NATIONAL ACADEMY OF SCIENCES

EDWIN C. KEMBLE

1889—1984

A Biographical Memoir by

ALEXI ASSMUS

Biographical Memoir

National Academies Press
washington d.c.

Photo by Bachrach

Edwin C. Kemble

EDWIN C. KEMBLE

January 28, 1889–March 12, 1984

BY ALEXI ASSMUS

UNUSUAL AMONG PHYSICISTS but in consonance with his religious views, Edwin Crawford Kemble approached his career with humility. He spoke of his own research on molecular quantum physics depreciatingly, was reticent in accepting its importance for the growth of the American quantum physics community, and made little of his lifelong devotion to teaching. Perhaps we can regard his career more dispassionately, neither with embarrassment nor with a memoirist's false grandiosity.

Edwin C. Kemble began his college career at Ohio-Wesleyan University in 1906, but stayed there only a year before transferring to the Case School of Applied Science from which he received his B.S. in physics in 1911. He began graduate school at Harvard University in 1913 and completed his Ph.D. in physics in 1917. After a short time doing war work and a half semester teaching physics at Williams College, Kemble returned to Harvard in 1919 as an assistant professor in the physics department. He remained there the rest of his career, and was made chairman of the department in 1940. He spent a Guggenheim fellowship year in Europe in 1927-28. In 1925 Kemble married Harriet Mary Tindle. The couple had two children, Robert and Jean. Two years before their fiftieth wedding anniversary, Harriet died. In 1978 Kemble married Martha Chadbourne Kettelle, his Radcliffe fiancée from graduate student days.

As a graduate student Kemble made an exciting and courageous move into quantum theory and in 1919 Percy Bridgman, his thesis advisor, convinced him to accept the job of building up theoretical research in the Harvard physics department. Not only did Kemble introduce a theoretical sophistication at the university, but he also focused attention on quantum physics, a subject that generally had been ignored, both at Harvard and in the United States as a whole. In his first decade at Harvard, Kemble played a crucial role in the creation of a national research program in the application of quantum concepts to molecular structure and dynamics. In this endeavor, Kemble worked closely with young colleagues and graduate students. In later years he would turn his attention to college undergraduate and high school education.

The orientation towards community that was evident in Kemble's career reflected his upbringing in the home of Duston and Margaret (Day) Kemble, former Methodist missionaries. Kemble was born in 1889 in Delaware, Ohio. Like many of his colleagues, he was raised in a midwestern religious household that maintained an admiration for science, rather than an antagonism towards it. In fact, he described his minister father as to "some degree, an inventor."[1] He began his college studies at Ohio-Wesleyan in preparation for missionary duties (1906-1907), but between his brother's urgings and his own inclinations he decided to transfer to the Case School of Applied Science and to follow in the footsteps of his engineer brother. After a summer spent working in his brother's business, the Case Machine Company, which had produced one of Minister Kemble's inventions, Kemble changed his mind once again and began a scientific career. This choice was not in conflict with his family's or with his own religious views. For Kemble, as for many other physicists of his generation, religion and science did mix. Religion brought to science a dedication to include others in a community that believed in a higher truth.

Case, the site of Kemble's first scientific education, was founded in 1880 as an engineering school in industrial Cleveland. By the time Kemble attended Case it had developed strengths in science. Dayton C. Miller, a nationally recognized scientist worked there in the physics of acoustics, but because many students at Case wanted to be physicists, Miller had only one or two students a year. Kemble was one of the few. While working on his undergraduate thesis project with Miller, Kemble burst into a week of productive, frenzied work, which, he told historian Thomas Kuhn fifty years later, "left one with a vivid sense of the way . . . mental activity propagates itself."[2]

Kemble graduated from Case in 1911 and spent the following year as a physics instructor at the Carnegie Institute of Technology in Pittsburgh, a school founded, as was Case, in response to the growing demand for higher education for technologists. During that year, Miller obtained a graduate fellowship for Kemble at Harvard—a fellowship personally financed by Harvard Professor Wallace Sabine, a colleague of Miller's in acoustics. In 1913 Kemble came to Harvard as a graduate student.

