
In Memoriam: Bernard Osgood Koopman, 1900-1981

Author(s): Philip M. Morse

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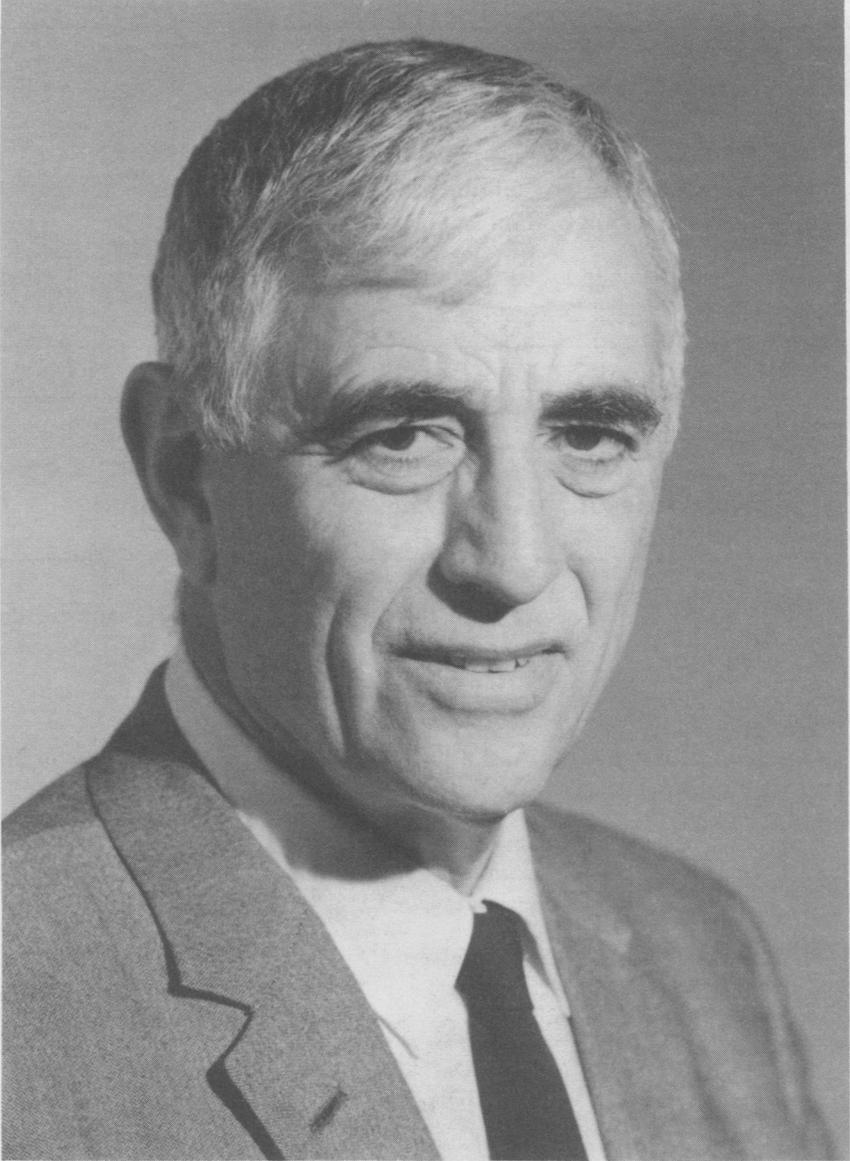
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Bernard Osgood Koopman

Bernard Osgood Koopman, 1900–1981

PHILIP M. MORSE

Professor Emeritus, Massachusetts Institute of Technology, Cambridge, Massachusetts

THE LIFE of Bernard Koopman covered a wide span, both geographically and intellectually. He spent his youth in France and Italy but he came back to Harvard for his college education, and his last days were spent in his beloved Randolph, New Hampshire, in the shadow of Mounts Adams and Madison. His interests were likewise spacious. He could dogmatize on the merits of Pommard versus Fleurie or reminisce, in his characteristic Cambridge (Mass.) accent, about climbs in the Alps, the Tetons or the White Mountains. During his postdoctoral fellowship in Paris he listened to the lectures of Borel, Lebesgue and Hadamard, but his lifelong interests were in the less “pure” aspects of mathematics, Hamiltonian dynamics and the bases of probability; and his later years were spent enriching the field of operations research. He was a stimulating companion. Once one pierced the crust of rough frankness, one found a supportive and permanent friend.

