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DAVID MATHIAS DENNISON

1900—1976

A Biographical Memoir by

H. RICHARD CRANE

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Biographical Memoir

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April 26, 1900–April 3, 1976

BY H. RICHARD CRANE

DAVID MATHIAS DENNISON, distinguished theoretical physicist and member of the National Academy of Sciences since 1953, died on April 3, 1976 at the age of seventy-five. His principal work concerned the application of quantum theory to the interpretation of the infrared spectra of molecules, a field in which he was a pioneer discoverer, and in which he remained a leader throughout his life. He made important contributions in other areas as well, including the first application of microwaves to spectroscopy, the exploration of the optical properties of thin films, and the theory of high energy accelerators.

David Dennison was born in Oberlin, Ohio on April 26, 1900. Since his father was a professor of classics, he grew up in an academic atmosphere. The family made several moves among academic settings: from Oberlin, Ohio to Ann Arbor, Michigan (the father's native state) when young David was two years of age; to Rome, Italy for a sabbatical year when young David was seven; and to Swarthmore, Pennsylvania when he was ten. David continued his schooling there, and graduated from Swarthmore College in 1921. His college years were difficult. His father died in 1917, the year in which David entered college. To make ends meet David obtained a scholarship, and his mother served as a housemother at a dormitory. As a further complication, upon the entry of the U.S. into World War I, David was enrolled in the Student Training Corps.

In 1921 David Dennison enrolled as a graduate student at the University of Michigan in Ann Arbor, the town in which he had lived as a boy. There he received the Ph.D. in physics in 1924. The summer of that year was an eventful one for him. He was married to Helen Lenette Johnson of Ludington, Michigan, he was granted a General Education Board (Rockefeller) fellowship for two years, and he and his bride departed for the Institute for Theoretical Physics in Copenhagen. The period of study in Europe was extended to include a third year, which was divided among three leading centers of physics: Zurich, Copenhagen, and Cambridge.

In 1927 the Dennisons returned to Ann Arbor where David had an instructorship in the Physics Department of the University of Michigan waiting for him. He remained a member of the faculty of the University for the rest of his career, and he served as chairman of the Physics Department from 1955 to 1965. He retired to become Professor Emeritus in 1970, but continued to be active in physics research until the end of his life.

It is always interesting to try to see how talented scientists first become interested in science. In this case we are fortunate to have the transcript of a lengthy interview with Dennison conducted by Professor Thomas S. Kuhn of Princeton University in 1964 for the Sources for the History of Quantum Physics Project (see acknowledgement). The interview is revealing in many ways, and it is rich in the history of physics, for David Dennison was a good teller of stories, including those about himself. Much of the information that follows has been drawn from that source.

It often turns out that one's choice of a career can be traced to the influence of a friend or a teacher encountered at an early age; Dennison's case was no exception. There were two such individuals: one was an Episcopal minister and the other was a technician for a mustard company. Both were ardent amateur scientists and tinkerers who lived in the neighborhood and welcomed interested high school students after school hours. David was a regular visitor, and he worked with, or learned about, electrostatic machines, telescopes, spectrographs, arc lights, replica gratings, double pendulums, lathes, surface tension phenomena, and many other things. Using what he learned, he later set up, on his own, a spectrograph, a double pendulum, and a water-motor that drove a dynamo. He was tremendously excited about all of this. The enthusiasm with which he recounted these early adventures to his interviewer would surely lead one to think that he would have followed the track of experimental science—but that was not to be the case. During his college years he gravitated toward the mathematical approach to physics, which was to be the theme of his life work. We shall try to trace how this attraction grew.

After entering Swarthmore College, further adventures in experimental science were opened to David. Immediately he was allowed to use the College's 6-inch telescope. A little later he became involved in a program of systematically recording the parallaxes of certain stars, and that gave him access to the College's 24-inch telescope. In an electrical engineering course he was given the project of building a Tesla coil that would give a 6-foot spark. To his great satisfaction, he succeeded. He did not lack for opportunities to experiment at any time. Nevertheless, before he progressed very far in his college career, his primary interest turned toward understanding physical problems by mathematical, rather than experimental, methods. The elegance of that approach appealed to him, and he found that he could understand the mathematics with ease. Real physical systems remained his primary interest, but his method of analyzing them had moved toward the theoretical. There is no better evidence of the strong tie Dennison retained between his interests in theory and real "hardware" than the fact that throughout his life both his home and his office were replete with instruments that exemplified simple principles of physics.