At the time the physics department at Harvard was hospitable neither to the new quantum physics making its appearance on the Continent nor to a practice of physics that included theorists as well as experimentalists. (Theoretical physics had made its appearance in Europe thirty years prior.) It was not that Americans completely ignored quantum physics. Planck's blackbody radiation law was well known and an

American, Robert A. Millikan, was the one to put Einstein's photoelectric equation to an experimental test. (He expected to prove it wrong!) Physicists in the United States were primarily interested in experimental matters and had not confronted critically the quantum theory as a whole. Kemble was formally introduced to the new theory in G. W. Pierce's course on radiation, but the professor had much to say against it. Kemble, on the other hand, was drawn to the new ideas. "Everything with a quantum in it, with 'h' in it, was exciting."[3] His early enthusiasm took the form of two graduate "theses," so-called papers required for graduate courses at Harvard. They were on an area of physics where quantum ideas were coming into conflict with older principles. The theses were on the problem of specific heats of solids and on the statement of the equipartition theorem. While considering dissertation topics, Kemble jumped at the ideas introduced in a talk by fellow student James B. Brinsmade on the recently introduced quantum theory of molecular spectra.

In the usual accounts of the history of physics little has been said about the unraveling of molecular structure, a feat accomplished by the study of molecular spectra. The focus had been on atomic structure, because it was in this area that the most interesting and foundational questions of quantum theory were addressed in the period 1916-25. During this time Niels Bohr became a central figure in the development of the new theory. Historians of modern physics have emphasized his work, especially his papers of 1913, which predicted accurately the spectra of atomic hydrogen. Yet, in 1913 and for several years after, Bohr's work was not part of the mainstream effort to develop a quantum theory. In fact, atomic structure and atomic spectra were hardly considered in the years between 1900 and 1916; instead, the focus was on the quantum behavior of collective systems (blackbody radiation and specific heats). The particular mechanical systems that were quantized—the oscillator and the rotator—were basic to molecular structure.

At the Solvay conference in 1911 the question of how to quantize the rotator was discussed thoroughly. In the laboratory of the organizer of the conference, Walther Nernst, work was being done on predicting the spectra of molecular gases, particularly HCl. A young Danish researcher, Niels Bjerrum, took the model of a "quantized" rotator and used it to predict accurately what is now called the vibrational-rotational spectra of molecules. Bjerrum made the analysis independently of and slightly prior to Bohr's application of quantized motion to atomic spectra.

It was to Bjerrum's theory of molecular spectra that Kemble turned as a graduate student. Kemble, so interested in "everything with a quantum in it" had found a problem. He wrote in his first paper: "The explanation of the structure of infrared bands of gases given by Bjerrum has led to striking direct confirmation of the quantum theory in the form first proposed by Planck (assuming absorption as well as emission by quanta), and gives to the study of these bands a large significance for the further development of the theory."[4] Kemble took Bjerrum's model of a molecule as a simple vibrating quantum rotator and modified it to include anharmonic vibrations and interactions between vibrations and rotations. Bjerrum's formula for the spectral lines of molecular bands was $v_t = v_o \pm v_r$ where v_o is the vibrational frequency and v_r the rotational frequency quantized to give $v_r = nh/2\pi^2 J$ (J the moment of inertia). (Bjerrum applied the traditional electrodynamic identification of radiation with mechanical frequencies.) With his inclusion of nonlinear terms Kemble obtained to second order $v = (v_o - a/v_r^2) \pm v_r$, the adjustment coming in a decrease in the vibrational frequency as the rotator speeded up, pulled apart, and sampled the non-linear range of the force holding the two atoms together.

Percy Bridgman supervised Kemble's work as a graduate student. Harvard's well-known experimentalist championed the cause of a young graduate student who wanted to do theory. Even though Bridgman could not help with the quantum theory, he did provide Kemble with a philosophy for doing physics, which Kemble described later as "heaven sent." Inspired by Einstein's definitions of space and time, Bridgman came to believe that all concepts in physics must be definable in terms of measurable quantities. To define a concept meant to explain, at least in principle, how to measure it. He argued that concepts not definable in operational terms were meaningless.[5] Kemble embraced Bridgman's operationalism, as it came to be called, and made it central to his own understanding of quantum theory. Even though Bridgman's operationalism provided Kemble with a philosophy of quantum mechanics, Bridgman himself never felt comfortable with (nor did he ever accept) quantum mechanics.

Kemble was given permission to do a theoretical thesis (one of the first presented in this country), but only after his advisor managed to convince other members of the department of its value. A compromise was agreed upon; Kemble must have an experimental section, too. Kemble collaborated with Brinsmade, the fellow graduate student who had introduced him to Bjerrum's theory, to obtain beautiful molecular spectra, which confirmed Kemble's postulated anharmonicity of vibrational motion.

