Edward Uhler Condon, 1902–1974

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The middle third of the twentieth century was the era of hegemony of physics in American science. During that whole period Edward Uhler Condon was a leader in physics, in research of his own, in stimulating research in others, in applying physics, and in calling attention to the effects on all of us of its indiscriminate and irrational application. When he made his first contribution to theoretical physics in 1926, the word physics was not in the vocabularies of most Americans and the revolutionary concepts of quantum mechanics and relativity were just being worked out in Europe; by 1960 the applications of electronics and solid state physics had begun to change our lives irreversibly, and the implications of nuclear physics were manifest to everyone. Ed Condon contributed to each part of this explosive evolution.

Condon's father, William Edward Condon, was a builder of railroads in the west. He and his wife, Carolyn Uhler Condon, moved from place to place as the construction jobs required. When Ed was born, on March 2, 1902, they happened to be in Alamogordo, New Mexico, an ironic concidence not apparent until July 16, 1945. By the time he was ready for high school the family had settled down in Oakland, California. Ed's rival interests, science and journalism, pulled in different directions. In the turbulent year of 1918, when he graduated, rather than going on to college he became a reporter for the Oakland Enquirer. His experience in the ensuing three years had a lasting effect on his attitude toward government and society.

In his own words, "On the Enquirer I specialized in news of organized labor. The dock and timber workers and the migratory farm laborers were drawn to communism. The California State Legislature had passed a strong bill defining criminal syndicalism and making it a felony. The politicians were looking for a place to use it. On November 9, 1919, I was the only reporter from a conservative paper to cover the organization meeting of the Communist Labor Party of California, as it was then called. I wrote sensational stories about this small group of persons, which resulted in indictments against them, and which required that I had to testify against them, in trial after trial, over the next several years; there I watched police framing some of the defendants in matters where I knew the facts to be otherwise. The effect of this involvement on me was to wipe out any desire to be a newspaperman; so I entered the university and went in to physical science largely as a means of escape from the corruption of the world, in addition to the fact that I was genuinely interested in physical science." (Condon, 1973)

He entered as a feshman in the College of Chemistry of the University of California at Berkeley in 1921, but when he learned that his high school physics teacher, W. H. Williams, had joined the physics faculty at Berkeley he switched from chemistry to take Williams' courses in theoretical physics; thus his choice of career was determined.

Courtesy of Photographic Department, University of Colorado.

In that same year, 1922, he married Emilie Honzig, a tiny bundle of energy who encouraged Ed in his scientific work and actively supported his extracurricular activities.

At that time, as Condon has commented, "the physics department was rather weak in a research way, except for the recent addition to the faculty of Raymond T. Birge, who concentrated on the early development of the quantum theory of interpretation of diatomic molecular band spectra, and of Leonard B. Loeb, who spent his life making important contributions to the conduction of electricity through gases." (Condon, 1973)

Ed did well, for he received his AB degree in three years with highest honors, went on directly to graduate work in physics, and received his Ph.D. in 1926. Birge was then making great progress in measuring and analyzing band spectral intensities. Condon put these observations together with a suggestion of James Franck concerning the photodisintegration of diatomic molecules, to come up with an explanation of the regularities in the intensities. He wrote it up over a couple of weekends and presented it to Birge as his Ph.D. thesis. The combined suggestion–explanation later became known as the Franck–Condon principle, when Condon reworked it later in the language of the newer quantum mechanics.

In those years an education in physics was not complete without a year or two spent in Germany. Condon got an NRC fellowship and he and Emilie, with infant Marie (now Mrs. Wayne Thornton, Jr.), spent the fall of 1926 in Göttingen, and the spring of 1927 in Munich. He imbibed the probabilistic interpretation of quantum mechanics from Max Born and, under Arnold Sommerfeld, began the

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wave mechanical formulation of the Franck–Condon principle.

