Title

A Biographical Memoir for Publication by the National Academy of Sciences

Permalink

Author

Alvarez, Luis W

Publication Date

1958-08-01

UCRL-17359

University of California

Ernest O. Lawrence Radiation Laboratory

ERNEST ORLANDO LAWRENCE

August 1, 1901 - August 27, 1958

A Biographical Memoir for Publication by the National Academy of Sciences

Berkeley, California

DISCLAIMER

To be submitted to the
National Academy of Sciences

UCRL-17359
Preprint

UNIVERSITY OF CALIFORNIA

Lawrence Radiation Laboratory
Berkeley, California

AEC Contract No. W-7405-eng-48

ERNEST ORLANDO LAWRENCE

August 1, 1901 - August 27, 1958

A Biographical Memoir for Publication
by the National Academy of Sciences

Luis W. Alvarez

ERNEST ORLANDO LAWRENCE

August 1, 1901 - August 27, 1958

A Biographical Memoir for Publication
by the National Academy of Sciences

Luis W. Alvarez

In his relatively short life of fifty-seven years, Ernest Orlando Lawrence accomplished more than one might believe possible in a life twice as long. The important ingredients of his success were native ingenuity and basic good judgment in science, great stamina, an enthusiastic and outgoing personality, and a sense of integrity that was overwhelming.

Many articles on the life and accomplishments of Ernest Lawrence have been published, and George Herbert Childs has written a book-length biography. This biographical memoir, however, has not made use of any sources other than the author's memory of Ernest Lawrence and of things learned from him. A more balanced picture will emerge when Herbert Childs' biography is published; this sketch simply shows how Ernest Lawrence looked to one of his many friends.

Lawrence was born in Canton, South Dakota, where his father was superintendent of schools. As a boy, Lawrence's constant companion was Merle Tuve, who went on to establish a reputation for scientific ingenuity and daring, much like that of his boyhood chum. Together, Lawrence and Tuve built and flew gliders, and they collaborated in the construction of a very early short-wave radio transmitting station. This experience can be seen reflected in their later work--Lawrence was the first man to accelerate particles to high energy by the application of short-wave radio techniques, and Tuve, with Breit, was the first to reflect short-wave radio pulses from the ionosphere, a technique that led directly to the development of radar. In the early thirties, Lawrence and Tuve were leaders of two energetic teams of nuclear physicists. Lawrence with his cyclotron, and Tuve with his electrostatic accelerator, carried the friendly rivalry of their boyhood days into the formative stages of American nuclear physics, and all nuclear physicists have benefited greatly from it.

Lawrence began his college work at St. Olaf's in Northfield, Minnesota, and then went back to the University of South Dakota for his B. S. degree. He worked his way through college by selling kitchenware to farmers' wives in the surrounding counties. Very few of the scientific colleagues who admired his effectiveness in selling new scientific projects to foundation presidents and government agencies knew that he had served an apprenticeship in practical salesmanship, many years before. And indeed, it would be quite wrong to attribute his later successes in this field to any early training -- it was always obvious that he convinced his listeners by an infectious enthusiasm, born of a sincere belief that his ideas were sound and should be supported in the best interests of science and of the country.

Although Lawrence started his college career as a premedical student, he switched to physics under the guidance of the talented teacher one so often finds in the background of a famous scientist's career. In Lawrence's case, this role was played by Dean Lewis E.

Akeley, who tutored him privately and sent him on to the University of Minnesota as a graduate student. On the wall of Lawrence's office, Dean Akeley's picture always had the place of honor in a gallery that included photographs of Lawrence's scientific heroes: Arthur Compton, Niels Bohr, and Ernest Rutherford.

At Minnesota, Lawrence came under the influence of Professor W. F. G. Swann, and when Swann moved to Chicago and then went on to Yale, Lawrence moved both times with him. Lawrence received his Ph. D. degree at Yale, in 1925, and remained there three years more, first as a National Research Fellow, and finally as an assistant professor. From this period of his formal training in physics, very little remained on the surface in his later years. To most of his colleagues, Lawrence appeared to have almost an aversion to mathematical thought. He had a most unusual intuitive approach to involved physical problems, and when explaining new ideas to him, one quickly learned not to befog the issue by writing down the differential equation that might appear to clarify the situation. Lawrence would say something to the effect that he didn't want to be bothered by the mathematical details, but "explain the <u>physics</u> of the problem to me." One could live close to him for years, and think of him as being almost mathematically illiterate, but then be brought up sharply to see how completely he retained his skill in the mathematics of classical electricity and magnetism. This was one of the few heritages he brought from his apprenticeship with W. F. G. Swann and the physics departments of the 1920's. Almost everything that Lawrence did -- and more particularly, the way he did it -- came from himself, not from his teachers.

In 1928 Lawrence left Yale for Berkeley, where, two years later at the age of 29, he became the youngest full professor on the Berkeley faculty. It is difficult for one starting on a scientific career today to appreciate the courage it took for him to leave the security of a rich and distinguished university and move into what was, but contrast, a small and only recently awakened physics department. In later life, when he needed to reassure himself that his judgment was good even though it disagreed with the opinions of most of his friends, he would recall the universally dire predictions of his Eastern friends; they agreed that his future was bright if he stayed at Yale, but that he would quickly go to seed in the "unscientific climate of the West."

The predictions of a bright future for Ernest Lawrence were solidly based. His doctor's thesis was in photoelectricity. Later, he made the most precise determination, to that time, of the ionization potential of the mercury atom. In his characteristically candid manner, he often depreciated this highly regarded measurement. He had a preconceived "correct value" of the ionization potential in mind, and he would say in a contrite manner that he looked for possible errors of the correct sign and magnitude to make his preconception come true. Every scientist has fought this battle with himself, but few have used themselves as an example to impress their students with the necessity of absolute honesty in scientific inquiry. Lawrence's value for the ionization potential has stood the test of time, but he always shrugged it off by saying he was lucky; if he had looked hard for errors of the opposite sign, he would have found them, too. After this early experience with looseness in science, Lawrence formed the habit of critically examining any scientific result, regardless of its origin. He applied the same rigid standards of criticism to his own work, to that of his associates, and to the reports from other laboratories. Visitors sometimes formed an early impression that Lawrence was overly critical of the experimental results of others, but they soon found he encouraged his juniors to criticize his own work with equal vigor. He believed that a scientific community that did not encourage its members to criticize each other's findings in an open manner would quickly degenerate into an association of dilettantes. Scientific criticism was with him an impersonal reaction; he gave it or received it without any feelings of hostility. He did, however, reserve a bit of scorn for some members of the profession who, in his opinion, drew unwarranted conclusions from each "bump and wiggle" of a curve obtained with poor counting statistics.

Lawrence's name is so closely associated with the field of nuclear physics in the minds of most physicists today that it often comes as a surprise to them to find that he had a distinguished career in other branches of physics before he invented the cyclotron. After he moved to California, he continued his work in photoelectricity, and together with his students published a number of papers in this field. It is difficult for one not intimately familiar with a particular area of physics to appraise the value of another's work in that area. But one can gain some idea of the esteem in which the work was held by examining the literature of the period, and seeing how often the work was referred to by the experimenter's peers. Fortunately for this purpose, an authoritative treatise on photoelectricity was published shortly after Lawrence left the field to concentrate his efforts on the cyclotron. Hughes and DuBridge's Photoelectric Phenomena appeared in 1932, and it contains a biographical index of about 700 names. A quick examination of the "multi-lined entries" shows that only twelve of the 700 experimenters were referred to more often than Ernest Lawrence. His contributions were referred to in all eight of the chapters that dealt with non-solid-state photoelectricity, which is a measure of Lawrence's breadth of coverage of the field in the few years he devoted to it.

One of the references in Hughes and DuBridge's book is to Lawrence's investigation of possible time lags in the photoelectric effect. He published several papers in the field of ultrashort-time-interval measurement while he was at Yale, and in this work he was closely associated with Jesse Beams, who is now Professor of Physics at the University of Virginia. Beams was a pioneer in the use of the Kerr electrooptical effect as a light shutter capable of opening in times of the order of 10^{-9} second. He and his students investigated time lags in the Faraday (magnetic rotation) effect, and he has devoted a major portion of his distinguished scientific career to the study of short times, high accelerations, and other related phenomena. Lawrence and Beams showed that photoelectrons appeared within 2×10^{-9} second after light hit the photoelectric surface. Although these measurements were made more than thirty-five years ago, they are modern in every other sense of the word. Only in the last few years has the measurement of time intervals of 10^{-9} second come into routine use in the laboratory. This renaissance is a direct result of the expenditure of vast sums of money on the development of photomultiplier tubes, wideband amplifiers, high-speed oscilloscopes, and a host of auxiliary equipment, such as coaxial cables and shielded connectors--none of which was available to Beams and Lawrence.

