanks are due to Dr. R. P. Feynman for pointing out an error in the original manuscript."

In the years following WWII, physics was a heroic calling. Being a scientist was alluring and romantic. This mystique was not lost on Pat Carpenter, a gregarious brunette who oversaw the food service at the Institute, which included serving tea in the afternoon for Albert Einstein. Ken and Pat became romantically involved early in Ken's IAS postdoc, and the romance continued for his two years at the Institute and during his postdoc the following year at the University of Rochester.

Case was eager to begin his academic career, and he accepted a job offered to him by the University of Michigan in 1951, albeit in the chemistry department. Ironically, the same Slotnick involved in the Case theorem business got the job in the physics department instead of Case, but the next autumn he drowned in a boating accident. As Case and Slotnick had become good friends by then, the physics department assigned Ken and Pat the heart-wrenching task of notifying Slotnick's mother of his death. Case also helped write Slotnick's obituary.

In addition to the satisfaction of having a real job, compared with a fellowship, Case was happy to leave the IAS as he was anxious to escape the ongoing debate between Oppenheimer and Edward Teller about the making of the hydrogen bomb. He may not have known that even as they were speaking the IAS computer was busy making calculations for the first thermonuclear weapon. (8)

Case recalled sitting in a room at the Institute as Teller and Oppenheimer debated, thinking "I am going to the University of Michigan." But first he had to complete the postdoctoral year at the University of Rochester. According to Norman Francis, a graduate student at Rochester that year, Case and (future Nobel laureate) C. N. Yang shared an office where they were studying spin-orbit coupling. (9) This topic may have been engendered by Case's summers at Berkeley, where nucleon-scattering experiments were all the rage, as evident from one of his early papers. Francis was impressed with Case's popularity among the Rochester graduate students, both for his pleasant personality and his willingness to help whoever was trapped in a difficult computation. (10)

In the autumn of 1950, Case proposed to Pat. They were married the following August 19 at Temple Emanu-El in New York City. The 27 years that followed were undoubtedly the happiest of his life. They were enormously productive for Case professionally as well.

Also in 1950, Case created a stir in the theoretical physics community with a paper based on research he had done at the IAS. Involving his study of singular potentials, (11) the paper was cited at least 412 times. Eugen Merzbacher, who followed Case by two years at Harvard as a Schwinger Ph.D. and who spent the academic year 1950–51 at the IAS, pointed out that singular potentials was a hot topic there at the time. (12) In particular, Merzbacher said, everyone was trying to understand the Case paper! The physical application Case suggested, the spectra of spin zero and one-half particles moving in the fields of highly-charged nuclei, was what interested physicists. The mathematical problem was the fact that the singular potential made the Hamiltonian operator non-self-adjoint. Case found a way to enforce self-adjointness using orthogonality relations. He mentioned in the paper that his results were of particular interest in calculating scattering processes, especially resonances.

From fundamental to applied research

Case ultimately began trying to transform himself from a particle physicist into an applied mathematician, applied physicist, or mathematical physicist. Or perhaps all three. One of his last papers along "fundamental physics" lines (13) was a very important work (14) cited at least 340 times. Here is a summary of John Wilkerson's comments:

I would agree it is an important paper. It was published shortly after the discovery of parity violation in weak interactions [by Yang and Lee], where there was a hint, but not yet a general understanding (agreement), that the interactions were V-A. It provides a Majorana theory formalism for both parity-conserving and -violating interactions (or a mixture) that is cited by other important papers and authors over the next 30 years.(15)

Later in his career Case apparently seemed to regret (somewhat bitterly) his decision to move away from fundamental physics, going so far as to advise an unnamed young particle physicist—in the throes of making the same transition— not to do it. This advice was ignored, and that physicist went on to become a MacArthur Fellow and winner of the E. O. Lawrence Medal and the Wolf Prize, among other honors he received.