Again the rivalry of interest between the gaining and the imparting of new knowledge intervened. Quoting Condon again, "By spring and summer of 1927, papers on quantum mechanics were appearing at a great rate. In those days a young theoretical physicist was supposed to keep abreast in every area of physics. I became discouraged and decided that if this were the normal pace of work in my chosen field (which it was not!) then I was not equal to the task. About this time there appeared a help-wanted advertisement in the Physical Review for a man to write popular science for an industrial laboratory, the requirement being that the candidate must have newspaper writing experience as well as a Ph.D. in physics. I may well have been the only person in America with that combination at that time. At any rate I applied and was accepted. It turned out that the position was in the public relations department of the Bell Telephone Laboratories, then at 463 West Street in lower Manhattan."

"We returned to America and found an apartment near Columbia in October 1927. At Bell Laboratories, C. J. Davisson and L. H. Germer had just done the experimental work on scattering of low-energy electrons by single crystals of nickel, which led to the discovery of electron diffraction. The importance of this work was not at first appreciated in the business management side of the Laboratories, and I devoted a good deal of attention in the fall of 1927 to explaining to such people that the work was destined to win for the Bell Labs. the first Nobel prize to be awarded to an industrial organization."

"In that fall I found that the American physicists on the Atlantic coast were having as much trouble understanding and assimilating quantum mechanics as I had had in Germany. The profession of theoretical physics was much smaller then than now. As I remember it, Gregory Breit, John Slater, John Van Vleck, and Edwin Kemble were about the only ones in America who were really active in research in quantum mechanics then. I soon found myself in demand as a colloquim speaker at various universities and my boss, R. W. King, encouraged me to accept such invitations, even though they bore little relation to the work I was supposed to be doing for the telephone company."

"I was asked by George Pegram to be a lecturer in physics at Columbia University in the spring of 1928. I accepted and started on my first regular university appointment by giving two graduate courses, one in quantum mechanics and the other on electromegnetic theory of light. Besides giving these courses I travelled around giving colloquium talks on quantum mechanics and on the Franck–Condon principle. So great was the demand for young faculty who could deal with these subjects that in the spring I was offered six assistant professorships for the fall of 1928. I ended up by taking the offer from Karl Compton to go to Princeton."

The chronicler of this biography first met Condon at Princeton in the fall of 1928. He was a new kind of professor. A close-cropped brush of black hair accentuated the roundness of his head, his broad face was usually adorned with a grin, his brown eyes looked steadily but somewhat skeptically at one through rimless glasses. The western vocabulary, the proletarian outlook, the rough-edged kindliness, all contrasted with the eastern establishment manners then the Princeton norm. He was only a year older than the chronicler, but while his greater experience and maturity made a great deal of difference to the student it made no difference to the professor.

Condon has remarked that this first year at Princeton, 1928–29, was the most productive in his life. He has said (Condon 1973), "For teaching I gave a course in quantum mechanics again, improving the notes of the previous Columbia course, and a junior course in classical mechanics, of which the most outstanding student was E. Bright Wilson, now Mallincrodt professor of chemistry at Harvard. Philip Morse, who received a doctorate under K. T. Compton that year, took my course and we worked up the lecture notes into the book *Quantum Mechanics* (Condon and Morse) which was published in the fall of 1929, the first text in the new subject in America."

"I personally wrote the paper that give a fuller statement of the quantum mechanics of the Franck–Condon principle. But by far the most important piece of work done that year was the development of the barrier leakage picture of alpha-particle radioactivity, done with R. W. Gurney. The same idea was developed almost simultaneously by George Ganow, then a postdoctoral fellow in Göttingen. This was the first application of quantum mechanics to details of inner structure of atomic nuclei, and at the same time its success gave a big boost to the probability interpretation of the intensity of the Schrödinger wave, that was only being reluctantly accepted in some quarters."