In this period, Lawrence and Beams performed their well-known experiment of "chopping up a photon." The uncertainty principle states that the energy of a system cannot be determined more accurately than about $h/\Delta t$, where h is Planck's constant and Δt is the time available for the measurement. A narrow line in the optical spectrum is a system with a very small energy spread; each light quantum, or photon, has the same energy to within an uncertainty ΔE. Lawrence and Beams showed that if they decreased the time available for the energy measurement (by turning the light on and off again in a small time Δt), the width of the spectral line increased as predicted by the uncertainty principle. It is not generally known that Lawrence played an important part in the evolution of the high-speed rotating top which Beams later developed so beautifully. But the bibliographies of Beams and Lawrence show that the first reference to the high-speed rotor is in an abstract by Lawrence, Beams, and Garman, dated 1928.

As a result of their scientific collaboration, Lawrence and Beams became very close personal friends. They took one summer away from their work, and toured Europe together. Lawrence often referred nostalgically to that period in his life, when he could travel and see the sights without the responsibilities of the speeches and receptions which marked his later tours in foreign lands.

Shortly before Lawrence left Yale, he had an experience that is known to only a few of his close associates, but which was most important in his development as a scientist. At that time, television was considered to be a rather impractical dream, because the basic element was the rotating scanning wheel. It was obvious to everyone that this mechanical device would limit the development of picture quality by restricting the number of "picture lines" to less than 100. Lawrence's experience with photoelectricity and the newly developing cathode-ray tube led him to believe that he could make an all-electronic television system without rotating wheels. He quickly put together a rudimentary electronic television system, and being quite sure that he was not only the first to have the idea, but also the first to "reduce it to practice," he contacted a friend at the Bell Telephone Laboratories. After hearing Lawrence say that he had an important new idea in the field of television, his friend invited him down to the Bell Laboratories to talk about it. The friend took him through what Lawrence later described as a "whole floor" full of electronic television apparatus, with excellent pictures on cathode-ray tubes that were beyond anything he had imagined might exist. After dreams of the financial reward his invention would bring him, it was a real shock for him to see how far ahead a good industrial laboratory could be in a field that was important to it. He resolved then and there to concentrate on the things that he knew most about, and not to dilute his effort by competing in the commercial area. He kept firmly to this resolve until the last decade of his life, when he had received all the honors that were available in the scientific world. He then turned some of his creative ability to the problem of color television, a field in which he contributed many new ideas. Paramount Pictures supported an extensive development of the "Lawrence tube," or "Chromatron." In the last few years of his life, Lawrence was issued dozens of patents on his inventions in the field of color television.

Since it has only recently been considered "respectable" for a scientist to hold patents, it is worth reviewing Ernest Lawrence's attitude toward patents, and the financial rewards from inventions. The cyclotron and the other Lawrence inventions of the prewar era were patented in Lawrence's name, and assigned by him to the Research Corporation. No royalties were ever asked by the Research Corporation, and Lawrence encouraged and helped scientists throughout the world to build cyclotrons. Lawrence was legally the inventor of the Calutron isotope separator, but he assigned the patent to the government for the nominal one dollar. Some of his colleagues in the atomic bomb project were awarded large sums of money by the government for the infringement of their patents, but Lawrence never allowed his name to be used in any litigation, and therefore received no compensation for his wartime inventive efforts beyond his normal salary.. Although he greatly enjoyed the luxuries that came with wealth, and encouraged others to follow his example of inventing for profit in peripheral areas, he felt that it was unwise to foster the patenting of scientific discoveries or developments for personal profit. One of his greatest accomplishments was the encouragement of scientific colleagues to work closely together in an atmosphere of complete freedom for exchanging ideas. (As an extreme example of the pre-Lawrence method, one can recall that Roentgen spent several weeks in a detailed study of the properties of x rays before he told the men in the adjacent research rooms of his discovery.) Lawrence was acutely aware of the change he had wrought in the methods of doing physics, and was worried that patent consciousness might turn back the pages of progress. As he expressed it, a man would be very careful how he talked over his new ideas if the person to whom he was talking might enlarge on them and subsequently make a fortune from a patent.

While at New Haven, Lawrence was a frequent visitor in the home of Dr. George Blumer, Dean of the Yale Medical School. It was soon obvious that he particularly enjoyed the company of the eldest of the four Blumer girls, sixteen-year-old Mary, or Molly as she is known to all her friends. They were engaged in 1931; he returned to New Haven the next year and brought her as his bride to live in Berkeley. Two years later Eric, the first of the six Lawrence children, was born; he was followed by Margaret, Mary, Robert, Barbara, and Susan. With Ernest and the children, Molly Lawrence created a home that was famous throughout the world of physics for its warmth and hospitality. In it, they entertained the steady parade of visitors to the Radiation Laboratory. In 1941, Molly's sister Elsie became the wife of Edwin McMillan, the present director of the Laboratory. Completing the family group in Berkeley were Ernest's parents, who settled there when the elder Dr. Lawrence retired from his distinguished career in education, and Ernest's brother, John, whose pioneering role in the medical aspects of radiation is mentioned later in this memoir. Surely one of Ernest Lawrence's greatest satisfactions must have come from the knowledge that his mother's life had been saved by radiation therapy, using the one-million-volt "Sloan-Lawrence" x-ray machine at the University of California Medical School. After Mrs. Lawrence had been told by many distinguished specialists that she had an inoperable tumor, she was treated, more or less in desperation, with the novel resonance transformer device which Lawrence and his co-workers, in the incredibly busy days of the early 1930's, had installed in the San Francisco Hospital. At the time of her son's death, she was still living in Berkeley, twenty-one years after Dr. Robert Stone treated her with the only million-volt x rays then available in the world.

In the period when Ernest Lawrence was moving from New Haven to Berkeley, physicists were excited by the news of the nuclear transformations being achieved in Lord Rutherford's Cavendish Laboratory, at Cambridge, England. It was generally recognized that an important segment of the future of physics lay in the study of nuclear reactions, but the tedious nature of Rutherford's technique (using the alpha particles from radium) repelled most prospective nuclear physicists. Simple calculations showed that one microampere of electrically accelerated light nuclei would be more valuable than the world's total supply of radium--if the nuclear particles had energies in the neighborhood of a million electron volts. As a result of such calculations, several teams of physicists set about to produce beams of "million-volt particles." Cockcroft and Walton at the Cavendish Laboratory used a cascade rectifier plus a simple acceleration tube, and although they never reached their initial goal of a million volts, they found that nuclear reactions took place copiously at a few hundred kilo electron volts.

Lawrence had spent enough time in the study of spark discharges with the Kerr electrooptical shutter to develop a very healthy respect for the spark-breakdown mechanism as a voltage limiter. He followed the early work of Van de Graaff, whose electrostatic generator made spectacular high-voltage sparks, and the work of Brasch and Lange, who attempted to harness lightning discharges to the acceleration of charged particles. Although he wanted to "get into the nuclear business," the avenues then available didn't appeal to him, because they all involved high voltages and spark breakdown.

In his early bachelor days at Berkeley, Lawrence spent many of his evenings in the library, reading widely, both professionally and for recreation. Although he had passed his French and German requirements for the doctor's degree by the slimmest of margins, and consequently had almost no facility with either language, he faithfully leafed through the back issues of the foreign periodicals, night after night. On one memorable occasion, while browsing through a journal seldom consulted by physicists, "Arkiv für Electroteknik," he came across an article by R. Wideröe entitled, "Über ein neues Prinzip zur Herstellung hoher Spannungen." Lawrence was excited by the easily understood title, and immediately looked at the illustrations. One showed the arrangement Wideröe had employed to accelerate potassium ions to 50,000 electron volts, using a double acceleration from ground to ground, through a "drift tube" attached to a radio-frequency source of 25,000 volts. Lawrence immediately sensed the importance of the idea, and decided to try the obvious extension of the idea to many accelerations through drift tubes attached alternately to two radio-frequency "bus bars." Since he could do his own thinking faster than he could translate Wideröe's classic paper, Lawrence had the pleasure of independently arriving at many of Wideröe's conclusions. It struck him almost immediately that one might "wind up" a linear accelerator into a spiral accelerator by putting it in a magnetic field. He was prepared to arrange the magnetic field to vary in some manner with the radius, in order that the time of revolution of an ion would remain constant as its orbit increased in radius. A simple calculation, on the spot, showed that no radial variation of the magnetic field was needed--ions in a constant magnetic field circulate with constant frequency, regardless of their energy.