In spite of his European childhood, his New England roots were deep. His mother, Louise Osgood, was the first cousin of Harvard mathematician William F. Osgood. She met his father, Augustus Koopman, during an art class in Holland, she a student and he the instructor. They were married (in Paris) within the year. He had come from Philadelphia and studied at the Beaux Arts in Paris. After their two children were born, the Koopmans went to the United States for a year but then came back to stay in rural France and Italy, so Augustus could paint (he specialized in rural and seaside scenes, though he was also a good portrait painter). As Bernard’s sister, Ellen, says, “It was fortunate that we had each other, as we spent years seeing few other children, living as we did in the country. Fortunately we were fond of imaginative games, inspired by the books our mother read to us. She taught us to read too.” The first book Bernard read was an Oz book.

By the time Bernard was eight the family settled in Paris for the winters, so the children could get a more regular education. The years at the Lycee Montaigne were not happy for him but he survived—and learned. Meantime the family was breaking up. In 1912, divorce, serious illness for Bernard, and the start of a two-year decline in his father’s health must have been traumatic for the children. Augustus Koopman died early in 1914. Louise and the children became paying guests at an estate near Bordeaux. There they stayed during the first winter of the

German invasion. By spring it was obvious they had to come back to New England. They sailed from Bordeaux in May 1915 and by that winter Ellen and Bernard were students at the Cambridge Latin School.

By 1918 the United States was at war. Bernard worked at the Hingham Arsenal that summer; entered Harvard on a scholarship that fall (in the Student's Army Training Corps). By that time he had decided to become a scientist. He studied electrodynamics with Chaffee, thermodynamics with Bridgman, math and more math with Birkhoff and others. During the summers Bernard, his mother and sister took to staying in Randolph, next door to the Bridgman's summer place; he came to love the place—and the mountains. In 1922 he graduated, *summa cum laude*, highest honors in mathematics.

A fellow student, Marshall Stone, also a *summa cum laude* in mathematics in 1922, describes their next several years as graduate students.

Bernard introduced me to his mother and sister, with whom he was living in Cambridge. We became very close friends, studying together and enjoying various social activities. Koopman had a special interest in dynamics and mathematical physics. His dissertation was written under George D. Birkhoff and was devoted to dynamics. My interests did not include classical dynamics, but in the fields of differential equations and Fourier analyses we were both interested and took some courses together.

In 1924 we were both awarded Sheldon Traveling Fellowships, designed to give Harvard postdoctoral students a year abroad with emphasis on general culture rather than professional improvement. Neither of us had received our doctoral degrees, but our theses were well advanced, enough to qualify for recommendation by the Mathematics Department. The two of us decided to spend our year in Paris at the Sorbonne, with generous time off for travel. This decision led us to take rooms with Mrs. Koopman in a small hotel near the Luxemburg Garden, and travel together elsewhere now and then.

In July 1924 we took a French Line steamer from New York and went direct to Göttingen to join our mutual friend, Brinckman, for some travel in South Germany. Koopman then left to meet his mother in Paris and I went for a month to Dijon. That fall Bernard and I explored the Sorbonne together and we gradually settled down to following a few courses. We both had an interest in probability so we listened to Borel's lectures for a while. One "must" for mathematicians in Paris in those days was regular attendance at Hadamard's seminar at the College de France. Bernard and I were usually present. During the spring and early summer I left Paris for some travel; Bernard, I believe, went to Switzerland to climb mountains. We returned together to New York, in time to start teaching in the autumn of 1925, he with a Benjamin Pierce Instructorship at Harvard and I at Columbia. A couple of years later our positions were exchanged; I went to Harvard and Bernard began a lifelong career at Columbia.

