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OWEN CHAMBERLAIN
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A Biographical Memoir by

HERBERT STEINER

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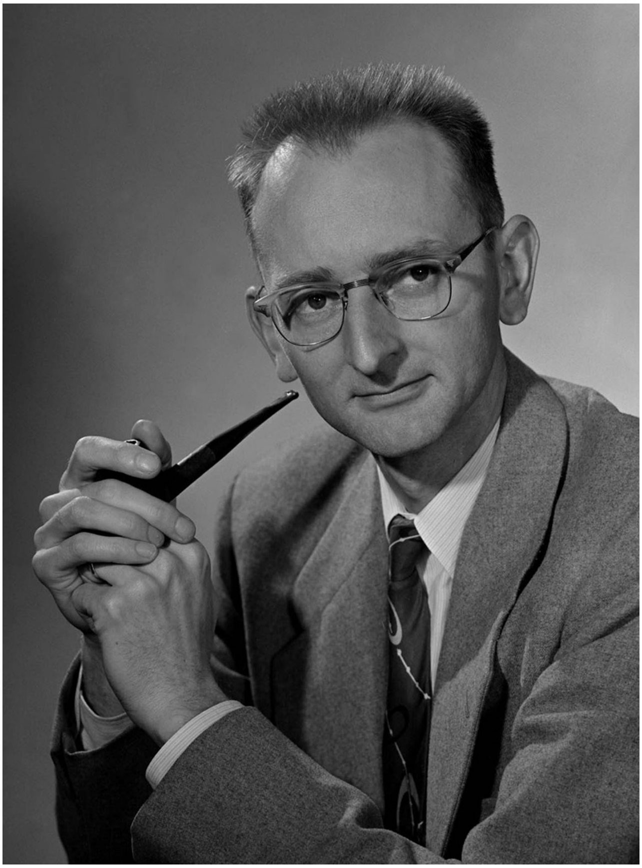
Biographical Memoir



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Owen Chamberlain

OWEN CHAMBERLAIN

July 10,1920–February 28, 2006

BY HERBERT STEINER

OWEN CHAMBERLAIN, WHO SHARED THE 1959 NOBEL PRIZE in Physics with Emilio Segrè for the discovery of the antiproton, died of complications from Parkinson's disease at his home in Berkeley, California, on February 28, 2006. Owen, as he liked to be addressed by colleagues and students alike, was not only an outstanding physicist and teacher but also a humanitarian who translated his deep concerns for peace and justice into action.

He was born in San Francisco on July 10, 1920, the son of W. Edward and Genevieve Lucinda Chamberlain (nee Owen). His father was a physician and radiologist at the Stanford University Hospital then located in San Francisco. Owen described his early years as typical for a child in a physician's family. Most of the family's friends were physicians, and most of his friends lived in the neighborhood.

In 1930 his father accepted a position at Temple University, the family moved to Philadelphia, and Owen's horizons widened. In the shop that Temple had built for his father, Owen gained a little hands-on technical experience, but he never considered himself a dyed-in-the-wool tinker. At first his schooling, in Philadelphia as in San Francisco, was rather routine. He did well in the subjects he liked, such as arithmetic, but only so-so in languages and English. His interest in science would come only later. His move to junior high school was more eventful. There he encountered a tough bunch of students who didn't take kindly to a kid from the West Coast. Even though the group adopted Owen as a kind of mascot after his nose was bloodied in a fight, his parents decided that he should attend a school with fewer tensions and distractions. At the end of the ninth grade they moved him to the Quaker-run Germantown Friends School. There he often labored on his history and English homework for two to three hours a day but was able to finish his physics and chemistry assignments in 10 minutes. He was well regarded by all of his teachers; even those who gave him rather poor grades in history and English wrote strong recommendations for him when he applied for admission to college.

His first real exposure to physics occurred while he was in the 11th grade in high school; he came into contact with Shirley Quimby, a professor at Columbia and the husband of one of his father's colleagues. Quimby delighted in giving young Owen stimulating physics problems and mathematical puzzles. In his oral history Owen recalled one of these problems. "Quimby had said: 'Now you know there is no such thing as a perpetual motion machine.' I said: 'Yes.' And he said: 'Then explain to me why this perpetual motion machine won't work?'" The machine consisted of an electron and a positron that gained energy as they fell down an evacuated pipe under gravity. When they got to the bottom of the pipe, they annihilated. They gave up their mass, and some additional kinetic energy that they had picked up from gravity, to make a photon that went back up the evacuated pipe, and had enough energy to make a new electron and positron pair. Owen was able to explain why this kind of machine wouldn't work: there was a gravitational red shift. This was the way he learned about the gravitational red shift.

