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GEORGE ELBERT KIMBALL

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A Biographical Memoir by

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GEORGE KIMBALL was a generalist, capable of achieving outstanding recognition in two fields of science and of leaving his mark on other fields of human endeavor. Perhaps his greatest contribution was the education and inspiration he gave to many younger men, now working in various fields of science and technology.

Kimball was born in Chicago, in 1906. None of his immediate family evidenced an interest in science. His father started as an office boy in the Chicago office of a New Britain, Connecticut, cutlery firm and worked his way up to be president of the firm, selling many things beside cutlery. Kimball's mother was a grade-school teacher in Illinois before she married. His only sister had a career in radio and the theater and his only brother was a Rhodes scholar who became a professor of journalism at Columbia. When Kimball was three years old his family moved to New Britain, the home office of the firm his father eventually was to head, thus reversing a family migration. Kimball's grandfather had moved from Salem, Massachusetts, to Chicago early in this century.

Kimball grew up in New Britain, displaying few remembered signs of precocity and no marked preference for science. He took all the Latin his local high school could provide but his chemistry teacher was the one who caught his interest. After a year at Exeter Academy, he went on to Princeton, in 1924, urged there by his father, who felt that there were too many Yale graduates in Connecticut.

George was a fairly typical undergraduate at Princeton—he was on the water-polo team—though not a typical chemistry student of the time. He later claimed he chose the chemistry program because it allowed him to take as much physics and mathematics as chemistry, and he wanted to learn all three subjects. It was a fruitful time at Princeton in those subjects: Hugh S. Taylor was breaking new ground in chemistry; E. U. Condon and H. P. Robertson arrived there in 1927, fresh from Göttingen and filled with the new quantum mechanics; Veblen and Eisenhart were still teaching; E. P. Adams was still giving his gemlike lectures on analytic dynamics; the next wave was about to break. By the time Kimball received his bachelor's degree, in 1928, his interest had centered on quantum chemistry.

His abilities were such that the Chemistry Department offered him one of its best graduate fellowships, so he returned to Princeton, to work under Hugh Taylor, to soak up more physics and mathematics and, though yet a graduate student, to give a private course in quantum mechanics to the faculty of the Chemistry Department. E. Bright Wilson, who was two years behind Kimball, remembers that "I had an enormous respect for his knowledge and his ability to explain things. He seemed to know everything, and I think he really did. It was not at all that he was boastful or a show-off—I used to seek him out for enlightenment, and he always provided it."

His first research in quantum chemistry was forestalled by Henry Eyring, at that time in Berkeley. This did not matter much, for Eyring came on to Princeton the next year, 1931, and the two began a fruitful ten-year collaboration that resulted in their well-known treatise on quantum chemistry. Kimball's thesis for the doctoral degree, granted in 1932, was on the quantum mechanics of the recombination of hydrogen atoms.

Meantime John Slater, new head of the Physics Department at M.I.T., had been making progress in the quantum mechanics of molecular and crystal structure, and Kimball was desirous of extending his knowledge in this direction. He applied for a National Research Fellowship in physics for 1932-1933 but missed out because he was not well known to the physics fraternity. He stayed on at Princeton as instructor and next year applied for a National Research Fellowship in chemistry and won, coming to M.I.T. for the two years 1933-1935. Though he was officially assigned to the Chemistry Department, he spent much of his time working with Slater and others in the newly reconstituted Physics Department at the Institute.

These were heady times. The new faculty was augmented by a galaxy of postdoctoral fellows, and a new generation of graduate students enlivened the scene. Among this aerie of eaglets Kimball more than held his own. In 1965 he wrote a reminiscence of those times for the *International Journal of Quantum Chemistry,* which says, in part:

"The group which inhabited the third floor of the Eastman Laboratory sat at the feet of an academic trinity. John Slater (then 33 years of age) was the Old Man, with a long and illustrious career behind him. Philip Morse was the junior member of the trinity (he and I had been graduate students together at Princeton). The third, and most spiritual member was Julius Stratton, another old man (only a year younger than Slater), who mystified everyone (except Bill Hanson) by being more interested in Maxwell's equations than in the Schroedinger equation.

"These were exciting days, for in spite of Dirac's brave claim that the principles of quantum mechanics as then understood (1928) were sufficient to explain the whole of chemistry and most of physics, it was clear that the demonstration was far from complete. The great interest in Slater's group was in what is now called solid-state physics, and in the attempt to derive the properties of solids from the principles of quantum mechanics.

"The great Depression was at its height (my first job after I earned my Ph.D. paid the magnificent salary of $900 a year). As a result the group of graduate students and postdoctoral fellows with whom I worked lived a sort of Vie de Boheme. The center of this life was the third floor of the Eastman Laboratory, where we shared office space. We spent our evenings as well as our days there, but not always at our work. There was a ping-pong table, and someone discovered that the long, long corridors of M.I.T. made a wonderful place to roller skate.