An interesting sidelight, indicating that Dennison did not "catch on" to the ease with which he could solve problems by theory until he was well into college work, is found in the taped interview. He remarked that the introductory physics course failed to capture his attention, and he did poorly, receiving a grade of C. He remarked further that during his first two years he had to work hard on all of his courses, but that at the end of two years he had somehow learned the trick of working the examinations, so that from then on, he got straight A's. He said he supposed that after he had acquired the trick of it he learned much less!

Dennison's first encounter with research physicists came in the summer of 1920, between his junior and senior years. The General Electric Research Laboratory at that time was offering summer appointments for students, and the engineering professor who had coached Dennison in building the Tesla coil encouraged him to apply for an appointment. The experience proved to be a great eye-opener. David was assigned to assist Irving Langmuir, who was trying to understand and use the new Bohr model of the atom. David had just studied the model in a course, so they started on an even footing and learned together. Later that summer, David was assigned to assist A. W. Hull, whose interests were the diffraction of X-rays from powdered materials. Dennison's work during the summer was mainly to attempt to interpret Hull's data by means of theory, but he did at times have a chance to operate the apparatus. With Hull's help, he published a paper—his first. What he got from the summer was primarily, of course, the introduction to real research scientists in a real and vigorous laboratory. He returned to the same laboratory for two later summers, 1922 and 1923. At those times he worked mainly with Saul Dushman, who studied thermionic emission problems. Dennison's association with the Laboratory resulted in an offer of employment at the conclusion of his Ph.D. program, but he declined because he was intent on continuing his studies of theoretical physics.

To go back a bit, when in 1921 Dennison presented himself at the Physics Department of the University of Michigan to begin his Ph.D. program, he announced that he wanted his thesis to be in theoretical physics. Professor H. M. Randall, then Department chairman, was, the story goes, quite astounded, in view of the outstanding experimental program in the Department, and the fact that no theoretical thesis had so far been sponsored. But Randall acceded.

The first part of David's graduate program was rather uneventful: he was a teaching fellow, and he took graduate courses, some (as he remarked) good and some bad. In the latter part of the program he became immersed in the theoretical interpretation of the infrared spectrum of methane, which Randall and others in the laboratory were measuring. He worked mainly with Walter F. Colby, H. M. Randall, and with a visitor from Copenhagen, Oskar Klein. During that period the laboratory had the honor of a visit by Niels Bohr, and it was through that brief but exciting association that Dennison made up his mind to find some way to go to Copenhagen as soon as he received his Ph.D.

Dennison's way of getting to Copenhagen is interesting. He applied for and was awarded a National Research Council fellowship. But because it contained a new proviso that it was to be used only in the U.S., he declined it. Through the efforts of one of his former Swarthmore professors, he was granted a General Education Board (Rockefeller Foundation) fellowship to go to Europe for two years. He was able to extend his stay for a third year with the help of funds from the University of Michigan. Professor Randall arranged for the extension as what now would be called a "holding pattern." He wanted to hire David, but he would not have the position to offer for another year.

Dennison's years in Europe coincided with a period of great excitement in physics. He was in the right places at the right times and got to know most of the important persons. Among these were Werner Heisenberg, Niels Bohr, and Erwin Schrödinger. He became especially interested in the new (and controversial) matrix mechanics that were being developed by Heisenberg, and he applied it to what seemed to him a very challenging problem, that of the rigid, symmetric rotator. His work may have been the first application of the new quantum theory to anything beyond the simple two-body system. It resulted in a publication. But what did most to establish Dennison in the world of physics was a piece of research he did quickly and just at the end of his stay in Europe. This occurred at Cambridge University, and through an interesting chance happening, as David recounted it in a talk he gave much later.*

Dennison related that during his time in Copenhagen he had become acquainted with R. H. Fowler of Cambridge, who invited him to visit Cambridge before returning to the U.S. When he arrived, Fowler was giving a seminar course on statistical mechanics, and he invited David to give three of the talks about matters of his own choice. Preparation for these out of material David already knew went smoothly as far as such material lasted, but it fell short of what would be needed to fill the three sessions. In the search for material to fill the gap, he decided to have another look at a perplexing problem

* "Recollections of Physics and of Physicists During the 1920's," *American Journal of Physics,* 42(1974):1051–56.