To quote Owen:

> I think that's the first time that I was exposed to a physicist under circumstances where he could toss me problems that made me think. Some of these problems he'd give me and say: "Now, I'll be back here in a few months and you tell me the answer." I think that's what really got me into physics. I sort of got started with a fascination with some of these problems and puzzles. That's what physics is all about: problems and puzzles.

He entered Dartmouth College in 1938. He describes his education there as "all too classical," so classical in fact that it "sent me off to a rather poor start as a physicist." For example, he comments, "The impression I got from my teachers at Dartmouth was that quantum theory was very dubious in that it maybe predicted a few things correctly, but it was very unlikely to be an ultimate true theory." Most of the physics professors were of the older generation, and modern physics hadn't really started there yet. Nevertheless, he was drawn to physics because "it was always the easiest thing to do." His coursework was very tightly focused on math and physics and, in fact, one of his acquaintances quipped that Owen had gone to a liberal arts college, to avoid getting a liberal arts education.

Owen's academic record at Dartmouth was very impressive. He was awarded the Thayer Prize in Mathematics and the Dartmouth Kramer Fellowship, a fellowship that provided enough money to support him during his first year of graduate work at whatever school he chose. He chose and was accepted for graduate work at Berkeley in the fall of 1941. His adviser, Raymond T. Birge, Physics Department chair, was sufficiently impressed by Owen's academic accomplishments at Dartmouth to recommend that he take Robert Oppenheimer's graduate course in quantum mechanics even though Owen had not been exposed to the subject previously. Owen recalled that Oppenheimer's course, which attracted the best and the brightest students at Berkeley, was one of the most traumatic experiences of his life as a student. He claims he didn't understand a thing, and that he survived only because some of his more talented fellow students allowed him to copy their homework. He also took an optics course from Emilio Segrè, who was to play such an important role in his future activities as a physicist. In his autobiography *A Mind Always in Motion* Segrè recalls, "In one of my optics courses there was a student who amused himself in finding flaws in the lectures. His objections, always polite, were often well taken and showed a critical and alert mind. I appreciated the young man, who obviously was interested in the course, and used his head, and I made friends with him. He was Owen Chamberlain."

On December 7, 1941, the Japanese attacked Pearl Harbor, and Ernest Lawrence, director of the Berkeley Radiation Laboratory, started to recruit students and faculty to help with the war effort. Owen was assigned to help Emilio Segrè study spontaneous fission at the Berkeley 60-inch cyclotron. The research took up more and more of his time, and in less than a year he dropped all of his courses. He learned a great deal from fellow graduate student Clyde Wiegand, a whiz at electronics, who would go on to become one of Owen's closest scientific collaborators and friends. With barely one semester of graduate school behind him, and little technical experience, his role in the Segrè group was more that of a helper than a research scientist. For the first nine months of the project he didn't even know what the project was all about. When he finally did find out, it was from an unlikely source, Segrè's student Miss Chien-Shiung Wu, who many years later discovered parity violation in weak decays. One day in the basement of LeConte Hall Miss Wu, who as a noncitizen wasn't even a project participant, told Owen: "Well, they've got a bunch of stuff over there that they call aluminum magnesium, but they're obviously uranium isotopes." That was the first time he understood that he was working with uranium isotopes.

Owen's beginnings as experimental physicist at Berkeley were not exactly auspicious. In his autobiography Segrè recounts the following incident:

> During the summer of 1942, a theoretical group under Oppenheimer's direction met in Berkeley to try to design a nuclear bomb. Hans Bethe, Robert Serber, Edward Teller, E. J. Konopinski, and two younger physicists, Stanley Frankel and Eldred Nelson, worked on this project. As they proceeded in their calculations, they needed more and more experimental data that had not been measured, and we tried to help them out as much as possible. To proceed with a concrete plan for a bomb, it was necessary to know, among other things, the fission cross section of uranium, as well as many other cross sections, as a function of neutron energy. At the time such data were few and unreliable. It was hard to obtain monoenergetic neutrons of known energy between a fraction of an eV and a couple of MeV. Some specific energies could be reached using photoneutrons. Chamberlain, Wiegand, some other students, and I used photoneutrons generated by gamma rays of Na^{24} on beryllium and deuterium. During these experiments we had a nasty accident when Chamberlain dropped a strongly radioactive solution of radio-sodium. He was seriously irradiated and his blood showed sufficient alterations to require a vacation.