"Every afternoon we had tea, served by Alice Hunter, student in chemistry, who has since done me the honor of becoming my wife. Those teas became a sort of discussion group, led by Norbert Wiener, who would argue violently on any subject, such as Chinese grammar, or whether or not the number of palindromic primes is infinite.

"From time to time we would have a party. The most famous of these was a theater party at which we all had seats in the second balcony to see a D'Oyly-Carte performance of the Gondoliers. Alice Hunter brought her knitting, including a large ball of bright orange yarn. During an intermission Ralph Johnson, sitting beside her, picked up the yarn and Satan (in the person of Bill Shockley) whispered 'Throw it.' Ralph did, all the way to the orchestra pit. Someone tried to throw it back, but it only got as far as the first balcony. From there it was thrown back and forth until the whole theater was festooned with orange yarn. Finally an ingenious usher broke the yarn and carried the remains of the ball up to the second balcony.

"My office mate was George Shortley. He was putting the finishing touches on Condon and Shortley's *Theory of Atomic Structure*. It is interesting to note that out of this rather small group, three of us, Philip Morse, George Shortley and myself have since served as presidents of the Operations Research Society of America.

"As far as I know John Slater is the only member of this group still working in quantum mechanics. The rest of us were diverted by World War II into other fields. Quantum mechanics is seldom mentioned as a way of training students to be business managers, but more of us seem to have landed in that spot than any other; Ralph Johnson became vice president of Thomson-Ramo-Wooldridge Inc.; Harry Krutter is chief scientist of the Naval Air Development Center; and George Shortley became director of the Washington operations of Booz, Allen Applied Research."

In addition to writing two papers with Shortley on quantum theory and one on the electronic structure of diamond, Kimball lectured on quantum chemistry in the Chemistry Department during his stay at M.I.T., helped organize a graduate course in methods of theoretical physics, and continued work on the text on quantum chemistry he and Eyring had begun. During the summer of 1935 he returned to Princeton, to work with Eyring. After a year spent teaching physics at Hunter College, he entered the Chemistry Department of Columbia University as an assistant professor. With a few intermissions he remained there until 1956, becoming Professor of Chemistry in 1947.

The five years 1936-1941 were productive ones. He published nine papers on reaction rates and electrochemical surface effects, he introduced and taught courses in quantum chemistry, and he supervised graduate student research. Some reminiscences by his colleagues will indicate his continuing interest in the educational process. For example, Professor Louis P. Hammett writes: "George was outstanding for his ability to understand, rearrange, and restate a mathematical development in a way to make it relatively intelligible. I have heard that he was successful in this respect with admirals, I have again and again seen him successful with chemists, including especially myself. He was a highly effective teacher with graduates and advanced undergraduates; I don't remember that he had much experience with elementary students. He was always extremely generous with his learning and his time, perhaps even too generous."

Professor Joseph E. Mayer writes: "During the time I was at Columbia George concerned himself considerably with our examinations, grading, and advising of graduate students. This was something that he felt we were not doing well, as we were not (but then I have never been at a department where I felt it was well done). He was, at the time, on the College Entrance Board Advisory Committee. I remember that he was a strong adherent of the short answer (multiple choice) type of question. This was partly because of necessity on an examination given to a very large population, but he had other arguments for its value; one that the grading could be absolutely quantitative and impersonal, and secondly that by having many questions one reduced the statistical fluctuation occasioned by hitting a student's 'blind spot' in an examination with few questions. He was, however, very aware, as few faculty members seem to be, of the necessity of very careful choice and wording in the questions.

"All this may sound as if George wanted to reduce education to a mechanical system, which was far from the case. He was very interested and concerned with the students and he had a considerable intuitive understanding of them. Above all, he was tolerant! I know he had a very critical appraisal of his friends' and colleagues' intelligence, but he was far too kindly to show obvious disapproval. I always felt he liked people. George and Alice had a home in Leonia, N.J., a few blocks from ours. We played bridge with them and some four other couples quite often. George was probably the best player of the group."

As to the effect of his interest in examinations, one of his students, Dr. Isaac Asimov, writes: "I had a lab course from Kimball in physical chemistry and at one time was asked one question out of a number of possible questions and drew a complete blank. I got a zero. I came to him afterward and said that the question I was asked was the *only* one of the alternatives I couldn't answer perfectly and that a mark of zero was not a true measure of the state of my knowledge. He said, 'The time will come when you will be asked a question, and it will be the *only* one of a number of alternatives which you *can* answer perfectly. You will then get a mark of one hundred and that will not be a true measure of the state of your knowledge either. But you will not complain then, will you?' Very much against my will, I saw the justice of that and subsided. I kept my zero—but I passed the course."