Koopman was offered an instructorship at Columbia early in 1926, shortly after he had been granted his Ph.D. But he also was awarded a

National Research Fellowship, so he persuaded Columbia to wait for a year, while he went to Princeton for the winter. He returned to Columbia in the fall of 1927 and stayed on, eventually becoming head of the Mathematics Department. Of course his finally having a permanent base did not stop his peregrinations. Every summer he was off somewhere: California, Rome, the Alps, the Tetons—and always a week or month at Randolph. Even during term time he would travel: to Princeton, particularly after John von Neumann arrived there; and back to Harvard, to talk things over with Birkhoff. A student and later colleague, Edgar Lorch, can next take up the story.

The authorities at Columbia had tried for some time to rebuild its Mathematics Department. They eventually decided to seek the most brilliant young men available, and eventually appointed Bernard Koopman and Paul Smith. It was indeed a felicitous choice. The two men were of quite different character, though they became close friends. Smith was aloof, quiet and strictly interested in his own pursuits. Koopman was open, a social gadfly, always ready to pull people's legs, much in evidence at the lunch table, where he would expose his wit with clarity, zest and often pungency.

In the early thirties the young mathematicians of Koopman's age lived in Furnald Hall, one of the university dormitories. Later this arrangement broke up and Koopman and Smith took an apartment together. This situation lasted for a time, until one day Smith said, "Oh, by the way, Bernard, I am getting married and am terribly sorry but you will have to leave the apartment because Suzanne is going to move in." Bernard took it in his stride and proposed to me that we should set up quarters together. I promptly agreed and the two of us lived together for a year. One day in the spring Bernard, a little in the manner of Smith, told me, "Ray, old boy, I don't know whether I told you I was getting married to Mary Louise Harvey. I am terribly sorry, but you will have to leave." We stayed good friends.

Almost single-handedly, Bernard rejuvenated and reformed the undergraduate mathematics program. He introduced courses in probability, finite differences, and elementary function theory for the upper undergraduate years. His teaching of these courses was very successful and his notes were most useful to other teachers. Genuinely usable texts in these subjects did not exist at the time.

All this while he was in close contact with John von Neumann and with G. D. Birkhoff. In his open way he discussed freely, during his visits, what was going on elsewhere. This put him in the middle in the controversy over the ergodic theorem.

Questions of ergodicity had been in the foreground for many years and had attracted the attention of powerful mathematicians. Koopman was well versed in this domain and had discussed it with both Birkhoff and von Neumann. In March of 1931, Koopman published a note in the National Academy Proceedings, transforming the problem into one dealing with one parameter unitary groups in Hilbert space. Since these groups may be represented by self-adjoint transformations and since they were known to have a particularly decent structure, the door was open to rapid extension. Koopman communicated his ideas to von

Neumann, who, in a short time, gave a proof of the ergodic theorem in a Hilbert space sense, establishing convergence in the mean but not actual convergence. In a state of considerable excitement Koopman told von Neumann's result to Birkhoff, who worked feverishly and succeeded in proving the theorem, establishing point-wise convergence almost everywhere.

Birkhoff's notes were published in the late 1931 Proceedings of the Academy. Von Neumann's results, which had been obtained earlier, were published in the early 1932 Proceedings, seemingly a year later. Koopman, who had been the catalytic agent in the process, felt quite embarrassed. However the problem was clarified by the publication of three notes; one by Birkhoff and Koopman, another by Koopman and von Neumann and a third by von Neumann alone, setting the work in its proper order. All gave priority of place to Koopman's original result.

In 1940 Koopman moved to Chappaqua, New York, with his wife and newly born daughter, Philippa. He was promoted to Associate Professor that year. His long-term interest in probability gave rise to an important paper, "The Bases of Probability," in the October 1940 *Bulletin of the American Mathematical Society*.