A short piece on Kemble in McGraw-Hill's Men of Science series sharply criticizes Kemble for his equating of radiation frequencies with mechanical frequencies and his ignorance of Bohr's new frequency condition that gives radiation frequencies as differences in energy (rather than as a function of mechanical motion).[6] The absence of Bohr's theory from Kemble's work sheds light on history, however, and should not lead to the conclusion that the young American was ignorant. When Kemble was working on his graduate thesis, Bohr's frequency condition did not apply to molecular dynamics; it was clear from Bohr's papers of 1913 that the condition applied only to electronic motion and not to the rotation and vibration of molecules. Kemble made no mistake in ignoring it. The straightforwardness and success of Bjerrum's more semi-classical approach, which equated radiation frequencies with mechanical ones, delayed the application of Bohr's frequency condition to the infrared spectra of molecules. In fact, Bohr's frequency condition led to difficulties. Why were so many frequencies forbidden? Partly due to this difficulty it was not until 1919 that a unified explanation of frequencies would apply to molecular and atomic spectra.

When Kemble graduated from Harvard in June of 1917 the country was at war. Kemble felt it his duty to develop airplane engines at Curtiss Aircraft Company, which he did until he was laid off precipitously as the war neared its end. Although Harvard wanted him back as a faculty member (in fact, the department had never wanted him to leave), a position could not be found immediately, and Kemble taught at Williams College for half a semester. When Harvard did make Kemble an offer, he was shocked at the low salary and the low status of the position he thought that implied. Kemble told the department that he would have to support his parents in the future and reminded them somewhat cryptically of the "shipwreck of an engagement" he had suffered in the past. (After his first wife died Kemble married his fiancée from his graduate student years.)

In a long letter designed to lure Kemble to Harvard, his old advisor Bridgman explained his plans to build up theory at Harvard and to support its growth across the country. Kemble's coming to the university was crucial to the plan. Bridgman outlined a restructured curriculum that had Kemble teaching four upper-level courses (two of them graduate): radiation theory; quantum theory of the infrared, photoelectricity, and specific heats; X-ray crystal structure; and a special topics course in theory. Previously the Harvard department, like others in the country, had focused on electromagnetism (e.g., radiotelegraphy, optics, and wave propagation). More than three-quarters of the physics classes given in 1919 fell under this rubric. Now Bridgman envisioned a move away from this concentration, and he wanted his former graduate student's help.

> I am really enthusiastic about this scheme of courses. It comes pretty close to what I have been wanting for a long time. If we can get the courses well given, it ought to put Harvard pretty near the top in this country. What is more, it is a good beginning to putting the country on the map in theoretical physics. Course 22 [the special topics course] is designed especially for this, and would nominally be taken only by those students specializing in theoretical physics, of whom we shall hope for an increasing number. But you see that you are an essential part of this program. Don't you want to be a member of a Department that is trying to do this, and don't you feel the challenge in this?[7]

Kemble accepted the challenge. Establishing theoretical physics at Harvard and taking the department to the top was a heavy responsibility for a young man. Kemble started immediately. His first year at Harvard he taught one of the earliest courses in quantum theory given in the country. His approach to the subject was taken from Bridgman and exemplified the American approach to theory.

> It seems to me essential that we approach the subject in a proper frame of mind. The quantum theory is an attempt to correlate and ultimately to give a partial explanation of a series of startling facts which are in apparent conflict with the laws of classical mechanics and classical electrodynamics. I

say that it is an attempt to give a {*partial*} explanation of these facts because in the last analysis the physicists seek merely to formulate a few fundamental equations from which the behavior of matter may be predicted and into whose origin we will hardly inquire. . . . In such a subject as this we must not look for rigorous logical deductions and we must not make too much of the paradoxes which come up from time to time. The theory is simply justified by (a) the nature of the phenomena it is designed to explain, (b) the results already obtained in the shape of formulae which stand the test of quantitative comparison with the results of experiment, and (c) the gradual clarification of the fundamental ideas on which it rests.[8]

Kemble's first graduate student was John Van Vleck, and many followed in the next fifteen years (e.g., Clarence Zener, James H. Bartlett, Eugene Feenberg, and J. L. Dunham). Although Van Vleck and Kemble worked on the crossed-orbit model of the helium atom, most of Kemble's students used the quantum theory to shed light on molecular structure. In fact, this was generally true of the emerging quantum physics community in the United States during the twenties; the focus was on molecular structure, not, as in Europe, on atomic structure.