A possible explanation of Case's great paradigm shift may lie in a statement he wrote in (5), "For generality, the proof is carried out using the Schwinger-Tomonaga many-time formalism." The early approach to quantum electrodynamics (aka QED, the interaction of light with matter) was that of Schwinger-Tomonaga; and the later approach was the Feynman-Dyson method that Feynman developed in 1948-49 and used in the "Case theorem" incident discussed previously. (Dyson proved the equivalence of the two methods.) Tomonaga, Schwinger, and Feynman shared the 1965 Nobel Prize in physics. (As so often happens, the Nobel rule that a prize can be divided among at most three laureates made the more-than-worthy Freeman Dyson ineligible.)

The important point is that the Schwinger-Tomonaga approach is virtually incomprehensible, even to the most brilliant physicists. Schwinger's students learned it while they were in the process of writing their dissertations, but by and large they abandoned it for the much easier to understand and more physical Feynman-Dyson approach. The Feynman-Dyson method was based on what are now called Feynman graphs (or diagrams), which give a concrete visualization of the physical processes that the photons, electrons, and positrons are actually undergoing. These graphs can be converted to integrals by some simple rules that one can, after considerable work, learn how to evaluate and thus arrive at the final answer. But as the physical problems encountered in fundamental research grew harder, it became more and more difficult for Case to use the old-fashioned methods of Schwinger so in a sense the mainstream of particle physics just passed him by. Frank Close (16) has described this trend quite explicitly:

> *Schwinger's encyclopedic formalism had attracted all the plaudits, though Dyson alone seems to have plumbed its full depths. Feynman's pictogram scheme was much easier to apply and would become the staple diet of students forevermore.*

University of Michigan and Rockefeller University

Case's gradual shift from fundamental to applied problems took place over his first 10 years or so at Michigan. But during this transition he produced some important fundamental work carried out in support of the experimental physicists at Michigan who were attempting an accurate measurement of the gyromagnetic ratio of the free electron (and, as a spinoff, the evaluation of the fine-structure constant α). In two successive papers Case, along with one of his students, provided a theoretical analysis of the analytical basis of the experiment (17) for the calculations done in (18).

Before turning to a discussion of Case's interest in more applied problems, there is one more paper on fundamental physics that should be mentioned. (19) This paper possibly may have been the spark that ignited the introduction of strangeness as a quantum number in particle physics and that eventually led to the classification of elementary particles by the group SU(3). Also interesting is that the work was done at Berkeley, presumably during a summer visit (as all three authors listed permanent addresses elsewhere).

A major indication of Case's new *modus operandi* came in 1957, with the publication of (20). His institutions are given as General Atomic (GA) and the IAS, with a permanent address given in a footnote as the Physics Department, University of Michigan. The topic of the paper was along the lines of GA's interest in atomic energy, with the formalism couched in neutron transport terms, although it is stated in the first sentence that the paper also is applicable to radiative equilibrium. One can reasonably assume that this research was carried out during a sabbatical year at the IAS. We recall that Case's old friend Freddy de Hoffmann was the founding president of General Atomic, and from personal discussions with Case we know that de Hoffmann for some years was after him to leave academia and relocate to GA—an offer that was seriously considered but eventually turned down.

The gist of (20) is stated on the first page, where Eq. (1) is the equation of one-speed neutron transport:

> *While (1) is, without simplifying assumptions, exceedingly complex, a few general statements can be made. These concern the reciprocity principle and questions of uniqueness. The first of these is most important. Besides enabling us to compare different experimental situations and simplifying much of the mathematics, it shows, it will be seen, how apparently difficult problems can be solved by relating them to simpler ones. Unfortunately, even the most elegant proofs have been rather complex."*

At this point there is a reference to the work of Chandrasekhar (21), perhaps the world's most eminent theoretical astrophysicist and a future Nobel Laureate. Case always felt the Chandrasekhar approach to be overly complicated and counterintuitive because in (20) he obtained equations more general than those of Chandrasekhar by using simpler and more rigorous arguments.