By 1941, as Hitler's Nazism swept over Europe, many American scientists turned to problems of national defense. Koopman spent part of his time with a National Defense Research Committee (NDRC) project at Columbia. The first time I met Koopman was in the latter part of 1943. By that time I was director of an NDRC-supported Operations Research Group (ORG, after the war called OEG, and, later, CNA) of about 40 scientists, assigned to the Navy, the only civilian group that was part of the staff of Admiral Ernest King, Chief of Naval Operations and Commander in Chief, U.S. Fleet. The Group had started in 1942 (we were called ASWORG then), advising on tactics in antisubmarine warfare. As we demonstrated our usefulness, we were asked to advise on other aspects of naval warfare. Our headquarters by that time was in the old Navy Building between the Washington Monument and the Lincoln Memorial, but nearly a third of the Group were at various Navy bases, from London and North Africa to Hawaii and Australia, assisting the operational naval forces.

One of the first tasks the infant ASWORG tackled was the problem of search, by ship or plane, of submarines and warships. In fact the first memorandum the Group published, in May 1942, was on Search. The recommendations, when followed, perceptibly improved the tactics of convoy protection and the search for the U-boat. Following its precepts, a barrier patrol was set up across the Atlantic from Brazil to Africa that captured four out of five ships carrying rubber from Japan to Germany. (The fifth ship escaped because it got through before the barrier patrol was activated.) But the "theory" was elementary and, while the results were highly useful, someone was needed who could be protected from the daily, urgent interruptions, and who had the interest and skills to give it mathematical consistency and logical foundation.

George Kimball, a Columbia theoretical chemist and my first lieutenant, had utilized simulation (on the rudimentary IBM data processing equipment we had introduced into the Navy) to improve search procedures, and Edward Lamar, an MIT physicist, had quantified the relationship between the physical properties of airborne radar and the effective search width of the plane as a search vehicle. By 1943 it was obvious that someone was needed to pull the whole subject together.

About that time I came across a note put out by the Columbia project, signed by Koopman, discussing a variant of Lanchester's law. Perhaps it was brought to my attention by Kimball, who had known Koopman at Columbia. I was then having a hard time recruiting a few more good men, in competition with the high-powered drives of the MIT radar laboratory, Johns Hopkins proximity fuse lab, Caltech's rocket project, and the mysterious, ubiquitous thing called the Manhattan District. So I wrote a rather petulant letter to Koopman, telling him he ought to quit theorizing at a distance and come down to Washington to work on *real* problems. To my surprise and delight he turned up the next week and, as soon as he could get cleared and obtain leave from Columbia, he came to work. Soon he moved from Chappaqua to Washington with, by that time, two daughters (the younger named Winifred) and an ailing wife.

For awhile he worked with Jim Dobbie on various problems of immediate urgency, some connected with search theory, such as the analysis of the threat of the expected German acoustic torpedo. Shortly, on the suggestion of Kimball, Koopman, Dobbie and a few others formed the Mathematics Section and set to work to consolidate and expand a number of working theories, with search theory high in priority. The work of the Section was protected as far as possible from distractions of daily "fire alarms," which of course were the ways we gained friends and continued to prove our worth. We had a basic agreement that up to a quarter of our work could be self-initiated, but only within the Group was it realized that long-range study was urgently needed to back up and extend the range of our expertise.

Shortly after VJ day many of the Group left, except for the shrunken core of the OEG, which the Navy had agreed to continue to support (through a contract with MIT) and for a few of us who stayed on to consolidate our experience into a series of final reports. Kimball and I wrote a general survey of the work, which we called "Methods of Operations Research," which was in a form which, we hoped, could soon be declassified. The other two volumes, one on the history of ASW in WW II and the third, by Koopman with the help of Dobbie and Lamar, on Search and Screening, we feared would take longer to declassify.

It took three years to persuade the Navy to declassify "Methods." By 1955 Koopman decided not to wait any longer, but to write the theoretical parts of Search and Screening in an easily declassifiable form. These were

published as three papers in the fourth and fifth volumes of the *Journal of the newly formed Operations Research Society of America*. In 1980 they were embodied, with much newer material, in the book *Search and Screening* published by Pergamon Press. While part of this material was a systematized treatment of the work of many ORG members, some parts, notably the section on the optimum allocation of search effort and the clarification of the probabilistic aspects of search theory were Koopman's own. His prewar interest in the bases of probability made him the natural originator of these seminal contributions.