At this time there was a fine spectroscopic tradition in the country. Harrison Randall headed a major infrared spectroscopy laboratory at the University of Michigan. At the end of the nineteenth century, Ernst Fox Nichols at Cornell had developed the residual ray technique to isolate hard-to-detect infrared radiation, and his student William W. Coblentz had invented and improved instruments to detect infrared frequencies. Coblentz's three-volume work *Investigations of Infrared Spectra* became the reference work for molecular spectra, as had Heinrich Kayser and Carl Runge's for atomic spectra. The molecular dynamics of rotation and vibration generate spectra in the infrared. Electron motions in molecules and atoms generally produce spectra in the optical and higher frequencies.

The national origins of these two compendia (one American, the other German) point to the research focus each country took during the 1920s. While the Germans and other Europeans focused on atomic structure in their quest for the foundation of quantum theory, the Americans achieved maturity as physicists by studying the quantum nature of molecular structure. They shunned the waters of atomic physics and thus avoided competing with those whom Raymond T. Birge, molecular spectroscopist at Berkeley and Kemble's close correspondent, called the "atomic structure sharks."

Kemble was at the center of the research program in molecular structure. Having introduced the quantum problem to the United States, he went on to chair the National Research Council's Committee on Radiation in Gases, which during its three-year-long preparation (1923-26) of a book-length report *Molecular Spectra in Gases*, served as the coordinating group for a national research program. Kemble represented Harvard and the east; Randall's group at Michigan was represented on the committee by Walter F. Colby, and Raymond T. Birge spoke for the west from his position as a skilled molecular spectroscopist at Berkeley. A postdoctoral fellow at Harvard, Robert S. Mulliken, played a large role in the research and writing of the report, although he was not an official member of the committee.

A crucial ingredient for the growth and success of the research program in molecular structure were the post-doctoral fellows, like Mulliken. Funded postdoctoral research and education was set up after World War I by the Rockefeller Foundation and the National Research Council. These two institutions chose to support physics and chemistry by creating a number of non-teaching, one- to two-year research positions for young Ph.D.s. The existence of these research positions, intermediate between professor and graduate students, marked the beginning of the modern scientific research group.

One of the first such research groups was the one that surrounded Kemble at Harvard from 1923 to 1927. Mulliken arrived at Harvard in 1923 and in the following years was joined by three other postdoctoral fellows. The group worked to understand fluorescent band spectra, the Zeeman effect, and the vibrational-rotational bands that appear in the electronic spectra of molecules. Mulliken became known for his untangling of molecular isotopic effects.

The years 1923-26 were a fertile period for the understanding of molecular structure. Because the older quantum theory gave essentially the same energies for the rotator and oscillator as did the soon-to-come quantum mechanics, the conclusions reached about dynamical structure were to remain valid across the great divide of 1926 (the invention of quantum mechanics). The success of the molecular program pre-1926 moved Kemble to introduce the National Research Council's report with: "Although the theory of quanta has marvelously illuminated all branches of physics connected in any intimate way with atomic and molecular processes, few subjects have become more strikingly clarified than that of band (molecular) spectra."[9]

The stability of molecules remained an insoluble problem in the context of the older quantum theory, however. The solution of the binding problem for the hydrogen molecule by Heitler and London in 1927, usually marks the beginning of quantum chemistry, but the discipline's roots go farther back. The education of American quantum physicists in the early twenties through the study of molecular structure set the stage for an American-dominated discipline of quantum chemistry in the late twenties and thirties; in this Kemble played a key role.

Right at the heyday of excitement over the discovery of quantum mechanics, in 1927-28, Kemble spent a Guggenheim fellowship year in Europe, mainly at Göttingen and Munich. Here Kemble made what he later called the worst policy decision of his life: to finish up an older quantum theoretical calculation for band spectra rather than throw himself wholeheartedly into learning the new theory. To friends in the United States he wrote that he could not make heads or tails of von Neumann's first lectures on quantum mechanics (and he mentioned that neither could Max Born). In the next decade, Kemble was to more than make up for his initial neglect of the theory.