There is much more in this paper that can still be read profitably today by practitioners of transport theory. Alas, we have attended a number of meetings where presenters had no idea of uniqueness per se or the important principles hidden in what mathematicians call the Fredholm alternative, which actually underlies Case's work.

Case's interests then largely turned to what might be called "transport theory," involving the (nonlinear) Boltzmann equation that describes the evolution toward equilibrium in a gas. This equation had been around since the turn of the 20th century and can be used to describe, mutatis mutandis, the motion of photons in solar and planetary atmospheres, waves in plasmas, and other related phenomena. Approximate solutions initially were obtained using methods similar to so-called diffusion theory, often used to describe such disparate phenomena as heat flow and the motion of neutrons in a nuclear reactor. More accurate solutions were sought, and the mathematicians Norbert Wiener and Eberhard Hopf succeeded in finding such a technique, to which their names are now attached. Physicists and engineers found this approach daunting, as it depended on mathematical techniques with which they were not completely familiar.

A number of transport theorists sought more familiar methods of analysis—those used in treating partial differential equations (PDEs)—to apply to the integro-differential transport equation. The first advance in this direction came from Boris Davison, who applied a separation-of-variables technique followed by a sort of eigenfunction expansion, the standard PDE approach. (22) This method, while suggestive, had some disadvantages: the spectrum of the separated variable operator consisted of the union of two disjoint semi-infinite intervals on the real line; the associated eigensolutions were not functions but Schwartz distributions (e.g., containing Dirac delta distributions); and only infinite-medium problems could be solved. Nonetheless, the method showed some promise, and Eugene Wigner did further work along these lines. (23) Although this paper appeared in 1961, its presentation at the Symposium predated Case's seminal 1960 paper (to be discussed later). We believe that Case was in attendance and heard Wigner's presentation, because one of us (PZ) recalls Case discussing that talk prior to publication of his 1960 paper.

Around this time, Case also became interested in plasma waves. In 1946 Lev Landau had studied the stability of longitudinal waves in plasmas, and using a Laplace transform analysis of the linearized Vlasov equation he showed that the wave amplitudes under certain physical conditions decayed in time—i.e., "Landau damping." (24) Also, in 1955, N. G. van Kampen had obtained Landau's results by a method eerily similar to the eigensolution expansions of Davison and Wigner for the neutron transport equation. (25) In a classic paper, Case studied the two seemingly disparate approaches and showed that they led to the same results. (26) It is worth quoting the first paragraph of this paper:

> *The initial value problem for an electronic plasma has been solved by two strikingly different methods. Landau (1) has given a solution using a Laplace transform technique. Van Kampen (2) has solved the problem by means of a normal mode expansion. It is interesting to see the complete identity of the solutions. This is shown below....The results obtained...indicate that many of the classical completeness and orthogonality theorems hold for quite pathological operators. [Case's references (1) and (2) are our notes (24) and (25).]*

Case's important innovation in this paper was the introduction of orthogonality relations, a far-from-obvious idea because, as has been noted above, the eigensolutions were Schwartz distributions, not like ordinary functions (think Fourier series).

We now turn to Case's 1960 chef-d'oeuvre, (27) a paper that garnered at least 485 citations. Recalling (22–26) we note that a parameter, usually denoted by ν, entered the solution and was sometimes referred to as an "eigenvalue." Actually, it was (and is) the separation constant introduced when the classic separation of variables is used. Typically a case of one-speed neutron transport—with the independent variables of position

x ($0 < x < \infty$) and direction cosine of the velocity μ—is considered. The parameter ν in the earlier work took on values (we may as well misname them "eigenvalues") on a set of the real line consisting of two semi-infinite disjoint intervals. This caused difficulties because to evaluate the solution it was necessary to integrate around the set.