One of my most cherished memories is of a Sunday afternoon in the Lawrence living room, about fifteen years ago. Young Eric came in to tell his father that his high school physics teacher had assigned him the responsibility of explaining the cyclotron to his class. His father produced a pad of paper and a pencil, and while I pretended to read a magazine, but listened with one ear, he explained the cyclotron to his eldest son. He told how when the particles were going slowly, they went around in little circles, and when they were going faster, the magnet couldn't bend them so easily, so they went in bigger circles, and had farther to go. The interesting thing was that the slow ions in the little circles took the same time to go around as the fast ions in the big circles, so one could push and pull on all of them at the same rate, and speed them all up. Eric thought about this for a short while, looked at his father with admiration, and said, "Gee, Daddy, that's neat!" I've always thought that the Nobel Committee must have had something of that feeling when they voted the prize to Ernest Lawrence, in 1939.

According to Lawrence, an ion with a charge-to-mass ratio of e/m will circulate in a magnetic field H, at an angular velocity ω, given by

$$\omega = 2\pi f = (H/c)\ (e/m)\ ;$$

here f is the rotational frequency of the ion, in cycles per second, and c is the velocity of light. If an ion is to be accelerated as it circulates in a magnetic field, one must impose on it an alternating electric field of the same frequency. If there was any element of "luck" in Lawrence's career, it was the ready availability in the 1930's of electronic components appropriate to the frequency range of about 10 megacycles. This is the frequency one obtains by substituting into the cyclotron equation the e/m value for the hydrogen molecular ion (or the soon-to-be-discovered deuteron) together with the magnetic field strength that is most easily obtained with an iron-cored electromagnet. Had the calculated frequency turned out to be 4000 times as great (as it is for the electron), cyclotrons would probably not have appeared on the scene until World War II had fathered the necessary microwave oscillators. I originally wrote the last sentence without the qualifying word "probably," but inserted it after recalling the many other technical innovations created by Lawrence in his drive to make the cyclotron a reality. He and his co-workers, M. Stanley Livingston and David Sloan, found it necessary to develop and build their own vacuum pumps and high-power oscillator tubes, because none with the required capacity was commercially available at a price they could pay. They were soon using the largest high-vacuum pumps in the world, the highest-power radio oscillators ever seen, and the largest magnet then in operation. Had they needed high-power microwave oscillators, they would probably have invented and built them, just as Hansen and his co-workers did a decade later at Stanford.

Lawrence is best known for his application of the cyclotron equation to nuclear physics, but he also used the equation to help devise the most accurate method of measuring the specific charge, e/m, of the electron. The method was employed by Frank Dunnington, one of Lawrence's students, in what remained for many years the most precise measurement of this important fundamental constant.

The first demonstration of the cyclotron resonance principle was reported at the Berkeley meeting of the National Academy of Sciences, in the Fall of 1930, by E. O. Lawrence and N. E. Edlefson. Their original apparatus is on permanent exhibit at the Lawrence Radiation Laboratory, together with the brass vacuum chamber of the first 4-inch-diameter cyclotron of Lawrence and Livingston, which accelerated hydrogen molecular ions to an energy of 80,000 electron volts. Lawrence and Livingston went on at once to build an 11-inch cyclotron, which they hoped would be the first accelerator to yield "artificial disintegration" of light nuclei. The device was giving protons with an energy of several hundred keV (which we now know would have been quite adequate for the job) in the spring of 1932, but Lawrence and Livingston pressed on to their goal of 1 million electron volts, which appeared to be well within reach. They had no counting equipment in their laboratory, but two friends from Yale, Donald Cooksey and Franz Kurie, were to bring counters to Berkeley in the summer of 1932, to help with the observations. When the visitors arrived, they made it possible for the Berkeley team to repeat the now famous work of Cockcroft and Walton, who had announced their discovery of the disintegration of lithium by protons in early 1932. This was the first of several important discoveries in nuclear physics that could almost as well have been made in Lawrence's laboratory. But, of course, many laboratories in the world, including all the accelerator laboratories, missed these same later discoveries. The first "miss" at Berkeley--the disintegration of lithium--involved the same mistake Cockcroft and Walton had also made earlier; neither group had looked at the lower energies we now know were sufficient to have done the job. The successful Berkeley experiment was planned for the summer of 1932 when counting equipment would be available, and it was carried off on schedule. Cockcroft and Walton simply got their experiment done first.

But for those who became physicists after World War II and who may be unacquainted with the primitive world of the early accelerator laboratory, a few words will provide an understanding of how Lawrence and his co-workers missed artificial radioactivity, and after that, the discovery that neutrons can produce artificial radioactivity. We should keep in mind that the development of the cyclotron, which actually had been ridiculed by some physicists as impractical, was an extremely difficult technological task that only a man of Lawrence's daring would have undertaken. To make it work required the development of technologies and arts that were not then known. What seems so easy today was won only with sweat and long hours by Lawrence and his associates in the early 30's. In the early years most of the time of the Berkeley group was concentrated on developing the cyclotron into the efficient tool that was subsequently used with such proficiency in many research areas.

The 27-inch cyclotron was built with incredible speed in an old wooden building near Le Conte Hall, the home of the Physics Department, and the birthplace of the smaller cyclotrons. The old wooden Radiation Laboratory, which was finally torn down in 1959, was the first of the modern nuclear physics laboratories--institutes in which experimentalists collaborated on joint projects, or worked on their own research projects, as they saw fit. The great enthusiasm for physics with which Ernest Lawrence charged the atmosphere of the Laboratory will always live in the memory of those who experienced it. The Laboratory operated around the clock, seven days a week, and those who worked a mere seventy hours a week were considered by their friends to be "not very interested in physics." The only time the Laboratory was really deserted was for two hours every Monday night, when Lawrence's beloved "Journal Club" was meeting. He initiated this weekly meeting when the cyclotron looked as though it might become useful in nuclear physics; he and his associates reported to each other the latest publications in nuclear physics, so they would know what to do when their new tools were ready. But soon the Journal Club became a forum in which the rapidly growing Laboratory staff discussed their own discoveries in radioactivity and allied fields.

The 27-inch cyclotron--later converted to a 37-inch pole diameter--was originally used in studies of artificial transmutations induced by high-energy protons. Immediately after the discovery of deuterium by Urey in 1932, Professor G. N. Lewis of the Chemistry Department supplied the Laboratory with a few drops of heavy water, and Lawrence, Lewis, and Livingston observed the first reactions induced by deuterons. The detecting device used in all these early experiments was a "thin ionization chamber" plus a linear amplifier. Such a chamber responds to fast atomic ions (protons and α particles) but not to the β rays from radioactive substances; to detect β rays, one needs a more sensitive device, for example, a Geiger counter. Lawrence and his collaborators made several attempts to manufacture Geiger counters in the Radiation Laboratory, but all their counters suffered from excessively high "background rates." Today, when Geiger counters are commercially available from dozens of companies, it is difficult to believe that Lawrence and his associates could have overlooked the fact that the high backgrounds were the result of a general high level of radioactivity in the whole laboratory--artificial radioactivity, to be more exact! But in those days, the rare experimentalists who could make good Geiger counters were looked upon as practitioners of witchcraft; their less fortunate friends might try for months without hitting the magic formula. So after several abortive attempts to make useful counters, the Berkeley group went back to the linear amplifier technique that others considered even more difficult, but which was nonetheless one that they had mastered. So it was not until the announcement in 1934 of the discovery of artificial radioactivity by Curie and Joliot that Ernest Lawrence and his associates realized why they couldn't make a decent Geiger counter; their whole laboratory was radioactive!

The discovery of artificial radioactivity had been missed by all the accelerator teams then operating throughout the world, so the next few months saw the discovery of dozens of radioactive species by members of the accelerator fraternity. The fact that none of the "machine builders" had noticed the phenomenon of artificial radioactivity puts the oversight by the cyclotron group in proper perspective. It was symptomatic of the general unreliability of all detection devices in those days, coupled with the great complexity of the accelerators themselves, rather than a deficiency in the men as scientists.

It is interesting to note that Irene Curie and Frederic Joliot had the largest source of "trouble-free" polonium in the world. For a quarter of a century, doctors all over the world had taken pleasure in sending their old "radon needles" to Madame Curie, as a token of respect. From many thousands of these otherwise useless glass tubes, she had isolated more than a curie of polonium--by far the strongest sample of the element in the world. Her daughter and son-in-law put this precious sample to good use in their nuclear investigations in the early 1930's. They used their polonium to generate neutrons, but they didn't realize what they had done. Chadwick read their report and immediately recognized its significance; a few days later, he had proved that the neutron existed. (So Lawrence was in good company when he let two big discoveries go unnoticed in his Laboratory.)