He still was footloose. He accepted an offer of full professorship at the University of Minnesota for the fall of 1929. But within a year he decided he preferred the stimulation of congenial colleagues to the kudos of the full professorship, so after giving summer courses at Stanford, he returned to Princeton in 1930, where he remained until 1937. During that decade he began to show his ability to spot, energize and guide emerging leaders in the next generation of theoretical physicists. Two of them have reported how he did it.

George Shortley, who became professor of physics at Ohio State University and went into the field of operations research during and after World War II, writes: "I was a senior at the University of Minnesota, taking a physics minor in my electrical engineering program. I signed up for both of his courses. His appearance was quite different from that of any professor I had ever seen. He was jovial, chubby, black-haired, crew-cut and boyish in appearance, wearing cream-colored plus-fours, after the fashion of the day for students, but decidedly not for faculty. One of his courses was the theory of atomic spectra, taught in the quantum-mechanical technique of Dirac before any useable text was available. The other was a course in classical methods of mathematical physics. The two courses meshed perfectly because the same mathematical functions were used in both. Condon was a beautiful lecturer; he had the facility of 'making a difficult subject sound easy' whereas other professors often had the opposite tendency to 'make a simple subject sound difficult'."

"These courses aroused my interest in the theory of atomic spectra and led eventually to my collaboration with Condon on the well-known book on this subject. In fact, later in this same senior year, Condon and I wrote and published our first joint research paper in this field."

"Early in 1930 Condon decided to leave Minnesota and return to Princeton. With considerable difficulty he arranged for me to go with him to Princeton as a graduate student. He also arranged for me to be his research assistant, at a salary that would enable me to support myself. After teaching at Stanford in the summer of 1930, he picked me up in Iowa for the drive back to Princeton with his wife Emilie and their little child, Marie, called Mädi; in fact Mädi sat on my lap for most of the trip."

"When I reported for my duties as research assistant he proposed the collaboration on the monograph on atomic spectra, and we proceeded to outline the chapters then and there. As indicative of the energy he expected of himself and of his students, he asked me the next morning how much I had written. Fortunately I had applied myself the previous afternoon and evening and had the draft of half the introductory chapter to show him."

And Frederick Seitz, president of the National Academy from 1962 to 1969 and since then, president of Rockefeller University, writes (Seitz, 1969): "I was a sophomore at Stanford University and decided to do my bit to reverse 1929 trends by becoming a professional physicist. While still enjoying the euphoria brought on by this decision, I read in the university newspaper that the visiting professor in theoretical physics for the summer quarter would be a brilliant young man, 28 years old, who had discovered the Franck-Condon principle while a graduate student at Berkeley, had attended the great centers of physics in Europe as a National Research Fellow, and had held prominent posts at the Bell Telephone Laboratories, Princeton University and the University of Minnesota. Just a year earlier, he and Ronald Gurney had given an interpretation of alpha disintegration of nuclei in terms of quantum mechanical tunneling. To top it all, the campus paper related that he had earned his way through Berkeley as one of the more worldly reporters of the Oakland Tribune [sic]. In this pursuit he had, among other things, stirred up a lively public discussion of whether a birdcage would weigh more or less when the bird was flying around inside instead of resting on its perch."

"The visitor, Edward Condon, was slated to give a course in modern physics which would be open to duly qualified undergraduates. I succeeded in persuading my father to provide means to attend the summer session and, early in July, found myself perched on a chair in the front row of the lecture room waiting for the show to start. It was not a disappointment."

"Precocious and crew-cut, Ed Condon exhibited even then all the characteristics that have carried him through a lifetime near the center of the stage. He was creative, energetic, perceptive, humerous, restless, eloquent, worldly and friendly. Moreover he knew, on a first-name basis, most of the top-billed physicists on the planet and loved to spin endless anecdotes about them. This was very rich fare for an undergraduate. Condon's lectures were then as now a wonderful combination of logic, anecdotes and humor. In those days, long before physicists were taken seriously by the public, and when they were still all but unknown to congressmen and security officers, Condon was flamboyantly cheerful practically all the time, his occasional bursts of wrath being directed at the petty annoyances of everyday life which plague us all. His bouts with various prominent individuals—particularly with General Leslie Groves—lay far in the future."