Case brilliantly overcame these difficulties by calling the separation constant $1/\nu$ instead of ν, thus transferring the disjoint eigenvalues to the much more manageable interval $[-1,1]$. He also set out to solve the transport equation for a semi-infinite medium, as noted above, while all the earlier work had been restricted to infinite media. It might be argued that semi-infinite media are no more prevalent in physical applications than infinite media, but remember that the earliest applications of transport theory involved photons diffusing in stellar atmospheres. Stars are so immense that they can, with reasonable accuracy, be represented by semi-infinite media. Similar remarks apply to other physical applications, such as plutonium-production reactors and light diffusion in oceans.

Having overcome the problem of the location of the eigenvalues, it remained to construct the solution of the transport equation using only positive ν eigenmodes because those associated with negative ν diverged at infinity. Case intuited, and was able to prove, what he called the "half-range completeness theorem." This asserted that any function defined on the "half range" ($0 < \mu < 1$) could be expanded in half of the eigenmodes, those corresponding to positive ν. (Positive μ entered the boundary condition at $x = 0$, since it was assumed that the influx of particles—neutrons, photons, or whatever—was specified at the boundary of the half space at $x = 0$ for incoming directions.)

This half-range completeness theorem is far from intuitive. The analog in the case of Fourier series would be that a function defined for positive x, say, could be expanded in terms of the positive Fourier components alone, which is not the case. So the introduction of this concept required a far-reaching imagination on Case's part, as described elsewhere. (28)

To complete the solution, a singular integral equation had to be solved. Such an equation closely resembles a Fredholm integral equation, except that the integral is Cauchy principal value. Case learned how to solve these equations from a book that recently had been translated from Russian. (29)

If the truth be told, the Case approach involved many of the complexities of the Wiener-Hopf method discussed earlier, except that it did avoid having to study the analytical domains of sectionally holomorphic Fourier transforms in the complex plane, which is a huge plus. But all in all, it was much more comprehensible to physicists and engineers than the older methods because it was based on the familiar separation of variables. At least 150 papers directly utilized Case's approach by 1973 (30), including the solution of problems not tackled with the Chandrasekhar (21) and Wiener-Hopf approaches.

In his paper (27), Case stated his hope that exact solutions of simplified problems could serve as benchmarks for numerical problems (this never really happened) and that his solutions might give insight into the mathematical structure of transport theory (this certainly came to pass). In (28) there is a nonexhaustive list of 17 areas of mathematics, physics, and engineering in which Case eigenmodes have been applied. They range from ordinary neutron transport to lattice spin systems (by Case himself) and relativistic electron dynamics.

A major development that simplified the solution of half-space problems was the discovery of simple half-range orthogonality relations that allowed the problem to be solved by multiplying and integrating, just as in the case of Fourier series. (31) After one of the authors (PZ) arrived at Michigan in September 1958 as a faculty member in nuclear engineering, it wasn't too long before he began working with Case, even though they were in different departments. They wrote some papers together, but their main work was a monograph. (32) The book was almost finished when (31) appeared, but they rewrote the appropriate sections to describe this exciting development.

One reason why Case and PZ hit it off as fast as they did may be that PZ not only understood what he was doing but also appreciated it. Unfortunately, many particle physicists of the time did not. This attitude may have helped Case in his decision in 1969 to leave Michigan for Rockefeller University—where he was reunited with George Uhlenbeck, one of his Michigan colleagues—instead of joining the mathematics department at California Institute of Technology. The Cases lived in Princeton during those years; he spent three days a week at Rockefeller and worked at the IAS the other two. Case was a perfect fit at Rockefeller because of his expertise in statistical mechanics (whose spiritual leader was Uhlenbeck) and particle physics (an impressive group led by experimentalist Rodney Cool). We should also mention the distinguished mathematicians Mark Kac, Case's principal collaborator, and James Glimm, who were at Rockefeller for a portion of his tenure there.