The Curie-Joliots used their unique source two years later in the discovery of artificial radioactivity. The fact that polonium does not emit β or γ rays made it possible for them to observe the "induced activity" during the time the α particles were bombarding their aluminum target. Accelerator physicists were denied this simple technique, because of the background radiation from their machines. But even with this advantage, the Curie-Joliots almost missed their great discovery; they almost dismissed the change in counting rate of their first artificially radioactive source as being due to the erratic behavior of their Geiger counter! It was only after the builder of the counter had insisted for several days that his handiwork was reliable that the Curie-Joliots became convinced that the phenomenon of artificial radioactivity really did exist.

That Lawrence's group, and all the other accelerator teams, did not anticipate the work of Fermi and his collatorators in the field of neutron-induced radioactivity is a different story, but again one which has an easy explanation in terms of its setting in time. Calculations of "yields" of nuclear reactions were made every day, and it was painfully obvious that one had to bombard a target with more than a million fast particles in order to observe one nuclear reaction. Everyone had thought of the possibility of using the high-energy α particles from the artificial disintegration of lithium as substitutes for the slower α particles from the decay of polonium. But "that factor of a million" always stood in the way, and it finally led to a firmly held conviction that "secondary reactions can't be observed." Certainly, Lawrence and others considered the use of neutrons to produce artificial radioactivity, but the factor of a million always made them turn their minds to other things. But Fermi, who was far removed from the pressures of an accelerator laboratory, looked at the problem from first principles, and realized immediately that <u>every</u> neutron would make a nuclear reaction. In other words, secondary reactions would be as prevalent as primaries, if neutrons were involved. The story of his success is well known, and needn't be repeated here. Lawrence often spoke of the day he first heard of Fermi's classic experiments, and how he verified Fermi's discovery of the neutron-induced radioactivity of silver, within a minute or two. He merely took a fifty cent piece from his pocket, placed it near the cyclotron, and then watched it instantaneously discharge an electroscope after the cyclotron had been turned off.

One normally doesn't dwell so long in a scientific obituary on those occasions when the subject failed to find what he was apparently equipped to find. But Ernest Lawrence wouldn't want such interesting bits of history to be swept under a rug. He was so accustomed to his own success and to that of his laboratory colleagues, that he enjoyed recounting his mistakes, without ever mentioning the mitigating factors just recounted. From 1931 until 1950, Lawrence's laboratory was the home of the highest-energy beams of particles in the world, and for several years in the mid-fifties, the Bevatron was the highest-energy machine in operation. Such figures by themselves mean nothing, but they do indicate that in that period the Radiation Laboratory could do important experiments that were difficult, if not impossible, at other institutions. Ernest Lawrence was always conscious of the importance of beam intensity in addition to beam energy, and worked diligently to see that all his accelerators kept producing larger currents of accelerated ions. He often spoke with satisfaction of the proven value of his long campaign to increase beam current (often in the face of opposition from his younger colleagues, who wanted to "use what we have" rather than shut down for improvements).

By 1937 Lawrence had succeeded in pushing the cyclotron current up to 100 microamperes, at 8 million electron volts. Other accelerator builders of this period were content with 1 microampere at 1 million electron volts. Lawrence's young associates felt sure that the cyclotron had reached its peak in the "beam intensity department," but Lawrence soon found that the cyclotron was doing ten times as well as anyone had suspected. Lawrence was always the best cyclotron operator in this period; he could "get more beam" than anyone else. One day, he noticed that as the magnetic field passed through the resonance value, the meters in the oscillator power supply showed a "loading" ten times as great as the 800 watts one would predict (100 microamperes times 8 million volts = 800 watts). Lawrence knew at once that this power must be going into an accelerated beam that wasn't reaching the detector. It was soon after this that Lawrence encouraged Martin Kamen to install two water-cooled probes to intersect the circulating beam that had formerly been lost. So now, in addition to the 100 microamperes of "external beam," another milliampere was always at work producing radioactive isotopes for Dr. John Lawrence's medical program.

One of the important reasons for Lawrence's concern with high intensity was his great interest in the medical and biological applications of the radiations from the cyclotron and from the radioactive substances it produced. A physicist can ordinarily compensate for a lower intensity by building more sensitive detectors. But in medicine, one must accept the human body as it is; if the radiation levels are too low, the body uses its healing mechanisms to minimize the effects that are under investigation.

Lawrence persuaded his brother John to come to Berkeley at first as a visitor, to advise him on the potential hazards of the neutrons from the 27-inch cyclotron. Later, John Lawrence headed a strong research team that investigated many phases of the new radiation medicine and biology. Ernest Lawrence, who had abandoned a medical career for one in physics, now had the vicarious pleasure of a "second life" in medical physics. He gave the Laboratory medical program his strongest support, often in the face of keen disappointment on the part of some of the physicists who worked so hard to keep the cyclotron in operating condition and whose research efforts had to be curtailed. In 1938 and 1939, all physics at the cyclotron was suspended for a full day each week, so that terminal cancer patients could be treated with neutrons from the 37-inch cyclotron. The oil-stained cyclotron was cleverly disguised with a set of white panels, and the patients were led through a side door into what appeared to be an immaculate hospital room.

Lawrence was actively engaged in promoting the use of radioactive isotopes throughout the biological and chemical fields, both as tracers and as sources of radiation. He committed his Laboratory to furnishing the materials for experimental programs in many University of California departments, and he derived great satisfaction from the important discoveries made by his colleagues in other scientific disciplines. The collaboration between physicists and biologists naturally blossomed after the development of the nuclear reactor, when radioactive isotopes became widely available. But the real pioneer in this area was Ernest Lawrence, who shared the hard-earned fruits of his labor with his University colleagues because he thought it was in the best interest of science.

Everyone recognized that Lawrence was responsible for the steady increase in the peak energy of the accelerated beams available in the Radiation Laboratory, and throughout the world. But Lawrence himself seemed to derive even more satisfaction from his steady drive toward higher beam intensities. He could point to the discovery of carbon-14, by Rubin and Kamen, as an immediate dividend of his obsession with higher intensities. ^{14}C could not have been discovered at any other laboratory in the world with the detection techniques then available. And although Lawrence never said so to anyone but his closest associates, he was convinced that his great concern for beam intensity was what had really made the whole Manhattan District program possible.

Physicists on both sides of the Atlantic had spent a great deal of effort in theoretical and experimental design studies for nuclear chain reactors, but these devices apparently had no relevance to the "war effort." It was not until the discovery of plutonium and its fissionable properties by Lawrence's co-workers at the Radiation Laboratory that the reactor program had a clearly defined role in the military program. And as was true in the case of ^{14}C, plutonium couldn't have been found anywhere but at Berkeley; its discovery required the enormous particle fluxes that came from Lawrence's long campaign to increase both the energy and the intensity of his ion beams.

As is well known, the Manhattan District's program was three-pronged; uranium-235 for bombs was made by two isotope separation processes and plutonium-239 was created in the chain reactors. Lawrence spent several of the war years perfecting the Calutron, or mass-spectrometer method of separating ^{235}U from ordinary uranium.

It is probably true that no one but Ernest Lawrence could have made a success of the Calutron process; the ion currents required were millions of times as great as anyone had even dreamed of before. So Lawrence's concern with beam intensity was the key element in two of the three successful attempts to produce fissionable material in the war period.

In believing that his own pioneering work in two of the three major processes was the key to the acceptance of the Atomic Bomb Project by the government, Lawrence in no way depreciated the accomplishments of the reactor designers or of the gaseous diffusion experts. He simply felt, from a great deal of experience with high-level Government officials, that the project couldn't have "been sold" unless there was one "sure way" to make fissionable material before the war was over. (The threat of postwar Congressional investigations into the waste of money on "boondoggles" hung over the scientific policy makers in those days.) The Calutron process was complicated and expensive relative to the gaseous diffusion process, but once a single unit had worked, there was no doubt that the application of large amounts of money could produce enough material for a bomb. Lawrence, who was personally involved in many of the key sessions that culminated in the establishment of the Manhattan District, always felt that this argument convinced the decision makers in Washington to authorize all three approaches to the production problem.

To return to the cyclotron development, we can note two milestones: the initial operation of the 60-inch cyclotron in 1939, and the authorization of the 184-inch cyclotron in 1940. The 60-inch cyclotron was installed in the new Crocker Radiation Laboratory; its historic contribution to the discovery of plutonium has already been mentioned. The Rockefeller Foundation gave the University of California 1.25 million dollars in 1940 to build the 184-inch cyclotron on the hill behind the Berkeley Campus.