"Condon was so deeply interested in people that he quickly came to know personally everyone in the class who managed to act reasonably alive. The small band of embryonic physicists who dominated the first row became his close friends. With Condon's ardent help, continued family indulgence and some permissiveness on the part of the Princeton Admissions Committee, I followed him back to Princeton as a graduate student a year and a half later. His lectures that spring were centered on Frenkel's book on the classical electromagnetic theory of light, which he embellished in countless ways. I still cherish a carbon copy of his notes."

With the completion of The Theory of Atomic Spectra, Condon's interest returned to atomic nuclei. He collaborated with Gregory Breit on a paper on the photodisintegration of the deuteron. But, as he has written, "much more important was the work done jointly with Breit and R. D. Present on the theoretical interpretation of the experimental results obtained by Tuve, Hafstad and Heydenberg at the Carnegie Institution of Washington on the scattering of protons by protons at energies up to about one million volts. These results showed clearly the charge independence of the strong nuclear force between nucleons on which all modern nuclear theory is based."

Between 1928 and 1938 Condon published two books; "Quantum Mechanics" and "The Theory of Atomic Spectra", both with co-authors; nine papers on general quantum mechanics; six papers on atomic spectra, all but one with co-authors; eight papers on the quantum mechanics of molecules, all but two with co-authors; two papers on solid state theory, one with a co-author; and two papers on the biological effects of radiation. In addition there were three articles in the *American Physics Teacher* on simple ways to understand physical concepts, two on semi-philosophical topics and one, published in the *Proceedings of the U.S. Naval Institute*, that can be considered either as an early example of operations research or as an example of Ed's sense of humor. He had come across, in his omnivorous reading, a set of heuristic rules for the amount of food a shipboard cook should prepare, as a function of the number of men to be served. Assuming that the rule represents a balance between satisfying the men's shipboard appetites and reducing the amount of food left over, he determined the parameters of the normal distribution of the men's appetites that the rule inferred and then embellished it with comments on the implications of the distribution and on the validity of the conclusion that there was a nonzero fraction of the men with negative appetites. The conclusions seemed to puzzle some commentators in later issues of the Proceedings. This is by way of illustrating that, in spite of his earlier noted complaint at keeping abreast of progress in physics, Condon did, in fact, read and understand an unusually large sample of scientific literature.

Princeton could not hold him long. In 1937 he accepted the post of associate director of research at the Westinghouse Electric Corporation. He moved his family, now increased by two sons, Paul Edward (now on the physics faculty at the University of California at Irvine) and Joseph Henry (now with the Bell Telephone Laboratories) to Pittsburg. Westinghouse wanted to strengthen its work

in fundamental physics and assured Condon of liberal support and a free hand in developing such work at the laboratories in East Pittsburg. Construction had already been started on a large pressurized van der Graaff machine for nuclear work. The project was put under Condon's direction and other lines of work were initiated.

Once again Condon became the center of a lively community of stimulating individuals. He purchased a roomy house close to Wilkinsburg that seemed to be undergoing continual growth and was usually bursting with interesting, if occasionally unconventional, visitors. Condon not only brought into closer communication the promising young scientists and engineers already employed at Westinghouse, but soon added new faces, through a system of postdoctoral research fellowships. Under his leadership the laboratory quickly grew to the state where it could become a significant factor in the research and development that was to be necessary in World War II.

The approaching war broke in on those developments. In the fall of 1940 the National Defense Research Committee was authorized by President Roosevelt. It soon established the MIT Radiation Laboratory, to develop microwave radar. It was agreed that Condon should devote as much of his group's energy as possible to radar work at East Pittsburg, in cooperation with the MIT Radiation Laboratory. So, during the winter of 1940–41 he commuted weekly between Cambridge and Pittsburg. Westinghouse made him chairman of the company committee to coordinate the expanding microwave research effort at its electronic laboratory in Bloomfield, New Jersey and its radio systems factory in Baltimore, adding to his crowded travel schedule. He also served briefly with R. C. Tolman and C. C. Lauritsen on the NDRC committee responsible for the rocket program that led to the establishment of the Jet Propulsion Laboratory of Caltech.