An indication of how Case's research interests shifted gears can be found from his selected bibliography, which is a portion of his 111 publications. (33) Prior to 1957 most of his papers appeared in *Physical Review*, the journal of choice for fundamental physics topics. After that date he tended to favor journals such as *Annals of Physics*, *Journal of Mathematical Physics*, and *Physics of Fluids*, all of which dealt with more applied topics or pedagogical subjects. Although Ken published fewer open-literature papers during his time at Rockefeller University and the IAS, it is likely he was publishing classified work in conjunction with Jason.

Jason

In 1960, a hush-hush organization of scientists (mainly theoretical physicists) was formed to meet for six weeks every summer in various locations in order to give the U.S. government advice on scientific aspects of defense matters. This group, called Jason, (3) has continued meeting ever since and eventually found a permanent home in La Jolla, California. Case was a member from 1961 until the early 2000s.

The Jasons were a stellar group of physicists from universities across the country, many of whom had known each other from very early in their careers. As a result of their annual six weeks of study, the men (they were all men in those days) developed a bond that lasted throughout their lives. Membership in the group also was an intensely social experience. The summer studies took place at vacation spots, sometimes Cape Cod, but more often La Jolla. The wives and children spent their days at the beach and in the evenings there was a continual round of cocktail and dinner parties.

A huge success of the Jasons was their development of so-called "adaptive optics"—resulting from their search for a method to correct the distortion of light passing through the turbulent atmosphere in order to improve telescopes' detection of Soviet spy satellites. Some Jasons came up with a way to deform the mirrors in the telescopes so as to compensate for the distortion, and it actually worked—and worked well. Adaptive optics now has evolved to the point that astronomers use it to study the formation of black holes in the early universe!

Case's involvement with Jason was deeply important to him, both professionally and personally. Although he loved the academy and the pursuit of scientific knowledge for its own sake, his experience with the Manhattan Project had instilled in him a passionate commitment to the ways in which scientists, and especially physicists, could help shape American policy, notably in the area of defense. He relished the six-week summer gathering and the smaller meetings in spring and autumn. Case and his colleagues in Jason reveled in their intellectual freedom to choose what projects interested them and to express their opinions to the Department of Defense (DOD) without censorship. Case felt that in this way Jason performed an invaluable service as a check on defense policy. Perhaps one of the best instances of this service was the Jasons' conclusion that Ronald Reagan's "Star Wars" program was utter nonsense.

Working as a consultant to the DOD could be politically tricky, however, and even morally dubious in the eyes of some. This was particularly so during the Vietnam War, when the Jasons were helping the government develop strategies for dealing with guerrilla warfare. Case was involved in developing the so-called electronic barrier, which was designed to halt the movement from north to south along the Ho Chi Minh Trail. One of us (LC) has memories of her father going down to Florida and flying around in a military helicopter to check out some sort of prototype of the electronic barrier. Essentially a movement detector, this barrier was ultimately declared a failure. But Case continued to support the idea, which he felt had failed because it had not been implemented on a large-enough scale. He even allowed himself to be interviewed by *Science*, and proudly claimed that the detector was so sensitive it "could hear a soldier peeing." (34) Incidentally, the Jasons also proposed that the electronic barrier be used to stop the flow of illegal drugs into the United States, a project as fruitless as closing the Ho Chi Minh Trail.

Later, Case became one of about 10 Jasons with a top security clearance from the Navy. It was during this period that he began his work on understanding the action of soliton waves (which fail to dissipate). The exact nature of this work remains classified, but it was part of a project aimed at understanding how to detect the presence of a nuclear submarine or, conversely, prevent such a submarine from being detected. Many of Case's later papers were most likely inspired by work he carried out for Jason; a good example is (35).

Most of the Cases' closest friends derived from the families of Jasons: Murph Goldberger, Ed Frieman, Ken Watson, Joel Bengston, and Marshall Rosenbluth, to name but a few.

Rerirement, UCSD, and Rockefeller

It was because of these friendships that the Cases chose to spend their retirement in La Jolla, where many Jasons had taken up residence. An added plus would be its relative proximity to their daughter Laurie and their two grandsons, who lived in Berkeley.