Before work could be started on the "giant cyclotron," as Lawrence referred to it in those days, international events conspired to change the character of the Laboratory. In the summer of 1940, Lawrence returned to Berkeley from a New York visit with his longtime friend, Dr. Alfred Loomis. Loomis had played a key role in the discussions with the Rockefeller Foundation officials, and Lawrence had great respect for his counsel. Loomis had been active in the establishment of the National Defense Research Committee, which was headed by Vannevar Bush, Karl Compton, and James Conant. Loomis introduced Lawrence to the members of the British Scientific Mission, who were visiting the country at that time. From his old friend, Sir John Cockcroft, Lawrence learned for the first time of the outstanding scientific contributions to the British war effort, many of them made by nuclear physicists. Before returning to Berkeley, he joined the NDRC Microwave Committee, under the chairmanship of Alfred Loomis. He assumed the responsibility for recruiting a group of young experimental nuclear physicsts to help the British "fight the scientific war." He persuaded Lee DuBridge to leave his own cyclotron at Rochester, New York, and head the embryonic Radiation Laboratory at Massachusetts Institute of Technology. (The name of the laboratory, together with its staff of nuclear physicists, was intended as a "cover" to mislead the curious into believing that its mission was in the field of nuclear fission.

In those days, fission was not treated seriously as a war project, but the mere idea that planes could be detected by radio echoes was considered to be exceedingly secret information.)

Lawrence recruited a fine staff of young physicists, many of them his former students. Most of them gave up their exciting careers in nuclear physics, more than a year before Pearl Harbor, for the simple reason that Ernest Lawrence came to see them and told them it was the most important thing they could do. From his own Laboratory he recruited McMillan, Salisbury, and Alvarez. The MIT Radiation Laboratory came into being in November of 1940, and its contributions to radar are too well known to be recounted here. Lawrence visited the laboratory frequently in its first year, and kept abreast of its activities in that period. But it was soon obvious that the laboratory could stand on its own feet, and Lawrence had other demands on his talents.

In the summer of 1941, Lawrence became involved in the antisubmarine warfare program; German submarines were then close to destroying the convoy system that was supplying Great Britain from the United States. Lawrence again acted as the chief recruiting agent for the new underwater sound laboratories, and persuaded McMillan to leave MIT for San Diego. Shortly thereafter, he converted the 37-inch cyclotron into a mass spectrometer for separating small amounts of ^{235}U from ordinary uranium. This work convinced him that the electromagnetic separation technique could be expanded to become a large-scale process for producing ^{235}U. In his characteristic style, he immediately committed his Laboratory and his reputation to the project. Although he had recently staffed two laboratories with many of his best students, he still could call on a number of his top-flight protégés to restaff the Berkeley Radiation Laboratory.

From the summer of 1941 until the summer of 1945, the Radiation Laboratory worked around the clock on the technical problems involved in the electromagnetic separation of ^{235}U. The 184-inch cyclotron magnet, assembled in a new laboratory high above the Berkeley campus, served as a working model for the hundreds of mass spectrometers soon to be constructed in Oak Ridge, Tennessee. Lawrence himself, and all his associates, worked twelve hours a day, seven days a week. It was a Herculean effort, and it was almost solely responsible for the ^{235}U that made up the Hiroshima bomb. The thermal diffusion process and the gaseous diffusion process contributed only in a minor way to the overall separation of the isotopes for the first bomb; Lawrence's fantastic mass spectrometer plant at Oak Ridge bore the brunt of the effort. (Shortly after the war, Lawrence's plant was shut down, and the more efficient gaseous diffusion plant took over the peacetime production of ^{235}U.)

One of the greatest difficulties one encounters in writing of Ernest Lawrence's career is that so much must be omitted in order to keep the manuscript within reasonable bounds. It is also a pity that Lawrence himself wrote nothing of the experiences he had in his five intensely busy war years, nor of the technical problems he met and solved in that period. Because of this, there is an apparently comprehensive book on the electromagnetic separation of isotopes with but a single mention of his name. However, in the minds of those who worked with him during the war there is no question that his foresight, daring, leadership, and technical ingenuity were the key ingredients in the success of the venture.

In early 1945, when the completed Oak Ridge plant was in full production, Lawrence turned his thoughts toward the postwar period. He persuaded General Groves (for whom he had a great respect that was apparently reciprocated) to authorize the conversion of the Laboratory to its peacetime mission. Manhattan District funds were accordingly made available to complete the 184-inch cyclotron, and to build a proton linear accelerator and an electron synchrotron. The synchrotron had just been invented by McMillan, and Alvarez's radar experience had convinced him that a proton linear accelerator could be built.

In the fall of 1945, the prewar Berkeley team was reassembled, together with some talented newcomers whose abilities had first come to light in the wartime effort. One of the first major decisions Lawrence had to make concerned the 184-inch cyclotron. It had been planned as a "conventional cyclotron," but its performance under those circumstances would have been marginal at best. McMillan's theory of phase stability indicated that the 184-inch machine would perform more satisfactorily as a synchrocyclotron; its proton energy should rise to 350 MeV, from the earlier design figure of perhaps 70 MeV. But, on the other hand, no one had ever built a synchrocyclotron, and the problems foreseen were formidable. Lawrence and McMillan called for the immediate rebuilding of the old 37-inch cyclotron. It was soon operating as the world's first synchrocyclotron, and it showed that the new device was much simpler to build and operate than the originally proposed conventional cyclotron. As a result of these early model tests, the

184-inch synchrocyclotron was accelerating deuterons to 180 MeV, and helium nuclei to 360 MeV, in late 1946.

Since this is the story of Ernest Lawrence's career, very little will be said about the new laboratory activities that were primarily the responsibilities of his younger colleagues. In addition to the two accelerators built under the supervision of McMillan and Alvarez, there were two important new chemical projects under Seaborg and Calvin. Seaborg returned to Berkeley from his successful wartime duties as director of the chemical phases of the Plutonium Project in Chicago. Calvin had played several important roles in the OSRD Chemistry Section, and was anxious to study photosynthesis with ^{14}C, which was soon to be in plentiful supply as a direct result of the huge neutron fluxes now available from nuclear reactors. In effect, all of us had "gone away as boys, and come back as men." We had all initiated large technical projects, and carried them to completion as directors of large teams of scientists and technicians. We were all prepared to reassume our subordinate roles, with Ernest Lawrence as our "leader" once again. But he made it clear by his actions, if not by his words, that we were to be free agents. We made all our own technical and personnel decisions, and for the first few years after the war, at least, we had unlimited financial backing. It was not until the "blank check" from the Manhattan District was replaced by more normal budgeting procedures that any of us felt any limitations on our ability to do whatever we thought should be done in our own areas of responsibility. Ernest Lawrence showed a keen interest in what we were doing, but in these early postwar years he never gave any sign that he thought his function was to give us advice of any kind. Wise parents let their children solve all the problems they can, but they stand by to help when the problems are too difficult of solution. Ernest Lawrence was always a wise "scientific parent"; all of us who were fortunate enough to be his "scientific children" will remember with gratitude the help and understanding he gave us when we needed it, as well as the freedom he gave us to solve our own problems when it seemed that we could eventually succeed.

With the completion of the 184-inch cyclotron, Lawrence once again became an active research worker. He had not been directly involved in any particular experiment since his 1935 work on deuteron-induced radioactivities. As soon as the 184-inch cyclotron was operating, Lawrence became an active participant in experiments using the recently discovered high-energy neutrons produced by "deuteron stripping." He personally discovered the delayed neutron activity that he and his colleagues soon showed was due to nitrogen-17. It was a refreshing experience for many of his young colleagues, who had known him largely as a Laboratory director and as a person with great skill in diagnosing troubles in complicated scientific tools, to see the complete devotion he now showed to personal involvement in basic scientific research. Soon after the ^{17}N mystery was unraveled, Lawrence became convinced that the 184-inch cyclotron could produce the newly discovered π mesons. Edward Teller had pointed out that even though the cyclotron's energy seemed too low to produce pions, there was some hope that with the aid of the Fermi momentum of an incident α particle, and of a carbon target nucleus, the job could be done. Lawrence worked closely with Eugene Gardner in this period, designing experimental setups using nuclear emulsions as detectors, but these efforts were unsuccessful.