Parallel to these developments was the work on nuclear fission that grew slowly at first, but by 1942 expanded into the huge complex of the Manhattan District. Condon worked for a while with the S-1 Committee, coordinating the start of this work. Later he spent a little time with Robert Oppenheimer, planning the establishment that was to become Los Alamos. But his duties to the microwave work at Westinghouse prevented his participating further, beyond preparing a text on nuclear physics that became known as the Los Alamos Primer. In 1943 he spent some time at E. O. Lawrence's laboratory at Berkeley, arranging for Westinghouse to build the huge magnets to be used for the electromagnetic separation of uranium isotopes. These multiple contacts with the Manhattan District strengthened Condon's aversion to the military control of scientific research and development. He had many anecdotes, some grim and some humerous, about the military attitude and the consequences of the paranoia for secrecy. For example, he would recall the time he, Oppenheimer, and General Groves were discussing the site of what was to be the Los Alamos Laboratory. Ed inserted the question, "As a western boy, I am wondering how we are going to supply this place with water?" General Groves brusquely said that that was his own problem and that Condon should concern himself with physics. "Yes, General," came back Condon, "but just how are we to get the water?" The fact that Condon's worry was justified and that the problem later had to be solved at enormous expense by trucking water up the mesa probably did not endear either of these strong characters to the other.

The end of the war pushed Condon onto the national stage. He had been elected to the National Academy of Sciences in 1944, and in 1945 he became Vice President of the American Physical Society and became its President the following year. The many physicists who were concerned at the military control of nuclear weapons looked to him for leadership, and his penchant for action set him to writing articles and giving talks about the dangers as well as the potentialities of nuclear power. These came to the attention of Secretary of Commerce Henry Wallace, who persuaded President Truman to appoint Condon Director of the National Bureau of Standards. Condon accepted and, before he was confirmed by the Senate, he came to Washington to work with Leo Szilard and others to lobby for the civilian control of atomic energy. The fight was violent and bitter, and affected the rest of Ed's life.

His appointment to the Bureau was confirmed by the Senate in November 1945, but before that Senator Brien McMahon asked Condon to serve as his scientific advisor for the special committee on atomic energy that McMahon chaired. For several months Condon gave a course for legislators on the atomic nucleus, its implications in war and peace. Until the summer of 1946, when the McMahon–Douglas bill established the Atomic Energy Commission, under civilian control, Condon held two jobs. With the establishment of the AEC he felt able to turn his undivided attention to the Bureau of Standards.

Condon was the first Director of the NBS to be appointed from outside the Bureau ranks, the first Director to be recruited from industry, the first theoretical physicist to head the Bureau and the first and only Director to live in a house on the Bureau grounds. As his colleague at the Bureau, Hugh Odishaw, has written: "The NBS had had a long and honorable history of scientific and technical contributions, but the depression years had seen its budgets slashed. Instruments and facilities were wearing out; there was little if any new gear; no significant opportunities to enter into new areas of research and negligible funds to attract young scientists. Condon was determined to change this."

"The struggle for greatly increased appropriations was limited in success, but Condon drew much larger funds from other agencies. With these he strengthened sound on-going activities and initiated new ones, in mass spectroscopy and betatron studies, for example, and through the creation of new divisions, as in applied mathematics and electronics. These latter two collaborated in a pioneering computer program—SEAC in the East and SWAC in the West. These were the first automatically-sequenced, high-speed digital computers, and much of subsequent computer technology stems from this endeavor." In addition, he and Odishaw assembled the highly useful Handbook of Physics, finally published in 1958.