In 1942, when I was asked by the Navy to organize a group to analyze antisubmarine tactics, Kimball was one of the first persons I recruited. I remembered his breadth of interest, his analytic ability, and the clarity of his exposition, all of them important for the task ahead. He joined the group within a month. Almost immediately he showed his worth. Together we worked out the basis of the theory of search, and then wrote it up, all in less than two months. The writing was, if anything, more difficult than the theory, for we were addressing naval officers, who were to use the results to work out search plans and convoy escort patterns. Kimball toured the naval bases along the east coast, explaining the ideas, working out applications, and learning the practical difficulties.

Within the year he became Deputy Director of the group, called the Operations Research Group (ORG) during the war, later called the Operations Evaluation Group, U.S.N. It grew to number seventy-odd analysts by 1945. Kimball's abilities were in daily use as an educator, as a universal scientific encyclopedia, and as a deviser of simple algorithms to solve tough problems quickly. His colleague then and later, Arthur A. Brown, comments: "In the ORG the initial work dealt with search and with the optimum geometirc patterns for the depth-charge bombing of German U-boats. In a very short space of time the group was working on tactical patterns for destroyer attacks, on the question of reliability of aircraft sightings, and the related question of whether or not to send out a destroyer force.

"This brought us into the question of convoy protection and convoy size, and into liaison with the Coastal Command of the R.A.F. The work spread into the South Atlantic and into the Pacific, in relation to our own submarine offensive against the Japanese supply lines; to our combat air patrols against attacks on the Third and Fifth Fleet operations; and to defensive tactics against Kamikaze attacks. By the end of the war the group had a network of field operations and a solid place in Washington's strategic councils. George Kimball was in the midst of all this and he contributed largely to it.

"Many of his contributions were simple and also ingenious. In 1943, before the present digital computers had been developed, he set up and ran a Monte-Carlo experiment on an IBM sorter. What he did was to draw the silhouette of a submarine on a punch card, punch out the inside of the submarine, take another card and move the silhouette according to a random draw from a two-dimensional normal distribution, do it again, and so on, until he had a respectable deck of randomized submarines. Then, using the sorter, he dropped a number of different depth-charge patterns onto the cloud of submarines and counted the hits. He was apologetic whenever he had to talk about it, saying that anyone would have thought of it. Nevertheless *he* was the one who did think of it, and do it, and it resulted in valuable conclusions when they were needed."

The day after Hiroshima, I, as Director of ORG, obtained one of the first copies of the "Smythe Report" to arrive in Washington. Within twenty-four hours my Deputy, Kimball, and I briefed Admiral King and his staff on the naval implications of the A-bomb; a day later we briefed Secretary of the Navy Forrestal and the joint Senate-House Naval Affairs Committee.

In the midst of all this, Kimball's work with Eyring and John Walter, started ten years earlier, was completed and the book *Quantum Chemistry* was published in 1944. At the end of the war some of the Operations Research Group decided to delay returning to their peacetime positions long enough to record what had been learned. Kimball and I wrote the volume *Methods of Operations Research;* Bernard Koopman, who had joined the group in 1943 wrote *Search and Screening;* and Charles Sternhell and Alan Thorndike wrote a technical history of *Anti-Submarine Warfare in World War II.* All three volumes were initially classified. It was not till 1951 that the first volume was declassified and turned over to a commercial publisher; the other two volumes were never declassified. The *Methods* still is used as an introduction to the subject, and is still referred to in the literature.

After this, Kimball returned to the Chemistry Department at Columbia, to resume his research and teaching in theoretical chemistry. That he was successful is evidenced by the dozen papers he published on chemical kinetics and on other subjects in chemical physics. Honors began to come his way. He received the Presidential Citation of Merit for his war work; he was elected to the National Academy of Sciences in 1954. He also retained his interest in the new field he had helped pioneer during the war, operations research. He continued his contacts with the Navy, acting as consultant with the Operations Evaluation Group and serving on various advisory panels on underwater ordnance. When the Weapons Systems Evaluation Group (WSEG) was formed in 1949, to carry out operations analysis for the Joint Chiefs of Staff and the Secretary of Defense, he contributed to its work, for a time as consultant and then for a while as a full-time member of the group. He also assisted in organizing the NATO Advisory Panel on Operations Research.

Even during the war Kimball had become convinced that operations research could be effectively applied in industry and in the public sector. He was interested in enlarging public awareness of its potentialities and was active in organizing the Operations Research Society of America, which was founded in 1952, with Kimball as a member of the society's first council. By 1964, when he was elected the society's president, the society had about 5000 members.