Shortly after these early experiments, C. M. J. Lattes arrived in Berkeley. He had been a member of the Bristol team that, under the leadership of C. F. Powell, had recently discovered the π meson. He quickly showed that the difficulties encountered by Gardner and Lawrence had been due to improper processing of the nuclear emulsions. Lattes immediately corrected the Berkeley development techniques, and a new set of exposures was made soon after his arrival, using the apparatus designed by Lawrence and Gardner. Lattes also brought a familiarity with the tracks of π mesons in emulsion that was available to only a few physicists in the world at that time, and he applied his keen eyesight to the tedious scanning of the exposed plates. His diligence was rewarded with success, one evening, when he observed tracks of the first artificially produced negative pion coming to rest in an exposed nuclear emulsion plate. He and Gardner immediately called Lawrence, who was entertaining visitors at an Oakland restaurant. Lawrence left the dinner, and as soon as he looked through the microscope, he experienced one of the greatest thrills of his life. Although he had played a major role in the discovery, both by his activities in procuring the money for and designing the 184-inch cyclotron and, more particularly, in the design of the apparatus used in the experiment, he characteristically insisted that the historical paper should be signed "Gardner and Lattes."

For a period of several years after the war, Lawrence devoted all his waking hours to the pursuit of basic science at the Radiation Laboratory. Lawrence was fortunate in having an administrative staff that had learned to cope with the problems of a much larger wartime organization, so he was relatively free to concentrate on the scientific activities of the many sections of his Laboratory. He took a great personal interest in the programs on photosynthesis, medical physics, and nuclear chemistry, but the intensity of his involvement in the physics program was a source of amazement to his younger colleagues. It was a rare week that he didn't spend several hours each evening and much of Saturday and Sunday in the cyclotron building, or wandering through other laboratories, talking to everyone from research assistants to visiting professors. Even though the Laboratory was now almost a hundred times as large in manpower as it had been 15 years earlier, everyone still had the feeling that he was a "member of Ernest Lawrence's team"--not simply an employee of the Radiation Laboratory. Even at present, almost twenty years after this exciting phase of the Laboratory's history, technicians as well as senior staff members continue to swap their favorite stories of "the time Professor Lawrence looked over my shoulder at 3:00 a.m. and asked what I was doing."

In 1948, William Brobeck convinced the Laboratory staff that a proton synchrotron could be built in the multibillion-electron-volt range. Lawrence immediately assumed the responsibility of securing financial backing for the "Bevatron," from the AEC and the Congress. The Brookhaven National Laboratory had recently been established, and it was simultaneously asking for support for a similar accelerator. The Atomic Energy Commission eventually authorized the 6.2-BeV Bevatron at Berkeley, and the 3-BeV Cosmotron on Long Island. The Cosmotron came into operation before the Bevatron, for two reasons. Lawrence, with a conservatism that many of his associates had not observed before, decided to build a quarter-scale model of the Bevatron, to be sure that the untried principle of "external injection" into a synchrotron would work. The model, as designed by Brobeck, worked within 9 months of the decision to build it, and the "go-ahead signal" for the Bevatron was immediately given by Lawrence. But soon after this decision had been made, the USSR exploded its first nuclear device, and Lawrence turned his attention once again to problems of national security.

Lawrence played a key role in the U. S. decision to embark on a program leading to the development of a thermonuclear bomb. Soon after President Truman made the decision to build the hydrogen bomb, Lawrence became concerned with the serious shortage of uranium reserves available to the United States. He felt that the country might soon be plagued with a neutron shortage, occasioned by the dwindling supply of ^{235}U. His solution to the problem was the construction of a high-energy, high-current deuteron linear accelerator that would produce neutrons by impact on heavy targets. Experiments at the 184-inch cyclotron had shown surprisingly high "neutron multiplicities" in such collisions. A 60-foot-diameter 60-foot-long test section of the accelerator was built at the Laboratory's newly acquired Livermore site, and it accelerated unprecedented currents of close to one ampere to several million electron volts. The project was abandoned when another solution to the neutron shortage problem proved successful; the AEC offered substantial cash payments for uranium finds in the continental United States. A flood of prospectors with Geiger counters promptly showed that there were enormous and previously unknown reserves of uranium in the Rocky Mountain states. (At the present date, the Canadian government is considering a program patterned after Lawrence's scheme of electronuclear neutron production, as an effective competitor to nuclear reactors. An advantage of the accelerator method is the lower power density in the neutron-producing target, per unit of neutron flux, than in nuclear reactors.)

In 1952, Lawrence expressed concern that all the U. S. nuclear weapons design effort was concentrated in a single government laboratory. He had great respect for the members of the Los Alamos Laboratory, and for their extraordinary accomplishments. Nonetheless, his extensive experience as a scientific consultant to many of the largest U. S. technical corporations gave him first-hand experience with the benefits of a healthy competition between independent development laboratories. He therefore urged the AEC and the Joint Congressional Committee on Atomic Energy to set up a second weapons laboratory. He offered the Livermore site as a suitable location, and pledged his personal oversight of the new project. The Laboratory was established in 1952, with Herbert York as director, and Edward Teller as a senior member of the staff. Most of the key group leaders were young physicists, chemists, and engineers trained in the Berkeley Laboratory. Lawrence spent most of his remaining time and effort on the affairs of the Livermore Laboratory until his death in 1958. The young competitor in the field of nuclear weapons "stubbed its toe" several times in the early years, but later it made the substantial contributions to the design of nuclear weapons that Lawrence had foreseen as its destiny. Without the steadying hand of Ernest Lawrence in the difficult early years, the Livermore Laboratory might easily have failed in its purpose, and the country as a whole would have been the loser. Lawrence's latest protégés, the first three directors of the Livermore laboratory, are a remarkable group of young men. Each of them, in turn, went from Livermore to a position of great responsibility in the Pentagon -- the Director of Defense Research and Engineering. Herbert York left Washington to become Chancellor of the San Diego campus of the University of California, Harold Brown became Secretary of the Air Force at the age of 38, and John Foster is still "DDR and E."

Concurrently with the establishment of the Livermore Laboratory, Lawrence developed what he called his "hobby," to divert his mind from the enormous pressures to which it was subjected in these years. He became fascinated with the technical challenges of color television, and invented some very ingenious solutions to the difficult problems of that field. Unfortunately, the business problems involved in the financing of initially unprofitable color television tube production lines were more difficult than the technical problems Lawrence tackled and solved, largely by himself. For these reasons, the Lawrence "chromatron" was never put into production in this country. But it is now being sold in Japan, and the Sony Company has announced plans to introduce it into the U. S. market in 1967.

In the middle 50's John Lawrence and Dr. Albert Snell, Ernest's personal physician, urged him to shed some of the great burdens he was carrying. On one occasion Ernest and Molly Lawrence took a leisurely ocean voyage to India, and it seemed that the period of rest had greatly relieved the intestinal problems that were aggravated by the pressures under which he had worked for so long. But the problems were only temporarily ameliorated; surgical treatment was recommended, but it was unsuccessful. Ernest Lawrence died in a Palo Alto hospital on August 27, 1958, without recovering consciousness after major surgery.

Lawrence's untimely death at the end of his fifty-seventh year was a great shock to his wide circle of friends in science, government, and industry. He had led a life of enormous usefulness to science and to his country; his influence was largely by example, and by the strength of his character. He had great admiration for his scientific colleagues who could influence national policies through high administrative office, or by writings, or in public speeches. But he felt that the proper way for him to be useful was to let policy makers know that he was available for consultation on his own personal opinions -- never as a spokesman for a pressure group. Leaders in government and industry respected his distaste for the limelight, and sought his counsel. His long record of success in the difficult tasks he set for himself, and the accuracy of his prognostications in diverse fields, made his advice most compelling to those who sought it.

For those who had the good fortune to be close to him both personally and scientifically he will always seem a giant among men. At present, when government support for basic science appears to be on the wane, one hears more and more frequently the lament, "The real difficulty is that there isn't an Ernest Lawrence any more."

Lawrence's place in the history of science is secure. He will always be remembered as the inventor of the cyclotron, but more importantly, he should be remembered as the inventor of the modern way of doing science. Element 103 was named Lawrencium by his young associates who discovered it shortly after his death. After his death, his friends endowed the Lawrence Hall of Science, to which science teachers from all over the country will come on year-long fellowships to learn the most modern methods of teaching their subjects. The Lawrence Hall of Science will soon be in operation, just above the Laboratory that Ernest Lawrence founded and nurtured and loved for so long, and that is now appropriately known as the Lawrence Radiation Laboratory.