Thus when Case retired from Rockefeller University in 1988, he and Pat sold their house in Princeton and moved to La Jolla, to a lovely home overlooking the ocean. It had a huge back yard where Ken spent many happy hours gardening.

Retirement did not end Case's research career. He returned to Rockefeller University every year, for one month in the spring and another in the fall, until 2002. During these visits he carried out research on various contracts and grants that Rockefeller was administering. He also served as an adjunct professor at the UC San Diego Institute of Nonlinear Studies until his death in La Jolla, California on February 1, 2006.

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NOTES

1. Glauber, R. 2012. Personal communication, September 28.
2. Case, K. M., F. de Hoffmann, and G. Placzek. 1954. I*ntroduction to the theory of neutron diffusion, Vol. I*. Washington, DC: U.S. Government Printing Office.
3. Finkbeiner, A. 2006. *The Jasons: The secret history of science's postwar elite*. New York: Penguin Books.
4. Case, K. M. 1948. The magnetic moments of the neutron and proton. *Physical Review* 74:1884–1885.
5. Case, K. M. 1949. Equivalence theorems for meson-nucleon coupling. *Physical Review* 76:14–17.
6. Slotnick, M. 1949. Minutes of the 1948 Annual Meeting in New York, Jan 26-29, 1945. *Physical Review* 75:1295.
7. Schweber, S. S. 1994. *QED and the men who made it*. Princeton, NJ: Princeton University Press.
8. Dyson, G. 2012. *Turing's Cathedral: The origins of the digital universe*. New York: Pantheon Books.
9. Case, K. M. 1950. On spin-orbit interaction and nucleon-nucleon scattering. *Physical Review* 80:203–211.
10. Francis, N. 2013. Personal communication, February 20.
11. Case, K. M. 1950. Singular potentials. *Physical Review* 80:797–806.
12. Merzbacher, E. 2012. Personal communication, August 30.
13. Case, K. M. 1957. Reformulation of the Majorana theory of the neutrino. *Physical Review* 107:307–316.
14. Cohen, E. G. D. 2012. Personal communication, September 20.
15. Wilkerson, J. 2012. Personal communication, September 10.
16. Close, F. 2011. *The infinity puzzle*. New York: Basic Books.
17. Case, K. M. 1954. Some generalizations of the Foldy-Wouthuysen transformation. *Physical Review* 95:1323–1328.
18. Mendlowitz, H., and K. M. Case. 1955. Double scattering of electrons with magnetic interaction. *Physical Review* 97:33–38.
19. Case, K. M., R. Karplus, and C. N. Yang. 1956. Strange particles and conservation of isotopic spin. *Physical Review* 101:874–876.

20. Case, K. M. 1957. Transfer problems and the reciprocity principle. *Reviews of Modern Physics* 29:651–663.

21. Chandrasekhar, S. 1950. *Radiative transfer*. New York: Oxford University Press.

22. Davison, B. 1957. *Neutron transport theory*. New York: Oxford University Press.

23. Wigner, E. P. 1961. Mathematical problems of nuclear reactor theory. In *Proceedings of symposia in applied mathematics, Volume XI, Nuclear reactor theory,* G. Birkhoff and E. P. Wigner (eds.), 89–104. Providence, RI: American Mathematical Society.

24. Landau, L. 1946. On the vibrations of the electronic plasma. *Journal of Physics USSR* 10:25–34.

25. Van Kampen, N. G. 1955. On the theory of stationary waves in plasmas. *Physica* 21:949–963.

26 Case, K. M. Plasma oscillations. 1959. *Annals of Physics* 7:349–364.

27. Case, K. M. 1960. Elementary solutions of the transport equation and their applications. *Annals of Physics* 9:1–23.

28. Zweifel, P. F. 2012. Kenneth Case and his singular "eigenfunctions." *Transport Theory and Statistical Physics* 41:406–417.