he had worked on with no success while in Copenhagen: the theoretical calculation of the specific heat of hydrogen gas. This had long been a real puzzle. Excellent measured values were available, and they were in striking disagreement with careful calculations that had been made by several theorists, including Dennison himself. The discovery of the spin of the electron in 1925 had aroused hopes that the problem would be solved if a spin were assigned to the proton, but that tack had come to naught. Dennison, in rethinking the problem for his seminars, retained the spin of the proton and, in a rare insight, added a new condition, namely, that the ortho and para states of the molecule were very long lived and that the ortho-para ratio did not change appreciably during the time in which the temperature was varied in the measurements. To quote from Dennison's 1927 note in the *Proceedings of the Royal Society* (see bibliography): "The coupling of the nuclear spins with the spin of the molecule which determines the transitions between symmetrical and antisymmetrical terms will indeed be very small, much smaller than the coupling forces between the electronic spins and the orbits which give rise to the very weak transitions between ortho- and para-helium. Let us make the assumption that the time of transition between a state symmetrical in the rotation, and a state antisymmetrical is very long compared with the time in which the observations are made. In this case we have in effect two distinct gases..." (all earlier calculations had, in effect, assumed rapid equilibration between the ortho and para modifications). By using this new postulate, and by using for the ratio of populations of the antisymmetric and symmetric forms of hydrogen the value three, which he could justify on quantum theory grounds, Dennison obtained a curve for the specific heat that agreed with the experiments exactly.

With the simple matter of the lifetime of the ortho-para states cleared up, a much more fundamental aspect of the agreement became evident. Since the agreement depended on assigning a spin of exactly ħ to the proton, it constituted (turning the argument around) the first quantitative (although indirect) measure of the value of the spin of the proton. Naturally, Fowler, in whose seminar course this was described, urged David to write his work up for publication. In writing the manuscript, however, David did not remark explicitly about the implication of the result for the spin of the proton because, as he recalled in a seminar talk* in later years, "It was so obvious there seemed to be no need to belabor the point. Like the names of the streets in the center of town; there is no need for signs because everyone knows the names." It remained for Niels Bohr, to whom David sent the manuscript, to urge David to make the point explicitly, which he then did. He also added a remark about the possibility of physically separating the ortho- and para-hydrogen gases. The work on the specific heat of hydrogen formed a most successful conclusion to Dennison's three-year experience in Europe and established his reputation in molecular physics.

Upon returning to the University of Michigan and taking his place in the midst of a burgeoning program of experimental infrared spectroscopy, Dennison found an abundance of raw material to feed his interest in molecular theory. He became especially interested in explaining the spectra of the simple molecules of water vapor, methane, carbon dioxide, ammonia, and methyl alcohol. All of these molecules were being worked on at Michigan in the large experimental program headed by H. M. Randall. Throughout these years Dennison worked very closely with the experimentalists, and

*Remarks, University of Michigan, Department of Physics Colloquium, November 15, 1968.

to him should go a great deal of the credit for the leadership the laboratory enjoyed in the field of infrared spectroscopy.

If Dennison's molecular work had a special theme, it can be found in his concentration on simple molecules whose physical constants happened to be in just the right range to elucidate particular basic phenomena; examples are carbon dioxide with its Fermi resonances, water vapor with the full complexity of an asymmetric rotator, ammonia with its inversion frequency, and methyl alcohol with all the features of rotational tunneling. He was very discriminating, working not to turn out papers, but always to follow some subtle and basic point that intrigued him. He never lacked for such questions; his work on methyl alcohol, which began in the 1930's, continued, with publications, until the end of his life. About forty-five of his papers concern the interpretation of molecular spectra. He almost continuously supervised doctoral students in that subject—about twenty in all.

A short time after Dennison joined the Michigan faculty (1927), the Department greatly expanded its capacity in theoretical physics. This was due to the efforts of H. M. Randall, the chairman, and Walter Colby, the resident theorist. Three promising young theorists besides Dennison were hired: Otto Laporte, whose work was in optical spectroscopy, and George E. Uhlenbeck and Samuel A. Goudsmit, who togehter had discovered the spin of the electron two years earlier. The four young theorists, with the vigorous backing of Randall and Colby, expanded the Department's summer session into a symposium of international importance, which continued until it was stopped by World War II. Dennison was a key organizer throughout the series.