Fortunately there were no long-term consequences to his health. According to Owen about 2 curies of radio-sodium had been produced in the 60-inch cyclotron and then dissolved in heavy water that had been meticulously collected and sent to Berkeley from all parts of the United States. Almost the entire U.S. supply of heavy water was lost in this accident. As it turned out the heavy water was quickly replaced, because just at that time the Steward Oxygen Company came online with a new heavy water plant that produced in one week the amount that had been spilled on the floor.

In January 1942 Owen met his wife-to-be, Beatrice Babette Cooper, who was an undergraduate student at Berkeley at the time. They were married in the spring of 1943. That summer

Segrè moved his group, including Owen and his new bride, to Los Alamos. The group was assigned the task of determining the spontaneous fission probabilities of uranium and plutonium isotopes. They found that the high spontaneous fission rates in plutonium would interfere seriously with the proposed method of bomb assembly. Segrè says, "…our results brought the Los Alamos lab to a real crisis. …Spontaneous fission in plutonium was so frequent that the plutonium alternative for making a bomb was excluded unless one could invent and develop a totally different assembly method."

Owen found the intellectual atmosphere at Los Alamos both stimulating and stressful: stimulating because of his interactions with some of the world's greatest scientists—such as Oppenheimer, Bethe, Fermi, Teller, Weisskopf, and Feynman—and stressful because of the constant pressure to solve problems and produce a workable nuclear weapon. On July 15, 1945, he was in the trenches at Alamogordo, New Mexico, when the first atomic bomb was detonated.

In light of Owen's later strong commitment to nuclear disarmament it is interesting to note that when he first went to Los Alamos, he supported the atomic bomb program wholeheartedly. He believed that if we didn't make a nuclear weapon someone else would. He was surprised that the use of the bomb ended the war so quickly and effectively. He soon became concerned that people who read the newspapers and realized that the atomic bomb had been instrumental in stopping the war would not comprehend what a revolution had occurred in the military. Within a few months he was taking an active part in the newly established organization called the Federation of Atomic Scientists, whose main goal was to prevent future nuclear wars.

After the end of World War II, in August 1945, many of the scientists left Los Alamos to resume the careers they had interrupted. Owen, too, wanted to get back to graduate school to finish his Ph.D., but the military draft was still in effect, so he stayed at Los Alamos until he was sure that he would not be drafted. Segrè encouraged him to study with Fermi at the University of Chicago, and in March 1946 he went there with his wife and small daughter to join one of the most impressive groups of graduate students ever assembled in one place at one time. During his time at Chicago his fellow students included C. N. Yang, T. D. Lee, Jack Steinberger, Geoff Chew, Marvin Goldberger, Lincoln Wolfenstein, Albert Wattenberg, Joan Hinton, and Leona Marshall.

Owen ranks Fermi and Segrè as his most influential teachers, but with very different styles. Fermi knew everything and, according to Owen, he had a knack for explaining almost all problems in terms of seven or so basic concepts. Fermi would manage to get around difficulties by making it appear that they weren't there; it was only when you tried to reproduce his steps that you fell into the traps that he knew how to dodge. Segrè was rather the opposite. He always fell into the trap himself; watching him work his way out was extremely instructive. Owen says, "I learned more physics that way—my kind of physics—than I could have if he'd had a smooth sailing presentation."

Owen initially intended to study theoretical physics but soon switched to experiment under Fermi's direction. His thesis experiment was the diffraction of neutrons by liquids. Raymond T. Birge, the longtime chair of the Berkeley Physics Department, recalled that during a visit to Berkeley in the summer of 1947, Fermi recommended Owen as his best student. By the summer of 1948 Owen had pretty much completed the required experimental measurements. But still he had not finished writing his dissertation. Nevertheless, he received offers for junior faculty positions from Harvard and Berkeley, and was being actively recruited by Isador Rabi at Columbia. On Fermi's advice he chose to accept an instructorship at Berkeley, and that fall he started what was to become a 41-year academic career there.