In the 1950s Kimball began to spend some time with the operations research division of Arthur D. Little, Inc., assisting in its consulting work for industry and for the Navy. This work increasingly engrossed his attention until, in 1956, he left Columbia and came full time to A. D. Little, first as Science Advisor and then, in 1961, as Vice President. When asked, later, whether he missed teaching, he replied that he was still teaching and that it was a greater challenge to teach people who didn't want to learn or didn't know they were learning.

Much of his work with A. D. Little dealt with applications of theory to the specific problems of the client. Most of this has of course not been published. A partial list of his internal reports and notes indicates that he initiated developments in dynamic programming, decision theory, inventory, and reliability theory, which others fed into the open literature later. Kimball was never particularly interested in publication. He would spend a great deal of time solving specific problems of immediate importance, or in making clear the underlying theory to clients or to classes, but to establish priority by publication, with all its drudgery of typescript, galley and page proof, had less attraction for him than some new problem. He always maintained that there was too much publication anyway.

Arthur Brown reports Kimball's comment when Brown said that someone should have pointed out that the theory of search anticipated the basic principles of information theory; that the probability of target detection is just the entropy of the target distribution. Kimball remarked that this was of course true, but everyone had known about entropy for decades. In his view all he had done was to apply known theory in a context which needed theoretical clarification.

Kimball also did his part as citizen and parent. John B. Lathrop, a neighbor and colleague at A. D. Little, reports: "George had a strong sense of responsibility to the community and gave it as much time as he could. He spent many years as officer, committeeman, or consultant for church, Boy Scouts, and community. An example is his study of the growth of the school population of his home town of Winchester, Mass., done for the local School Committee. His classification of the people of Winchester as 'old families,' 'new families in old houses,' and 'new families in new houses' and his tracing of the different patterns of change and incidence of school-age children in these groups was a model of useful statistical analysis, forecasting, and clear understanding of the phenomenon he studied. He concluded the report with a basis for decision—the earliest, expected, and latest dates when various school additions would be overcrowded.

"George had strong interests—and really was expert—in many fields; languages, naval history, bridge, music, cooking. For years he made almost all the family's gravy, and taught his children how (he was convinced it took a chemist to make a good gravy). There was a blackboard in the family kitchen and frequently he would sit there over a cocktail while dinner was under way, discussing calculus or chemistry or physics with one or another of the children. Three of the four children have definite scientific leanings. Prudence, the oldest, has a Ph.D. in chemistry; Thomas will probably pursue applied mathematics when he gets out of the Air Force; Martha, the youngest, is a chemistry major; Susanna, alone, did not have a scientific bent."

For his last several years, Kimball suffered from serious cardiac illness. For the final year, at least, he was in constant pain. Those who saw him daily knew that this was so, but none were made aware of it by his manner, his actions, or his words. He continued to work actively on all projects and in all the fields that interested him. His death, in fact, came in the midst of his duties; when he died, on December 6, 1967, he was in Pittsburgh as a member of the Visiting Committee of the Carnegie-Mellon University's Chemistry Department. At the time of his death he was chairman of the Northeastern Section of the American Chemical Society.

Kimball was a generalist—which doesn't seem to rate the acclaim the specialist gets nowadays—but his value to operations research, indeed to science, lay in his universal interests.

He could bring concepts from chemistry to bear on inventory and marketing problems; he could devise an abstruse mathematical algorithm to make a digital computer produce random numbers as fast as was needed. Everything he did had to be done well; if he couldn't do it well he didn't do it. In fact, his uncompromising standards kept him from publishing much good work, because it wasn't in final, polished form. Many of us wished that more of his lectures could have reached a wider audience, but that was not his way. He preferred to work directly with people, not via the printed word. And this was in line with his concentration on immediate problems, rather than on abstract theory.

Many of us would agree with Joseph Mayer's comment: "George and Alice were delightful friends to have and we enjoyed them immensely. I have always thought that George was one of the most pleasant companions of an evening, with whiskey and soda after a good dinner. He was not particularly a person who sparkled; he was just comfortably tolerant and *very* intelligent and informative."

Kimball's style of work was rooted in his personality. It was characterized by simplicity of thought and method. Another characteristic was theoretical power and depth. A third was a permanent adherence to reality. He never liked the spinning of elaborate webs of mathematics and he never liked to be too far from actual data. He was sensitive to problems of wording, emphasis, and timing in the presentation of research results, but he was wholly uncompromising in matters of principle. He set an example worth following.

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KEY TO ABBREVIATIONS

J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Chem. Phys. = Journal of Chemical Physics
J. Colloid Sci. = Journal of Colloid Science

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