29. Mushkhelishvili, N. I. 1953. *Singular integral equations*. Gröningen, Germany: P. Noordhoff.

30. McCormick, N. J., and I. Kuščer. 1973. Singular eigenfunction expansions in neutron transport theory. In *Advances in nuclear science and technology*, Vol. 7, E. J. Henley and J. Lewins (eds.), 181–282. New York: Academic Press.

31. Kuščer, I., N. J. McCormick, and G. C. Summerfield. 1964. Orthogonality of Case's eigenfunctions in one-speed transport theory. *Annals of Physics* 30:411–423.

32. Case, K. M., and P. F. Zweifel. 1967. *Linear transport theory*. Reading, MA: Addison-Wesley.

33. Web of Science listing for Case KM (as of May 23, 2013).

34. Kali Tal. Online at http://www2.iath.virginia.edu/sixties/HTML_docs/Texts/Scholarly/Tal_Jason.html

35. Case, K. M. 1978. The N-soliton solution of the Benjamin-Ono equation. *Proceedings of the National Academy of Sciences U. S. A* 75:3562–3563.

SELECTED BIBLIOGRAPHY

1949 On nucleon moments and the neutron-electron interaction. *Physical Review* 76:1–13.

Equivalence theorems for meson-nucleon couplings. *Physical Review* 76:14–17.

1950 With A. Pais. On spin-orbit interactions and nucleon-nucleon scattering. *Physical Review* 80:203–211.

Singular potentials. *Physical Review* 80:797–806.

1951 With K. A. Brueckner. Neutral photomeson production and nucleon isobars. *Physical Review* 83:1141–1147.

1953 With F. de Hoffmann and G. Placzek. *Introduction to the theory of neutron diffusion, Vol. I.* Washington, DC: U.S. Government Printing Office.

1954 Some generalizations of the Foldy-Wouthuysen transformation. *Physical Review* 95:1323–1328.

1955 With H. Mendlowitz. Double scattering of electrons with magnetic interaction. *Physical Review* 97:33–38.

Biquadratic spinor identities. *Physical Review* 97:810–823.

Hamiltonian form of integral spin wave equations. *Physical Review* 100:1513–1514.

1956 With R. Karplus and C. N. Yang. Strange particles and the conservation of isotopic spin. *Physical Review* 101:874–876.

With P. A. Moldauer. Properties of half-integral spin Dirac-Fierz-Pauli particles. *Physical Review* 102:279–285.

Pais-Piccioni experiment. *Physical Review* 103:1449–1453.

1957 Reformulation of the Majorana theory of the neutrino. *Physical Review* 107:307–316.

Transfer problems and the reciprocity principle. *Reviews of Modern Physics* 29:651–663.

1959 Plasma Oscillations. *Annals of Physics* 7:349-364

1960 Elementary solutions of the transport equation and their applications. *Annals of Physics* 9:1–23.

1962 With S. G. Gasiorowicz. Can massless particles be charged? *Physical Review* 125:1055–1058.

With R. Aamodt, M. Rosenbaum, and P. F. Zweifel. Quasi-classical treatment of neutron scattering. *Physical Review* 126:1165–1167.

1963 With P. F. Zweifel. Existence and uniqueness theorems for the neutron transport equation. *Journal of Mathematical Physics* 4:1376–1385.

1966 Singular solutions of certain integral equations. *Journal of Mathematical Physics* 7:2125–2124.

1967 With P. F. Zweifel. *Linear transport theory.* Reading, MA: Addison-Wesley.

1972 On fluctuation-dissipation theorems. *Transport Theory and Statistical Physics* 2:129–176.

1973 Inverse problem in transport theory. *Physics of Fluids* 16:1607–1611.

1976 Fredholm determinants and multiple solitons. *Journal of Mathematical Physics* 17:1703–1706.

1978 Integration of linearized evolution equations. *Physical Review Letters* 40:351–354.

The N-soliton solution of the Benjamin-Ono equation. *Proceedings of the National Academy of Sciences U. S. A.* 75:3562–3563.

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