According to Owen the atmosphere he found at Berkeley was completely dominated by Segrè. He and his group had done quite a bit of work on neutron-proton scattering, and here was an opportunity to follow this with proton-proton scattering. Owen recalls, "It was sort of lying there in front of me. Clyde Wiegand was about to start on it, and Segrè thought it would be a good thing for me to do. It was sort of left in my lap. Fortunately, I think we started a good collaboration right from the start." During Owen's first year, his research progress was constrained by his need to finish his thesis and to teach two physics courses (a course for premed students and the upper-division physics laboratory). With respect to teaching Owen says, "I had a particular attitude which I doubt is shared by very many of my colleagues currently. My attitude was that I earned my living by teaching, and one of the ways that I was paid for my teaching was to be allowed to spend time in the laboratory. This led me to take my teaching career seriously."

A year after his arrival in Berkeley the loyalty oath controversy arose. In his history of the Berkeley Physics Department Raymond Birge wrote,

> [I]t is worthy of note that Dr. Chamberlain did <u>not</u> sign the special oath prescribed by the Regents. Instead he wrote a letter, dated November 21, 1949, to President Sproul, with the enclosed signed and notarized oath: "I do solemnly swear (or affirm) that I will support the Constitution of the United States and the Constitution of the State of California, and that I will faithfully discharge the duties of my office according to the best of my ability." This is just the standard oath required of all State employees, as quoted earlier in this chapter. In his accompanying <u>letter</u> he wrote: "I am not a member of the Communist Party, or under any oath, or party to any agreement, or under any commitment that is in conflict with my obligations to the University, the State of California, or to the United States." Thus Dr.

Chamberlain included all the contents of the special oath, but he did not swear to its objectional [sic] features.

In 1950 he was advanced to assistant professor. By this time he had attracted a number of graduate students and joined forces with Segrè to co-lead the Segrè-Chamberlain group at the Radiation Laboratory. My own interaction with Owen started in the summer of 1953, when I decided to do my Ph.D. thesis under the direction of Emilio Segrè. I well remember my first day at the Rad Lab. Segrè wasn't in, so I knocked on Owen's door, and started to introduce myself. I had barely said, "Professor Chamberlain . . ." when he stopped me to say, "I'm Owen." Owen's easy informality came as a major cultural shock to a student like me, who had been brought up in the Germanic tradition of thinking of professors as rather remote omnipotent deities. There were typically six or so students in the group at any one time, and we all worked together. We were all rather intimidated by Segrè, who didn't suffer fools lightly, but who also taught us a lot from his extensive experience, and his ability to focus on the essence of a problem. Owen was the person we would go to when we didn't understand something or when we wanted to learn more. The other senior member of the group was Clyde Wiegand, the superb experimental physicist who turned our naïve ideas into reality and taught us by example how physics should be done. Clyde was also the one to whom we would go to ask the questions we were too embarrassed to ask even Owen. One student, in particular, deserves special mention. He was Tom Ypsilantis, who was always full of ideas, and he played an increasingly important role in the activities of the group as time went on.

In 1953-1954 the main thrust of the physics program of the Segrè-Chamberlain group was the study of the nucleon-nucleon interaction, and a number of scattering experiments were underway at the 184-inch synchrocyclotron. The most innovative part of this program was launched by Ypsilantis, who produced beams of highly polarized protons by scattering them from an internal target in the cyclotron. Over the course of the next few years the Segrè-Chamberlain group undertook a comprehensive series of double and triple p-p scattering experiments, that led to the first detailed phase shift analysis of the p-p system in the energy region under study. It may have been this initial contact with polarization phenomena that stimulated Owen's subsequent research interests with polarized targets.

In 1948 in his continuing quest to build ever more energetic particle accelerators, Ernest Lawrence launched the construction of the Bevatron at the Rad Lab. Its energy of 6.3 GeV was chosen to make it kinematically possible to produce antiprotons. As the Bevatron neared completion in 1954 it was clear, even to lowly graduate students like me, that the search for antiprotons would become a high-priority physics objective for physicists both inside and outside the Rad Lab. The genesis of the effort in our group probably dates back to the fall of 1953 when Owen returned to Berkeley after spending the summer at Brookhaven. In his oral history Owen recounts conversations with colleagues at Brookhaven that set him to thinking about how to do this experiment. He firmly believed that antiprotons would be found, and he was therefore particularly stimulated when he heard of a bet between two of his distinguished colleagues Hartland Snyder and Maurice Goldhaber. In his oral history Owen recalls,

> I heard that there had been a bet between Hartland Snyder and Maurice Goldhaber, with Maurice Goldhaber betting some large sum—it could have been $500; it seemed like a huge sum at the time. Maurice had bet that the antiproton didn't exist and Hartland Snyder had said it did. Well, I have great respect for Maurice Goldhaber as a physicist, and I suspect he made the bet when he was a little drunk, but even when drunk, Maurice Gold-

haber is a good physicist. So if someone of the stature of Maurice thought maybe antiprotons didn't exist, then this was a real spur to showing that they did. And I think it was at that moment that I decided "by Jove, this is what I want to do."