A most interesting episode took place in the early 1930's. In 1932, Dennison and Uhlenbeck had discovered a vibration mode in the ammonia molecule that should give rise to the absorption of radiation at a remarkably long wavelength—about 1.6 cm. The three hydrogen atoms in the ammonia molecule form a triangle, and the nitrogen atom lies in the center, but not quite in the plane of the triangle. The nitrogen can occupy symmetrical positions on either side of the plane (the situation has been referred to as the "reversing umbrella"). This configuration is unique to ammonia, and it is responsible for the long wavelength absorption. By coincidence, Professor Neil H. Williams and a graduate student, C. E. Cleeton, had been working in the Physics Department on the development of magnetrons of very small size that could generate radiation in the range of a centimeter or less. Dennison saw the opportunity there and persuaded them to collaborate with him in an attempt to measure the 1.6 cm wavelength absorption in ammonia gas with the magnetron radiation. The experiment, which required a large room full of venetian blind–size diffraction gratings, parabolic dishes, and a balloon of ammonia gas, was completely successful. The year was 1933, and the experiment constituted the birth of microwave spectroscopy, a technique that much later came into widespread use. But it was before its time—Williams, Cleeton, and Dennison carried it no further, and it was not reborn until after World War II.

During the war years, Dennison worked on problems associated with the VT (radio proximity) fuze. Much of his work was on the evaluation of the performance of the fuze, resulting in recommendations for changes in its characteristics to make it more effective. In the early years of the war he used data that he helped generate through scale-model experiments (a project at the University of Michigan), and later he used data that were transmitted to him from the battles in the Pacific Theater, where the ammunition was in daily use. For his contributions in this program, he received a citation for exceptional service from the U.S. Navy.

Dennison's work with the scale-model VT fuze experiments had an interesting delayed result. The experiment involved "flying" model airplanes past radio oscillators. Since the experiments were supposed to simulate the effects in free space, the reflections of the radiation from the ground had to be eliminated. This was done in the way that had been used by Winfield Salisbury, of the University of California at Berkeley, which was by stretching a sheet of poorly conducting cloth at a height of one-quarter wavelength above the conducting ground plane. (The cloth, which was made in quantity by the United States Rubber Company, came to be known as "Salisbury's shirt tail.") Dennison was intrigued at the time with this technique, and made many calculations about it. In retrospect, it should have been obvious that revolving in his mind was the possible application of the scheme (but on a microscopic scale) to infrared measurement problems. Immediately after the war he turned his efforts to that application. He and a thesis student, Lawrence N. Hadley, employed evaporated layers of transparent materials to create nonreflecting surfaces and band-pass filters for the infrared. Today, evaporated nonreflecting and filtering films have a multitude of uses, including, of course, the coating of all photographic lenses.

In the first few years after the close of World War II, Dennison explored yet another subject that was new to him. This was a study, with Theodore Berlin, on the stability of the orbits or particles in a new type of high-energy accelerator. It was occasioned by a proposal by H. R. Crane to modify a synchrotron into the form of two half-circles separated by straight sections, called, for obvious reasons, a "racetrack." While the straight sections would offer many practical advantages, the problem was that construction of such a machine could not be undertaken unless it could be proven that the particle orbits would be stable. Dennison and Berlin rose to the challenge and devoted about a year to the study.

The result was a paper that set forth the general conditions for the stability of the particle orbits in an accelerator having any even number of straight sections. It showed that such an accelerator could operate stably, and it therefore gave the go-ahead signal for the Michigan "racetrack." (The study showed that there would be an advantage in having four straight sections rather than two, and that change in the design was made.) The paper by Dennison and Berlin served as a text for other accelerator builders as well for some time to come. I understand why Dennison was immediately intrigued by the problem of an accelerator with straight sections. He saw it as a beautiful and complex example of a stability problem involving the Mathieu equation, which he had discussed for many years in his course in theoretical mechanics. He even had among his classroom props a simple mechanical model that demonstrated the principle.

Mention must be made of another interest Dennison had, which occupied him for a few years beginning in about 1939 and again in the early 1950's—the effort to apply his methods of treating molecules to the determination energy levels in nuclei. The oxygen-16 nucleus was particularly suitable, because he could think of it as a "molecule" of four alpha particles. He made the simplest assumption as to the configuration, namely, that the alpha particles were at the points of a tetrahedron, and then proceeded to apply the quantum conditions and selection rules to their vibrations and rotations. The results were encouraging, but the experimental data to which they could be compared were at the time rather meager. Later, in the 1950's, when the data were much better, Dennison returned to the problem and published another paper. The study represented an approach to energy level calculations that was different from that taken by the nuclear theorists of his time, and it proved to be a forerunner of later active studies of light nuclei according to the alpha-cluster model.

In the preceding few paragraphs, some wide-ranging problems that captured Dennison's interest and to which he was able to make contributions have been mentioned. For the most part, they were problems that came to his attention during his wartime experience. But it should be emphasized that his main interest, molecular rotations and vibrations, was remarkably durable. He returned solidly to it after every side-adventure.