Meanwhile Koopman had returned to Columbia as full professor and to his home in Chappaqua. His first wife died in 1946. He remarried, in 1948, to Jane Bridgman, daughter of his Harvard professor and neighbor for many summers at Randolph. They had three daughters; Anne, born 1950; Elizabeth, born 1951 and Barbara, born 1953. I used to spend pleasant days with all of them, climbing in the White Mountains. Bernard was head of the Columbia Mathematics Department from 1951 to 1956 and was appointed to the prestigious Adrain Professorship in 1955.

Koopman did not give up his interest in military operations research. He continued as consultant to OEG for several years. I came in close contact with Koopman again soon after I became the organizer and first director of research of the Weapons Systems Evaluation Group (WSEG) of the U.S. Joint Chiefs of Staff in 1949.* During the next five or six years Bernard would occasionally spend some time with the WSEG. He of course was active in the move to organize the Operations Research Society of America, was present at the founding meeting in 1952 and was elected president of the Society in 1956–1957.

In the meantime both OEG and WSEG underwent administrative changes. The nonprofit Institute for Defense Analyses (IDA) was organized, in part to administer the civilian portion of WSEG (then christened WSED) and OEG became CNA (Center for Naval Analyses). From 1956 to 1957 Koopman took leave from Columbia to work full time for WSED, heading a group that studied the then-time capabilities of ICBMs versus manned bombers. The final report was made in person to President Eisenhower.

Koopman's European experience could not be left out, of course. He attended and contributed a paper at the International Conference on OR, held at Oxford in 1957, from which arose the International Federation of OR Societies (IFORS). He continued to attend their triennial conferences until the latest one.

In 1959–1961 Koopman again took temporary leave from Columbia, this time to go to London as OR liaison between the U.S. Department of

* For further details of this and later episodes in this story, see Chapters 8, 9 and 10 of my book *In at the Beginnings* published by MIT Press in 1977.

Defense, U.K. military establishments and NATO. While there he was of immense help to me in the delicate task of introducing OR as a permanent activity of NATO. The first move had already been made by the Science Advisor to NATO (then Norman Ramsey, a good friend of both Bernard and myself), by sending 40 persons from NATO countries to a special summer course in OR held at MIT in the summer of 1958. The second move was to arrange to send to Europe, in the summer of 1959, a team of OR experts to lecture to a larger group of representatives from NATO countries and then to encourage OR by visiting each NATO country. Financial support was provided (through a contract with MIT) by the U.S. DOD and by NATO. The six experts were Kimball, Koopman and myself, plus G. E. Wadsworth, H. P. Galliher and R. A. Howard from the MIT faculty. A special text was prepared and printed (just in time) and the lectures were held at the Training Center for Experimental Aerodynamics (TCEA) near Brussels. After the 2-week course, which was attended by 132 persons (half of them military officers, and a quarter each from industry and the universities of the member countries) the team of six split in two to visit the member countries.

After this exercise, all of us except Koopman returned to the United States. He remained at the liaison office in London (his family moved across to be with him). His presence in Europe made it much easier to arrange the next stage in the introduction, which was to be a formal request from the Science Advisor (Frederick Seitz, also a mutual friend) to Morse, to attend a meeting of the NATO Science Committee, to propose that it set up an Advisory Panel on OR (APOR), which would supervise OR activities in NATO, organize conferences on the subject and administer a program of NATO Fellowships in OR. With Koopman's active help this took place in January 1960; APOR became official. By the summer, members from France, Germany, Italy, Norway, United Kingdom, and the United States were appointed and the first meeting took place in Freiburg, Germany, in August, just prior to the second IFORS Conference in Aix en Provence. Thereafter, for a number of years, APOR met twice a year. I was chairman until 1964; Koopman was the U.S. member from 1966 to 1970 (in the intermediate year 1964-1965 he was director of WSED in Washington). With his encouragement, the APOR conference program was substantially expanded. Several of Bernard's later publications are the outcome of papers he delivered at some of these conferences.