A little later Owen goes on to say: "Now in the antiproton business, I think I took an idea which was laying around for everybody to fuss with. I ran with it, of course with Clyde Wiegand, because the two of us worked very intently on it."

During much of 1954 the experiment rapidly took shape. Clyde and Owen spearheaded the effort in our group, with Emilio Segrè and Tom Ypsilantis in close support. It was interesting for me, as a graduate student, to watch the process unfold. Much of the work was done with an uncharacteristic degree of secrecy, because other talented physicists were well aware that the antiproton fruit was ripe for picking. Owen was the intellectual leader of the enterprise, Clyde the technical guru, Segrè with his penetrating insight and experience kept the experiment on track, and Tom Ypsilantis's unbridled imagination and enthusiasm was the dynamo that kept the wheels spinning. It was a truly potent mix of talents where the combined effort far surpassed the sum of its parts. The elegance of the experiment was its simplicity. The method of choice was to determine the mass and charge of the antiprotons by measuring their momentum and velocity. The proton beam in the Bevatron was used to produce secondary particles in a copper target. A series of magnets then transported a negatively charged beam of known momentum to the velocity-defining detectors. Two scintillation counters, separated by 13 meters, were used to measure the 13 nanosecond time difference between the rare antiprotons and the much more copiously produced pions in the beam. A narrow-band velocity-selecting Cherenkov counter, conceived by Owen and familiarly called the "Pickle Barrel" responded only to the antiprotons. It, together with a threshold Cherenkov detector, efficiently rejected any pions that might have fooled the time-of-flight system. The experiment itself got underway in August 1955, and by October the group had obtained clear evidence for the existence of antiprotons. Emilio Segrè and Owen Chamberlain were awarded a Nobel Prize in 1959 for this accomplishment. He was elected to the National Academy of Sciences in 1960.

Once the initial excitement about the antiproton had subsided, Owen began thinking seriously about what to do next. He had a strong urge to strike out on his own, rather than work only in collaboration with Segrè and Wiegand. On the family front he was by now the father of three children—Karen, Darol, and Lynne; his last daughter, Pia, was born while he was spending a sabbatical year in Rome in 1957-1958. During a semester at Harvard in 1959 as the Loeb lecturer, his Harvard colleague Richard Wilson reignited Owen's interests in polarization phenomena when he mentioned that he had heard rumors that it might be possible to build polarized proton targets to study spin-dependent phenomena in high-energy experiments. The rumors turned out to be based on the method of dynamic nuclear polarization that had been independently developed by Anatole Abragam at Saclay and by Carson Jeffries at Berkeley.

Upon his return to Berkeley in 1960, Owen followed up on Wilson's suggestion and embarked on a new project that was to occupy him for the next 25 years. In short order he and his coworkers, including Jeffries, successfully developed polarized targets for use in high-energy physics experiments. In typical Chamberlain style he set out to learn and master completely new experimental techniques that are not the bread and butter of particle physicists. It was a contagious endeavor, because following Owen's pioneering efforts it was not long before polarized targets sprung up like weeds in most of the particle physics laboratories in the world. The early targets were typically a few centimeters in size, contained only about 3 percent free protons, and had polarizations of 20 percent, but over the years meter-long targets, having 20 percent to 30 percent free protons with polarizations of over 90 percent have come onto the scene. The targets were mainly used to study basic scattering processes and fundamental symmetries. Owen and his group used polarized targets to study the resonant structure of the pion-nucleon interaction, to measure the spin-parity of hyperons, to look for time-reversal symmetry violation in electron-proton scattering, and to do the first polarized target experiments with high-energy proton and pion beams at Fermi National Accelerator Laboratory. Owen was always leading the charge in this experimental effort.

In the 1970s Owen found a new outlet for his versatility and skills as a physicist. In close collaboration with a group of young Japanese physicists he and his group initiated a program of muon-spin-rotation experiments at the 184-inch synchrocyclotron. A little later some of them joined his group to study the interactions of energetic light nuclei with nuclear targets at Lawrence Berkeley National Laboratory's Bevalac facility. In the 1980s he accepted another new challenge when he assumed responsibility for designing and overseeing the construction of the high-voltage system for the newly developed time projection chamber.