Condon was also interested in administrative problems. He simplified the Bureau's organization, initiated the first complete restatement (Public Law 81-619) of the Bureau's functions since its founding, and he presided over the establishment of major new facilities at Boulder, Colorado. The

results of his initiation of new programs and recruiting of young blood are still quite apparent at the Bureau, 23 years later.

Condon believed in removing obstacles, not going round them. This chronicler remembers being castigated for commending the formation of not-for-profit corporations as a means of providing technical assistance to government agencies without becoming enmeshed in civil service red-tape. Condon felt this was a cowardly evasion of the Augean task of revising civil service.

Such direct action, of course, makes enemies. He had already roused the ire of the House Unamerican Activities Committee (HUAC) by his opposition to the military control of atomic energy. In 1948 the Committee's chairman, J. Parnell Thomas, proclaimed that "Dr. Condon is one of the weakest links in our atomic security." Privately Condon described the impossibility of refuting such a charge as follows: "If you say I've got a wart on my nose, I can deny it. But if you just say I'm one of the ugliest men in town, all I can do is to argue that I'm really quite pretty." The verbal duels at the hearing reached heights of invective and illogic. Condon once alleged that someone actually asked how it had come about that Dr. Condon had been born so near the site of the first atomic bomb test. Time and again his security clearance status was reviewed and re-established, only to be challenged again, long after Congressman Thomas had been put in jail for taking kickbacks from his staff.

In 1951, the year that a star-chamber hearing had removed the clearance of J. R. Oppenheimer, Condon regretfully decided that the Bureau would fare better if he left. He had expected to stay at the Bureau for much of the rest of his life, devoting his energy and skill to making it one of the greatest scientific laboratories in the world, but it was clear that the continuing attacks on him were hindering further support of the Bureau by Congress. So he accepted an offer to become director of research and development for the Corning Glass Works.

Here again Condon recruited new scientists and initiated new research on the structure of glass and new applications of its properties. He published a highly useful sequence of four papers on the physics of the glassy state in 1954. At the Corning laboratories he initiated a number of new projects. Unfortunately one of them, on missile nose cones, was supported by the Navy, and thus clearance was required.

In September 1952 Condon was again called before the HUAC to answer further charges, one of which (Britten, 1971) was that there was reason to believe he might be disloyal "in that your wife was critical of the foreign policy of the United States and you did not reprove her." After a long delay, during which the strong support of his scientific colleagues was shown by his election to the presidency of the American Association for the Advancement of Science, Condon had his fourth hearing before the clearance review board and again was given a completely favorable verdict. Within four months, however, the Secretary of the Navy demonstrated the irrelevance of the semijudicial clearance hearings by arbitrarily suspending Condon's clearance. A few days later Vice President Nixon implied, in a campaign speech, that he had requested the suspension.

As Condon has written: "The Republicans were still bent on smearing the Truman record by pretending to a concern over the loyalty. The campaign promise in 1952 was that they would 'clean the reds out of Washington'. They kept their campaign promise to the extent that the procedures were revised and a number of persons were subjected to long and tiresome hearings. One of these was J. Robert Oppenheimer, who was finally deprived of his clearance; another was myself, where the outcome was favorable, as it had been in three previous hearings. However this verdict was arbitrarily suspended by the Secretary of the Navy. The Corning organization had proved to be the most satisfactorily, scientifically and humanly, of any with which I have had the good fortune to be associated. But I had been under intermittent harrassment in this way since 1947 and I decided I would subject myself to it no longer. So I arranged to become a consultant to Corning Glass Works (a position he held till his death). In the spring of 1955 I was offered professorships by the faculties of two major universities, but in both cases the trustees refused to confirm the appointments, under pressure from Washington. Finally I was allowed to become chairman of the physics department at Washington University in St. Louis, and later to come to the University of Colorado as professor and fellow of the Joint Institute for Laboratory Astrophysics (joint with the Bureau of Standards, thus formally reestablishing Ed's relationship with the Bureau that had never really been broken). As the cold war slowly died down the Department of Defense finally reinstated my clearance, but this, I am proud to say, I have never used."