As retirement from Columbia came closer, he began to plan his subsequent activities; it would not be Bernard's nature to relax thereafter. He needed real intellectual challenges during most of the year, but enough free time to travel widely and to recharge his spirits by a stay at Randolph. His colleague, Gordon Raisbeck, can explain how this was accomplished.

Bernard first started to do some consulting at Arthur D. Little in the late 60s, while George Kimball was alive. I am not sure where it began, but I know that by 1967 Bernard was involved to some extent in a project to do a quantitative prediction of the effectiveness of a large ocean submarine surveillance system in the Pacific. The object of the study was to recommend to the Navy a set of locations of listening arrays that would maximize system effectiveness. The work was well underway but far from complete. George died unexpectedly. The task was handed to me complete, and I learned of Bernard's involvement when I went through George's papers. I telephoned to Bernard, explained the situation and asked him to give as much time as he was able. He set aside a day a week and worked closely with me until we got the job put to bed.

During those months Bernard told me he expected to retire from Columbia in June 1968, and that he had been looking forward to continued consulting with Kimball after that time. I jumped at the opportunity to form an alliance with him and he joined the professional staff of A.D. Little, Inc., on September 2, 1968. Technically, because of the structure of our retirement trust and the labor laws, he joined us as a part-time employee but in fact for a number of years he worked full time except for a slightly longer than average summer vacation. In October 1974, he changed his status from employee to consultant, by which time he really was working part-time. This enabled him to take consulting assignments with government agencies and other companies without conflict. His consulting arrangement with A.D. Little continued until his death.

Bernard worked on a number of projects for the Navy besides the ocean area surveillance. When he arrived here he was already into a Navy force level study and he continued to work on that after he joined us. He also worked on a study of weapons systems for a new generation of submarines; on the feasibility of surface effects vehicles in antisubmarine warfare; on problems of search when false contacts are generated by real objects; on the suitability of hydrofoils for Navy combat vehicles; and on the potential of lighter-than-air ships for antisubmarine warfare.

From 1969 to 1972 he worked on a project with John Everett, myself, and several others, having to do with air traffic control system capacity, using computer simulation in a new way. Starting in 1969 he did some very interesting work on acoustic propagation in inhomogeneous media, the subject of a long paper which I hope we can get published soon.

From his retirement from Columbia in 1968 until 1978 Koopman lived in Lincoln, Massachusetts, after that he and his family lived year-round in Randolph. In 1979 he was given the Wanner Award of the Military OR Society (MORS). In 1980 he received the Kimball Medal from ORSA—in his acceptance speech he delivered a flowery speech of thanks to the person who was most responsible for his efforts in operations research—Adolph Hitler.

A few reminiscences deserve to be included here. These are from Dave Katcher, who worked with Bernard in WSED.

Bernard wrote his drafts in pencil with an extraordinary resort to erasing, so

that each page, by the time it was filled, looked like a patchwork quilt. Having learned each afternoon the steps in logic as it was developing, my job was to defend, next morning, what had been done the day before against a vigorous assault from Bernard's overnight doubts. He would then begin the process of erasing and reconstructing all over again. But the paper grew; out of this lapidary process there slowly was laid down a lucid statement of a complicated subject.

This quality of the pioneer shines of course in his scientific work—it extended broadly in the enjoyment of other human pursuits, from the English novel to baroque music. It carried over even to his skill in working with his hands. Bernard loved to fashion in wood and stone, to enlarge houses, cut trees, make gardens and stack wood. I have helped clamp furniture he made and helped hoist and move large stones, while listening to discourses on measurement, geometry, mechanical advantage, mountain climbing and the foolishness of belittling the scientific excitement in applied mathematics. When the conversation was completed and the job was done, the pieces fit as they were intended to and the whole served the function for which it was meant. It was particularly revealing to see him treat children with gentleness, a sense of mischief and with imagination. He granted the young one not only identity but full potential. Fortunate though I was to have him for a friend, I would then regret I had not had him for a teacher.

Bernard died as he lived, busy and full of plans for the future. With practically no warning, his heart just gave out. It is a way I, for one, would wish to die. . . .

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