In the 1990s his health began to fail, but he maintained his interest and involvement in the group's research activities. By that time we had joined forces with colleagues at the SLAC National Accelerator Laboratory to test the Standard Model of electroweak interactions with unprecedented precision. Again, polarization played a crucial role. A longitudinally polarized beam of electrons collided with unpolarized positrons in the Stanford Linear Collider to produce Z bosons.

The test consisted of measuring the asymmetry in the number of Zs produced when the electron polarization was reversed. The Berkeley group designed and built a novel polarimeter that was a key ingredient in this work, and participated fully in all aspects of this experiment. Owen, as usual, injected ideas and critical analysis.

Life in Owen's group was never dull. He enjoyed interacting with students and postdocs, and they in turn were attracted to him because of his openness and his impressive knowledge of physics. Owen loved to be confronted with new physics challenges, both conceptual and technical, and invariably he solved them in his own unique style. Owen had learned the core concepts of physics so well that he could adapt them with skill and imagination to almost any problem that faced him. He argued conceptually, rather than mathematically, and set a standard of correctness and rigor that was an inspiration, if also a hard act to follow. He was the one that colleagues and students would go to whenever they couldn't easily find the answer elsewhere, and seldom, if ever, did he fail to come up with an explanation that was not only to the point but often also unconventional. Similarly, when confronted with technical problems, he usually devised clever solutions that tended to be simple and elegant.

Anyone who has shared the adventure of visiting Owen in his office, whether on campus or at Berkeley, must have been awed, if not frightened into abject silence by the towering piles of binders, papers, journals, books, and other junk that occupied just about every square centimeter of table, desk, floor, and cabinet space, and which threatened to inundate the unwary visitor at the first flap of a butterfly's wings. As one measure of the filling factor in Owen's campus office, space was so tight that the blackboard he used to explain concepts to students during office hours had to be moved to the hallway. Despite the chaotic nature of his way of storing documents, he invariably was able retrieve the information he wanted by just reaching into one of the piles and pulling out what he was looking for.

He was a popular teacher. While his lecturing style might not have been as charismatic as that of some of his more flamboyant colleagues, he was always well prepared, and above all his commitment to helping students and making himself accessible were deeply appreciated by them. He taught just about every course in the Berkeley Physics Department's repertoire, and even some outside the department. He was a very effective supervisor of graduate students, and many of them went on to distinguished careers of their own.

While physics research and teaching occupied a large fraction of his time, over the years he became ever more involved in pursuing causes with broader political and social consequences. He was a passionate and tireless advocate for nuclear disarmament and world peace. He was one of the founders and leaders of the Ploughshares Fund to abolish nuclear weapons. He was a very vocal and active protester of the Vietnam War. Human rights were issues that he strongly supported and promoted, and ranged from those of dissident Soviet scientists Sakharov, Orlov, and Sharanski, to student rights during the Free Speech Movement of the late 1960s, to farm-worker aspirations for better working conditions. Together with mathematician Leon Henkin he launched and then for many years codirected a Special Opportunities Scholarship program that made it possible for talented disadvantaged high school students to attend the University of California. For him these were not just intellectual commitments but causes that he embraced with great zeal and dedication.

In 1978 his marriage ended in divorce. By then his four children were grown and scattered around the country. In 1980 he married the artist June Steingart, who died of cancer in 1991. He married his third wife, Senta Pugh, in 1998. She was not only a stimulating companion but also a dedicated caregiver as Owen's health declined. Despite Owen's deteriorating physical condition, thanks to Senta's considerable efforts he continued to attend the weekly Physics Department colloquia until just a few weeks before his death.

In 1995 on the occasion of Owen's 75th birthday I was asked to speak about "The Legacy of Owen Chamberlain." It was a rather pretentious title for a very unpretentious man. I focused primarily on physics-related experiences but noted that his involvement in social causes—as an advocate for peace and for providing opportunities for the professional development of young people—are surely a crucial part of his legacy. I consider the antiproton and the polarized target work as the most important components of what one might call his physics legacy. But when all is said and done the greatest legacy of all was his influence on the lives of the people with whom he came into contact—his students, postdocs, colleagues, and friends—all of whom are perpetuating this legacy in their own way. Owen was a very special person who has left an indelible impression on those of us who have had the privilege of knowing him and working with him.

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