Honorary Degrees

Degree	University	Dates
Sc. D	University of South Dakota	1936
Sc. D	Stevens Institute of Technology	1937
Sc. D.	Yale University	1937
Sc. D.	Princeton University	1937
LL. D.	University of Michigan	1938
Sc. D.	University of Chicago	1941
Sc. D.	Harvard University	1941
Sc. D.	Rutgers University	1941
LL. D.	University of Pennsylvania	1942
Sc. D.	McGill University	1946
Sc. D.	University of British Columbia	1947
Sc. D.	University of Southern California	1949
Sc. D.	University of San Francisco	1949
LL. D.	University of Glasgow	1951

Awards and Medals

Elliot Cresson Medal of Franklin Institute, 1937

Research Corporation Prize and Plaque, 1937

Comstock Prize of National Academy of Science, 1937

Hughes Medal of Royal Society (England), 1937

Nobel Prize in Physics, 1939

Duddell Medal of Royal Physical Society, 1940

William S. Dunn Award, American Legion, 1940

National Association of Manufacturers Award, 1940

Holley Medal, American Society Mechanical Engineers, 1942

Copernican Citation, 1943

Wheeler Award, 1945

Medal for Merit, 1946

Medal of Trasenster, Association of Graduate Engineers, University of Liège, Belgium, 1947

Officer de la Légion d'Honneur, France, 1948

Phi Delta Epsilon Annual Service Award, 1948

William Proctor Prize of the Scientific Research Society of America, 1951

Faraday Medal, 1952

American Cancer Society Medal, 1954

Enrico Fermi Award, 1957

Sylvanus Thayer Award, 1958

Memberships

Member

American Representative at Solvay Congress, Brussels, 1933

National Academy of Science, 1934

American Philosophical Society, 1937

Phi Beta Kappa

Sigma Xi

Gamma Alpha

American Scandinavian Foundation, 1942

Board of Foreign Scholarships, Department of State (Fulbright Act-79th Congress), 1947

Newcomen Society, 1948-51

Physical Society of Japan, 1954

Board of Trustees:	Carnegie Institution of Washington, 1944
	Rand Corporation, 1956
Board of Directors:	Yosemite Park & Curry Company
	Monsanto Chemical Company, 1957

Foreign Member

Royal Swedish Academy of Sciences, 1952

Honorary Member

Bohemian Club (California), 1940

California Academy of Sciences, 1940

Academy of Science, U. S. S. R., 1943

Royal Irish Academy of Science, 1948

Fellow

American Physical Society

American Association for the Advancement of Science

American Academy of Arts & Sciences

Honorary Fellow

Leland Stanford Junior University, 1941

National Institute of Sciences of India, 1941

The Institute of Medicine of Chicago, 1941

Royal Society of Edinburgh, 1946

The Physical Society, 1948

Indian Academy of Sciences, 1953

Appointments

National Defense Research Committee, June 1940.

National Academy of Sciences, National Research Council Reviewing Committee, April 1941.

Office of Scientific Research and Development, 1941.

Program Chief, S-1 Section of the Office of Scientific Research and Development (Uranium Section), November 1941 - May 1942.

S-1 Executive Committee of the Office of Scientific Research and Development, May 1942 - March 30, 1946.

Scientific Advisor and Project Leader, Manhattan District, May 1, 1943.

Research Board for National Security, 1945-46.

Elector, The Hall of Fame of New York University, 1947.

Advisor on the U. S. Delegation to the International Conference on the Peaceful Uses of Atomic Energy, Geneva, Switzerland, August 1955.

Physical Science Administration Officer (Consultant), Brussels Universal International Exhibition of 1958, Department of State, 1956-58.

U. S. Delegation to the Conference of Experts to Study the Possibility of Detecting Violations of a Possible Agreement on Suspension of Nuclear Tests, Geneva, Switzerland, July 1958 (three-member committee appointed by President Eisenhower).

PUBLICATIONS OF ERNEST O. LAWRENCE

1. The Charging Effect Produced by the Rotation of a Prolate Iron Spheroid in a Uniform Magnetic Field. Phil. Mag., Vol. 47, 842-847, May 1924.

2. The Photo-Electric Effect in Potassium Vapour as a Function of the Frequency of the Light (Thesis). Phil. Mag., Vol. 1, 345-359, August 1925.

3. The Role of the Faraday Cylinder in the Measurement of Electron Currents. Proc. Natl. Acad. Sci. U. S., Vol. 12, No. 1, 29-31, January 1926.

4. Transition Probabilities: Their Relation to Thermionic Emission and the Photo-Electric Effect. Phys. Rev., Vol. 27, No. 5, 555-561, May 1926.

5. A Principle of Correspondence. Science, Vol. 64, No. 1649, 142, August 6, 1926.

6. The Ionization of Atoms by Electron Impact. Phys. Rev., Vol. 28, No. 5, 947-961, November 1926.

7. On the Nature of Light (with J. W. Beams). Proc. Natl. Acad. Sci. U. S., Vol. 13, No. 4, 207-212, April 1927.

8. Ultra-Ionization Potentials of Mercury. J. Franklin Inst., 91-94, July 1927.

9. On the Lag of the Kerr Effect (with J. W. Beams). Proc. Natl. Acad. Sci. U. S., Vol. 13, No. 7, 505-510, July 1927.

10. On Relaxation of Electric Fields in Kerr Cells and Apparent Lags of the Kerr Effect (with J. W. Beams). J. Franklin Inst., Vol. 206, No. 2, 169-179, August 1928.

11. Element of Time in the Photoelectric Effect (with J. W. Beams). Phys. Rev., Vol. 32, No. 3, 478-485, September 1928.

12. Photo-Ionization of the Vapors of Caesium and Rubidium (with N. E. Edlefsen). Phys. Rev., Vol. 34, No. 2, 233-242, July 15, 1929.

13. Photo-Ionization of Potassium Vapor (with N. E. Edlefsen). Phys. Rev., Vol. 34, No. 7, 1056-1060, October 1, 1929.

14. Effect of Intense Electric Fields on the Photoelectric Behavior of Alkali Films on Tungsten (with L. B. Linford). Phys. Rev., Vol. 34, No. 11, 1492, December 1, 1929.

15. Early Stages of Electric Spark Discharges (with F. G. Dunnington). Phys. Rev., Vol. 34, No. 12, 1624-1625, December 15, 1929.

16. Intense Source of Continuous Ultraviolet Light (with N. E. Edlefsen). Rev. Sci. Instr., Vol. 1, No. 1, 45-48, January 1930.

17. Abstract: Broadening of Spectrum Lines During Early Stages of Spark Discharges (with F. G. Dunnington). Phys. Rev., Vol. 35, 134, January 1930.

18. On the Early Stages of Electric Sparks (with F. G. Dunnington). Phys. Rev., Vol. 35, No. 4, 396-407, February 15, 1930.

19. Effect of Intense Electric Fields on the Photoelectric Properties of Metals (with L. B. Linford). Phys. Rev., Vol. 36, No. 3, 482-497, August 1, 1930.

20. On the Direction of Emission of Photoelectrons from Potassium Vapor by Ultraviolet Light (with M. A. Chaffee). Phys. Rev., Vol. 36, No. 6, 1099-1100, September 15, 1930.

21. On the Production of High Speed Protons (with N. E. Edlefsen). Science, Vol. 72, No. 1867, 376-377, October 10, 1930.

22. Production of High Speed Canal Rays without the Use of High Voltages (with D. H. Sloan). Proc. Natl. Acad. Sci., Vol. 17, No. 1, 64-70, January 1931.

23. Production of High Speed Protons without the Use of High Voltages (with M. S. Livingston). Phys. Rev. Vol. 38, No. 4, 834, August 15, 1931.

24. Production of Heavy High Speed Ions without the Use of High Voltages (with D. H. Sloan). Phys. Rev., Vol. 38, No. 11, 2021-2032, December 1, 1931.

25. Production of High Speed Light Ions without the Use of High Voltages (with M. S. Livingston). Phys. Rev., Vol. 40, No. 1, 19-35, April 1, 1932.

26. Disintegration of Lithium by Swiftly Moving Protons (with M. S. Livingston and M. G. White). Phys. Rev., Vol. 42, No. 1, 150-151, October 1, 1932.

27. Disintegration of Boron by Swiftly Moving Protons. Phys. Rev., Vol. 43, No. 4, 304-305, February 15, 1933.

28. The Disintegration of Aluminum by Swiftly Moving Protons (with M. S. Livingston). Phys. Rev., Vol. 43, No. 5, 369, March 1, 1933.

29. The Emission of Protons from Various Targets Bombarded by Deutons of High Speed (with G. N. Lewis and M. S. Livingston). Phys. Rev., Vol. 44, No. 1, 56, July 1, 1933.