On his return to the United States, Kemble wrote with E. V. Hill two long review articles on quantum theory for the first issues of *Reviews of Modern Physics*. The articles were the first published exposition of the new theory in the United States. Kemble continued to work on understanding the basis of the theory, considering the meaning of probability in the quantum case and the relation between the wave functions and the physical states of the system. Kemble's efforts to secure a mathematical foundation for quantum mechanics culminated in his textbook *Fundamental Principles of Quantum Mechanics* (1937), a book so detailed and mathematical in its attempt to ground quantum mechanics operationally that it was little used as a textbook. Kemble openly attributed his approach to Bridgman's. Foundational concepts should be based on explicitly measurable properties, not on intuitive ideas or metaphysical comforts.

The care and consideration Kemble brought to his understanding of quantum mechanics—in many ways a mea culpa for his earlier decision to disregard the theory in 1927—was antithetical to a pursuit of his own research in molecular structure. In 1969 in a short autobiographical sketch, he wrote, "I am proud of them [the papers and the book on the foundations of quantum mechanics] and too deeply interested in questions of clarity in the organization of knowledge to wish that I had taken a different course in 1929. But I did pay a high price for my interest in philosophy."[10]

With World War II came another shift in Kemble's career. Many of his colleagues worked for the duration of the war at MIT's Radiation Laboratory. Kemble, who chaired the physics department from 1940-1045, supervised the teaching of basic physics to military officers. He consulted for the Navy's underwater sound laboratory during the war and in 1945 was part of the overseas ALSOS mission, whose top-secret job was to uncover German atomic bomb research.

Kemble enjoyed and was intrigued by his wartime task of explaining physics to non-physicists. At war's end, he had a chance to continue this work. Reacting to the great role science played in the war, James B. Conant, president of Harvard, high-level administrator in the bomb project, and chemist, proposed to teach science to all Harvard undergraduates by teaching them the history of science. Conant hoped to highlight the importance of science for social change. Kemble enthusiastically joined the general education project, and a lunchtime group was set up in the physics department to try to enact the ambitious plan. (It included Kemble, I. Bernard Cohen, Gerald Holton, Thomas S. Kuhn, Philippe Le Corbeiller, and Leonard K. Nash.)

As part of the general education program, Kemble taught a course in the physical sciences to non-science majors. The cartons of student papers he kept attest to his love of the job and his belief that writing the history of science could stimulate the imagination of those who would have to manage what he called the "issues of the day, . . . war and peace, racial injustice, overpopulation, automation, the pollution and contamination of the atmosphere and water supply [and] the breakdown of traditional values."[11] During the fifties, Kemble worked on restructuring the curriculum for physics majors as well. His major contribution was to chair a committee that forwarded recommendations for a revision of standard electromagnetism courses given at the college level.

Kemble's concern about the conditions of modern society was integral to his political and personal life as well as to his teaching. He protested security restrictions in the National Science Foundation bill of 1950, encouraged scientists to join the Federation of American Scientists during the Cold War, and played a role in the peace movement as part of a Methodist congregation.

Kemble retired from Harvard in 1957, having spent all but three years there since the time he entered graduate school. For three years after retirement, he was director of Harvard's Academic Year Institute, where high-school teachers could study with university professors. The beneficiaries of Kemble's teaching were many: young postdoctoral researchers, graduate students, undergraduates (both scientists and non-scientists), and finally high school teachers (and indirectly their students). He served his scientific community in official capacities as chairman of the Physics Section of the National Academy of Sciences (1945-48) and as a member of the Executive Committee of the National Research Council's Division of Physical Sciences.

Kemble was embarrassed and always apologetic about his scientific output. "As you see, my career has not been one of great distinction," he wrote.[12] The feeling was intensified by the high-caliber students he saw blossoming under him, physicists like John Van Vleck, Robert S. Mulliken, John C. Slater, and J. Robert Oppenheimer. After his wartime teaching experience, Kemble made a decision: "I saw myself spending the rest of my life panting to try to keep within hailing distance of what was going on. I deliberately quit being a scientist at that time although I continued to teach."[13]

Looking back at Kemble's entire career allows us to take a broader perspective than Kemble himself and recognize his value as a community builder, a task so in concert with his religious beliefs. Kemble's most important contributions to research were introducing the study of a quantum molecular structure to the United States and presiding over the budding research community that worked on the problem. Americans learned quantum physics by studying molecules. There is good reason to believe that this is why quantum chemistry was predominantly an American discipline when it emerged in the late twenties. It is foolish to attribute such large-scale developments to any one person, but it is reasonable to claim someone a place as one of perhaps several motivating forces. I believe that such a place belongs to Kemble.