Dennison accorded no less importance to teaching than to research. For decades he gave a major graduate course each semester, changing off among the subjects of theoretical mechanics, quantum mechanics, and electricity and magnetism. There is little doubt that the theoretical mechanics was his favorite. Past graduate students of all ages, when recalling their experience at Michigan, will mention Dennison's theoretical mechanics course as a high point (and a not inconsiderable hurdle).

Dennison's interest in teaching went beyond the preparation of the material to an interest in the process itself. He frequently had advice to give to young instructors or teaching assistants. A set of notes that he evidently made for himself in order to give a talk on teaching to a group of graduate students has survived—and is illuminating (also still good advice). The pity is that the illustrative stories he mentions were in his head and so are lost to us. The notes are repeated here in full, for they tell more about the man than would ten times as many words by this writer.

What can I tell you about how to teach? Very little. Great distrust of all educational systems. Nevertheless:

First: and most important point: want to be a good teacher—willingness to put in the time and energy in preparation.

Second: Analyze other teachers and colloquia speakers—what they do right—what they do wrong.

Third: Always watch your audience. Bored? Cannot follow? Watch for the sparkle and smile.

(The Fermi Story)

The mechanics of teaching:

1. Start on time—2 minutes of resume.
2. Stop on time. Resist the temptation to finish a subject.
3. Write clearly and speak slowly. Be sure you give the audience time to take notes.
4. Try to encourage questions, but discourage screwballs.
5. Always prepare—understand every point and more. 10 hrs. for a point that takes 5 minutes.
   (Story of suddenly discovering a flaw).
6. Allow some latitude in presentation, but not complete.
   (Story of Klein and of writing for Bohr.)
   (Story of Kramers)
7. Give enough problems, but not too many. Always work your own problems. Why?
8. Give enough blue books. Ask principles not tricks, not complicated algebra. Always work the questions yourself, in a blue book, and be able to complete them in 1/3 the allotted time.
9. Never get angry—unless on purpose.

The wonderful experiences, and the tradition:

The stories of:

Ehrenfest and Goudsmit
Ehrenfest and Uhlenbeck
Sommerfeld and Laporte
Bohr and me
Bohr and the idea that could not be expressed in words.

When it comes to hobbies, it should be said first of all that molecules and teaching were David's hobbies as well as his livelihood. Beyond that, his love of precise mechanisms led him to have in his house and office such things as a ship's chronometer, a precision lathe, fine cameras, a compound pendulum (by which he made beautiful Lissajous figures on photographic paper), a gyroscope from a World War II bombsight, and a surveyor's transit. Once he set up a Foucault pendulum in his basement, suspended from a floor joist, and found, after weeks of observations, that the deflection the pendulum produced in the house was the source of a systematic error! In all of these adventures he had pure scientific fun, without feeling that he had to break any new ground.

During his active career David Dennison was the recipient of many honors and prestigious appointments. Some have already been mentioned, namely, his election to the National Academy of Sciences and the citation by the U.S. Navy. In addition he received an honorary D.Sc. from Swarthmore College in 1950. He was appointed delegate to the Seventh General Assembly of the International Union for Pure and Applied Physics in 1951. He was selected as the University of Michigan's Henry Russel Lecturer in 1952 (the lectureship, the recipient of which is chosen by the Research Club, is the highest honor the University can bestow upon a faculty member for distinguished scholarship). He received a Distinguished Faculty Achievement Award from his University in 1963, and he was elected president of the Research Club of the University of Michigan in 1964. In 1966 he was appointed Harrison M. Randall University Professor. In 1975 he was invited as the principal speaker at the Fourth International Conference on Molecular Spectroscopy, at Tours, France. As a further high distinction, he was made an honorary citizen of the City of Tours (that was only the second time in the City's long history that the honor had been bestowed upon anyone). In 1976 the new physics and astronomy building at the University was named for Dennison, not only in recognition of his scientific achievements, but of the fact that earlier he had supplied the driving force that had brought the building into being.

Needless to say, Dennison performed many services within the University, in addition to serving as chairman of the Physics Department for ten years. He was considered an elder statesman and was called upon in critical matters. An interesting sidelight on his way of operating, however, is the fact that he was quite determined (successfully) to sidestep what he termed time-wasting committee appointments.

David Dennison is greatly missed by all who were privileged to know him. He was a scholar and friend in the finest tradition. He is survived by his widow, Helen Dennison, and two sons: Edwin W. Dennison, who resides in California, and David S. Dennison, of New Hampshire.

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