30. The Emission of Alpha-Particles from Various Targets Bombarded by Deutons of High Speed (with G. N. Lewis and M. S. Livingston). Phys. Rev., Vol. 44, No. 1, 55-56, July 1, 1933.

31. Neutrons from Deutons and the Mass of the Neutron (with M. S. Livingston and M. C. Henderson). Phys. Rev., Vol. 44, No. 9, 781-782, November 1, 1933.

32. Neutrons from Beryllium Bombarded by Deutons (with M. S. Livingston and M. C. Henderson). Phys. Rev., Vol. 44, No. 9, 782-783, November 1, 1933.

33. The Emission of Protons and Neutrons from Various Targets Bombarded by Three Million Volt Deutons (with M. S. Livingston). Phys. Rev., Vol. 45, No. 3, 220, February 1, 1934.

34. The Disintegration of Deutons by High Speed Protons and the Instability of the Deuton (with G. N. Lewis, M. S. Livingston, and M. C. Henderson). Phys. Rev., Vol. 45, No. 4, 242-244, February 15, 1934.

35. Artificial Radioactivity Produced by Deuton Bombardment (with M. C. Henderson and M. S. Livingston). Phys. Rev., Vol. 45, No. 6, 428-429, March 15, 1934.

36. On the Hypothesis of the Instability of the Deuton (with G. N. Lewis, M. S. Livingston, and M. C. Henderson). Phys. Rev., Vol. 45, No. 7, 497, April 1, 1934.

37. The Multiple Acceleration of Ions to Very High Speeds (with M. S. Livingston). Phys. Rev., Vol. 45, No. 9, 608-612, May 1, 1934.

38. The Transmutation of Fluorine by Proton Bombardment and the Mass of Fluorine 19 (with M. C. Henderson and M. S. Livingston). Phys. Rev., Vol. 46, No. 1, 38-42, July 1, 1934.

39. Radioactivity Artificially Induced by Neutron Bombardment (with M. S. Livingston and M. C. Henderson). Proc. Natl. Acad. Sci. U. S., Vol. 20, No. 8, 470-475, August 1934.

40. Radioactive Sodium Produced by Deuton Bombardment. Phys. Rev., Vol. 46, No. 8, 746, October 15, 1934.

41. Transmutations of Sodium by Deutons. Phys. Rev., Vol. 47, No. 1, 17-27, January 1, 1935.

42. Transmutations of Nitrogen by Deutons (with E. McMillan and M. C. Henderson). Phys. Rev., Vol. 47, No. 4, 273-277, February 15, 1935.

43. Transmutations of Aluminum by Deutons (with E. McMillan). Phys. Rev., Vol. 47, No. 5, 343-348, March 1, 1935.

44. A New Type of Excitation Function for Nuclear Reactions (with E. McMillan and R. L. Thornton). Science, Vol. 81, No. 2105, 421-422, May 3, 1935.

45. The Transmutation Functions for Some Cases of Deuteron-Induced Radioactivity (with E. McMillan and R. L. Thornton). Phys. Rev., Vol. 48, No. 6, 493-499, September 15, 1935.

46. Artificial Radioactivity. Ohio J. Science, Vol. 35, No. 5, 388-405, September 1935.

47. The Biological Action of Neutron Rays (with John H. Lawrence). Proc. Natl. Acad. Sci. U. S., Vol. 22, No. 2, 124-133, February 1936.

48. The Transmutation of Platinum by Deuterons (with J. M. Cork). Phys. Rev., Vol. 49, No. 11, 788-792, June 1, 1936.

49. Comparative Effects of X-Rays and Neutrons on Normal and Tumor Tissue (with John H. Lawrence and P. C. Aebersold). Proc. Natl. Acad. Sci. U. S., Vol. 22, No. 9, 543-557, September 1936.

50. On the Apparatus for the Multiple Acceleration of Light Ions to High Speeds (with Donald Cooksey). Phys. Rev., Vol. 50, No. 12, 1131-1140, December 15, 1936.

51. The Comparative Effects of Neutrons and X-Rays on Normal and Neoplastic Tissue (with John H. Lawrence and Paul C. Aebersold). Occasional Publ. Am. Assoc. Advan. Sci., No. 4, 215-19, June 1937.

52. The Biological Action of Neutron Rays. Radiology, Vol. 29, No. 3, 313-322, September 1937.

53a. Science and Technology. Rev. Sci. Instr., Vol. 8, 311-313, September 1937.

53b. Science and Technology. Science, Vol. 86, No. 2231, 295-298, October 1, 1937.

54. An Improved Cyclotron (with Donald Cooksey). Science, Vol. 86, No. 2236, 411, November 5, 1937.

55. Response to Presentation of Comstock Prize. Science, Vol. 86, No. 2236, 406-407, November 5, 1937.

56. The Cyclotron and the Elementary Course in Electricity. Am. Phys. Teacher, Vol. 6, No. 5, 280-281, October 1938.

57. Radioactive Iron and Its Metabolism in Anemia (with P. F. Hahn, W. F. Bale, and G. H. Whipple). J. Am. Med. Assoc., Vol. 111, 2285-2286, December 17, 1938.

58. Atoms, New and Old, Sigma Xi lecture. Science in Progress, First Series, Yale University Press, 1-34, 1939.

59. Radioactive Iron and Its Metabolism in Anemia--Its Absorption, Transportation, and Utilization (with P. F. Hahn, W. F. Bale, and G. H. Whipple). J. Exptl. Med., Vol. 69, No. 5, 739-753, May 1, 1939.

60. Initial Performance of the 60-Inch Cyclotron of the William H. Crocker Radiation Laboratory, University of California (with Luis W. Alvarez, Wm. M. Brobeck, Donald Cooksey, Dale R. Corson, Edwin M. McMillan, W. W. Salisbury, Robert L. Thornton). Phys. Rev., Vol. 56, No. 1, 124, July 1, 1939.

61. The Medical Cyclotron of the William H. Crocker Radiation Laboratory. Science, Vol. 90, No. 2340, 407-408, November 3, 1939.

62. Acceptance of Nobel Prize Award, 1939 Nobel Prize Award in Physics to Ernest Orlando Lawrence, University of California Press, February 29, 1940.

63. Acceptance of Nobel Prize Award. Science, Vol. 91, No. 2362, 323-330, April 5, 1940.

64a. The New Frontiers in the Atom (An Address Delivered at the Fiftieth Anniversary Celebration of Stanford University, June 16, 1941). Smithsonian Rpt. for 1941, Publication 3654, 163-173.

64b. The New Frontiers in the Atom. Science, Vol. 94, No. 2436, 221-225, September 5, 1941.

65. Nuclear Physics and Biology, Molecular Films--The Cyclotron and The New Biology. Rutgers University Press, 63-86, 1942.

66. High Energy Physics. The United States and the United Nations Report Series--5, The International Control of Atomic Energy, Vol. 2, 87-90, July 10, 1946.

67. Initial Performance of the 184-Inch Cyclotron of the University of California (with W. M. Brobeck, K. R. MacKenzie, E. M. McMillan, R. Serber, D. C. Sewell, K. M. Simpson, and R. L. Thornton). Phys. Rev., Vol. 71, No. 7, 449-450, April 1, 1947.

68a. High Energy Physics (Silliman Lecture - October 1947). Am. Scientist, Vol. 36, No. 1, 41-49, January 1948.

68b. High Energy Physics. Science in Progress, Sixth Series, Yale University Press, 55-79, 1949.

68c. High Energy Physics. Scheffield Scientific School, Yale University, 1-26.

69. A Progress Report on the Cyclotron. Science, Vol. 108, No. 2816, 677, December 17, 1948.

70. Address--The Associated Harvard Clubs, Proceedings and Reports of the Annual Meeting, September 9, 10, and 11, 1949, at San Francisco, California, 41-43, 1950.

71. Nobel Lecture, The Evolution of the Cyclotron, Les Prix Nobel, Stockholm, December 11, 1951.

72. A High Vacuum High Speed Ion Pump (with John S. Foster and E. J. Lofgren). Rev. Sci. Instr., Vol. 24, No. 5, 388-390, May 1953.

73. High-Current Accelerators. Science, Vol. 122, No. 3180, 1127-1132, December 9, 1955.

74. Men and Atoms. California Monthly, Vol. 66, No. 4, 24-27, December 1955.

75. Science and the National Welfare. Proceedings of The Robert A. Welch Foundation Conferences on Chemical Research, I. The Structure of the Nucleus, 163-168, November 20-22, 1957.

76. The Growth of the Physics Department, Symposium on the Physical & Earth Sciences honoring the 25th Presidential year of Robert Gordon Sproul, University of California Press, 12-27, Spring 1958.