Edwin Crawford Kemble died on March 12, 1984.

NOTES

1. E. Kemble interview with T. Kuhn, October 1, 1963. Archives for the History of Quantum Physics.

2. Ibid.

3. E. Kemble interview with T. Kuhn, May 11, 1962. Archives for the History of Quantum Physics.

4. E. Kemble. The distribution of angular velocities among diatomic gas. *Phys. Rev.* 8(1916):689.

5. P. Bridgman. *The Logic of Modern Physics.* New York: Macmillan, 1927.

6. *Modern Men of Science*, vol. 2, pp. 285-86. New York: McGraw-Hill, 1968.

7. P. Bridgman to E. Kemble. Lyman correspondence, March 16, 1919, box 8, folder K-1919, Harvard University Archives.

8. E. Kemble lecture notes for physics 16a, 1919-1920, box 18. Harvard University Archives.

9. E. Kemble et al. Molecular spectra in gases. In "Report on the

Committee on Radiation in Gases." Bull. no. 57, p. 9. Washington, D.C.: National Research Council, 1926.

10. E. Kemble to S. S. Ballard, December 20, 1969. Harvard University Archives.

11. E. C. Kemble. *Physical Science, Its Structure and Development*, p. 14. Cambridge, Mass.: MIT Press, 1966.

12. E. Kemble to S. Ballard, op. cit.

13. E. Kemble interview with T. Kuhn, October 1, 1963. Archives for the History of Quantum Physics.

SELECTED BIBLIOGRAPHY

1916

Note on the end effect in the electrostriction of cylindrical condensers. *Phys. Rev.* 7:614-24.

The distribution angular velocities among diatomic gas molecules. *Phys. Rev.* 8:689-700.

On the occurrence of harmonics in the infra-red absorption spectra of gases. *Phys. Rev.* 8:701-14.

1921

The probable normal state of the helium atom. *Phil. Mag.* 42:123-33.

1923

With J. H. Van Vleck. On the theory of the temperature variation of the specific heat of hydrogen. *Phys. Rev.* 21:655-61.

1925

The application of the correspondence principle to degenerate systems and the relative intensities of band lines. *Phys. Rev.* 25:1-22.

1926

Molecular spectra in gases. Bull. no. 57. Washington, D.C.: National Research Council.

1927

With R. S. Mulliken. Zeeman effect in the Angstrom CO bands. *Phys. Rev.* 30:439-57.

The rotational distortion of multiplet electronic states in band spectra. *Phys. Rev.* 30:387-99.

1929

With E. L. Hill. On the Raman effect in gases. *Proc. Natl. Acad. Sci. U. S. A.* 15:387-92.

With E. L. Hill. General Principles of quantum mechanics. Part I. *Phys. Rev.* 1(suppl.):157-215.

1930

With E. L. Hill. General principles of quantum mechanics. Part II. *Rev. Mod. Phys.* 2:1-59.

With F. F. Rieke. The interaction between excited and unexcited hydrogen atoms at large distances. *Phys. Rev.* 36:153-54.

1935

The intensities of the vibration-rotation bands of HCl. *J. Chem. Phys.* 3:316-17.

The correlation of wave functions with the states of physical systems. *Phys. Rev.* 47:973-74.

1937

The Fundamental Principles of Quantum Mechanics. New York: McGraw-Hill.

1938

Operational reasoning, reality, and quantum mechanics. *J. Franklin Inst.* 225:263-75.

1939

Fluctuations, thermodynamic equilibrium and entropy. *Phys. Rev.* 48:549-61.

The quantum-mechanical basis of statistical mechanics. *Phys. Rev.* 56:1146-64.

1941

The probability concept. *Phil. Sci.* 8:204-32.

1950

With others. The teaching of electricity and magnetism at the college level. I. Logical standards and critical issues. II. Two outlines for college teachers. *Am. J. Phys.* 18:1-25, 69-88.

1951

Reality, measurement, and the state of the system in quantum mechanics. *Phil. Sci.* 18:273-99.

1954

Scientists and political action. *Sci. Mon.* 78:138-41.

1966

Physical Science, Its Structure and Development. Cambridge, Mass.: MIT Press.