The appointment to Washington University was the result of the efforts of Chancellor Arthur Compton, who not only wanted to add an outstanding physicist to the staff, but also realized that the nation as well as Condon would be the loser if the irrational chain of events were allowed to continue. At Colorado he could finally settle down again to research in atomic theory with Halis Odobasi (now at the University at Istanbul) with the intent of rewriting The Theory of Atomic Spectra, and in further work on the properties of glass. He continued to write and lecture on the need for peaceful, worldwide cooperation; he took on the job of Editor of the *Reviews of Modern Physics*; he actively participated in the research of the Joint Institute of Laboratory Astrophysics and he found time to be president of the American Association of Physics Teachers in 1964.

And, in an incautious moment, he agreed to head a project, supported by the Office of Scientific Research of the Air Force, to investigate the many reports of unidentified flying objects (UFO's), with which the Air Force had been plagued for nearly twenty years. This occupied much of his time during 1967 and 1968. The report of this project was published in 1969. Condon gave a light-hearted account of some of his experiences as a talk before the American Philosophical Society. The report has been the subject of vituperative comment from persons anxious to continue to believe that flying saucers are visitors from outer space and who wish to see the government spend vast sums on further studies. Despite the views of many of his colleagues that the investigation was a waste of Condon's time, Seitz has said, "The introductory chapter of the report on UFO's, in which Condon describes with characteristic clarity his own view as a scientist on what constitutes worthwhile research, is a

classic; it deserves to be a landmark in the journey science has taken since the days of Stevin, Galileo and Kepler."[8]

Edward Uhler Condon died on March 26, 1974. Two comments may serve to close this survey of his life. One is by one of his colleagues at the NBS, Churchill Eisenhart: "Edward Condon was a brilliant scientist, with highly original ideas, a wide range of interests, a restless probing mind with voluminous information indexed for instant retrieval. He could meet with scientists of diverse specialities and stimulate each with fresh enthusiasm and new insights. He could elucidate scientific intricacies to non-scientists with clarity in layman's language. Whatever he knew he saw with crystal clarity; he could summarize it in a nutshell on a moment's notice or discuss it in detail with experts, with equal ease. He had an exuberant sense of humor, a gift of repartee and could be wittily caustic when provoked. He was a cordial, genial, straightforward individual; fond of people, mathematics, science, chamber music and conversation; allergic to formality, fuzzymindedness, pomposity and all forms of physical exercise. He was an active Quaker, a firm believer in human dignity, an outspoken liberal and anti-isolationist. He gave freely of his counsel and his time, generously of his finances and his home."

The other comes from Lewis M. Branscomb, a colleague at Boulder, now with IBM: "Watergate came as no surprise to Edward Condon, nor did its aftermath. I imagine he would have liked to see the outcome of the impeachment inquiry. But Condon understood and paid his share of the price of liberty. Somehow his idealism, his sense of humor and his inexhaustible energy made his relentless quest for a better world look like optimism. He was elected president of the AAAS during the height of his troubles with HUAC. He was president of the Society for Social Responsibility in Science (1968–69) and co-chairman of the National Committee for a Sane Nuclear Policy (1970). He was appropriately honored, on his retirement from JILA and the University of Colorado in the summer of 1970, by the volume edited by Brittin and Odobasi. Brittin relates a comment about Condon by E. Bright Wilson: 'Sometimes I think he looks for trouble'; and Condon's answer, 'It's not hard to find'."

Unfortunately it is not easy to find a brilliant scientist who is willing to speak out on questions of public policy, often with humor but always with determination, even in the face of official persecution.

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M.I.T. August 1974.

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Other quotations in this memoir are taken from personal letters to Philip M. Morse.

Courtesy of Photographic Department, University of Colorado.