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JOHN HASBROUCK VAN VLECK

1899—1980

A Biographical Memoir by

P. W. ANDERSON

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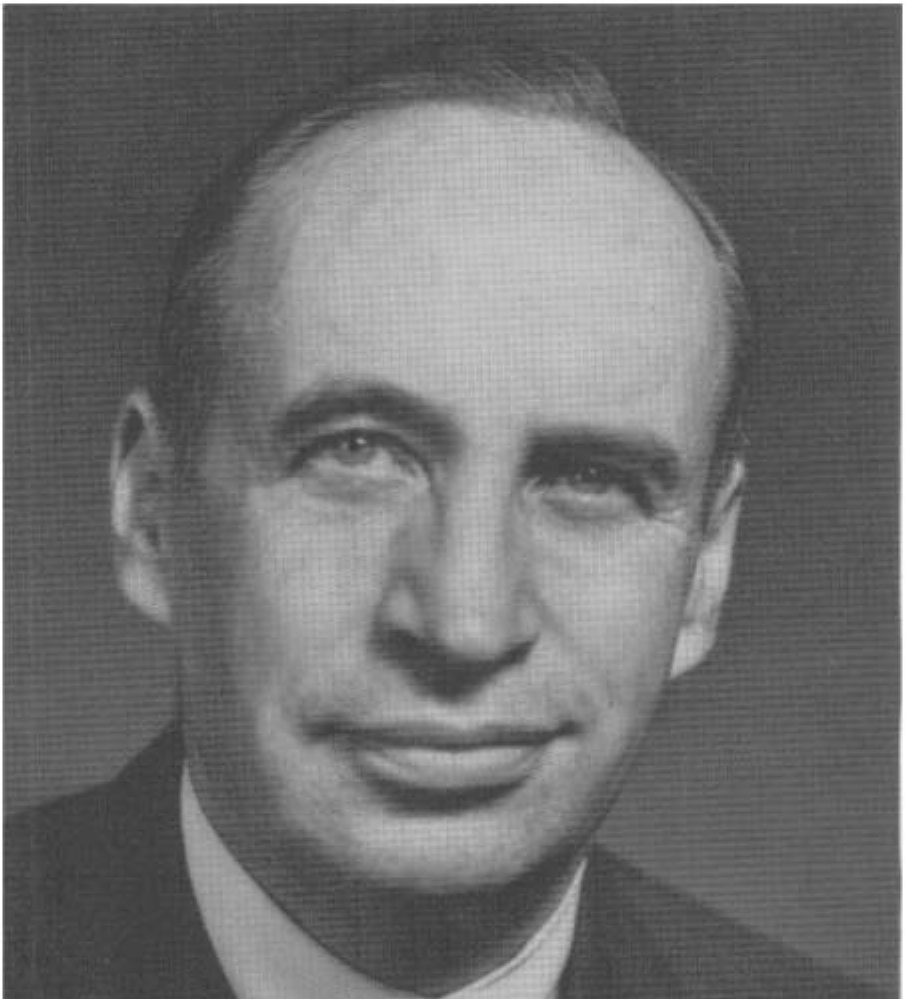

J. H. Van Vleck

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March 13, 1899–October 27, 1980

BY P. W. ANDERSON

JOHN HASBROUCK VAN VLECK was the most eminent American theoretical physicist between J. Willard Gibbs and the postwar generation. He has often been characterized as the "father of modern magnetism," but his influence was in fact much wider: He played a vital role in establishing the modern fields of solid-state physics, chemical physics, and quantum electronics. Many generations of students were influenced by his unique teaching style, and he made important administrative contributions at a crucial time in the history of Harvard University.

FAMILY AND EARLY YEARS

The Van Vleck family is of the patrician Dutch stock that has given the nation three presidents, among other eminent citizens. "Van" (as he was always known) was proud of his ancestry, which had been traced by an aunt[1] to prosperous burghers of Maestricht in the sixteenth century. One of the family, Tielman Van Vleck, was an eminent citizen of the Dutch colony of Nieuw Amsterdam and founded Jersey City.

Van's immediate family was very distinguished academi-

[1] More details will be found in The Royal Society memoir by B. Bleaney.

cally. His grandfather, John Monroe Van Vleck, was professor of mathematics and astronomy at Wesleyan University in Middletown, Connecticut, from 1853 to 1904, serving as acting president on two occasions. John Monroe's brother, Joseph Van Vleck, a successful New Jersey businessman, donated an observatory to Wesleyan in his honor. At the dedication ceremony in 1916, his son, E. B. Van Vleck, spoke and the young J. H. Van Vleck unveiled the memorial plaque.

All of John M. Van Vleck's four children were mathematicians, including Van's father Edward Burr Van Vleck (1863–1943). E. B. Van Vleck took a doctorate at Göttingen in 1893, taught for two years at Wisconsin, and then went to Wesleyan (1895–1905). He married Hester Raymond of Middletown in 1893, and here John Hasbrouck Van Vleck, his only child, was born on March 13, 1899. From 1905 to 1929 Edward Burr Van Vleck was professor of mathematics at the University of Wisconsin in Madison, Wisconsin, where the mathematics building is named Van Vleck Hall in his honor. He was eminent in his field and highly respected at Wisconsin. He was a member of NAS, president of the American Mathematical Society, and recipient of four honorary degrees. His lectures were noted for their formal clarity.

The E. B. Van Vleck home was a cultivated and a prosperous one, since he inherited a portion of his uncle's fortune. He built up a notable art collection, especially of Japanese woodblock prints but also of other objects of beauty. It is said that a number of the prints were acquired from Frank Lloyd Wright. They were collected during the building of the Imperial Hotel and sold to repay debts. Edward Burr and his wife Hester read voraciously and traveled widely, so that "the galleries, churches, and mountains of Europe were equally familiar." In this atmosphere Van absorbed his interest in travel and his deep cultivation very naturally.

Except for a few periods spent during his father's sabbaticals in schools or kindergartens abroad, Van was educated in the Madison public schools and went directly on to the University of Wisconsin. He did not recall any particular partiality toward science or mathematics as a boy, nor did his father make a special effort to interest him in advanced topics of mathematics, except for advising him to be sure to take mathematics through calculus in college. Reading his own remarks about this period of his life, one has the impression of a very normal boy with a rather matter-of-fact precocity. His interest in American football began early at Wisconsin, and he claimed that the Wisconsin song "On Wisconsin" was first sung at his first game in 1909 (information on this is contained in an article of his on the history of football songs). He played in the Wisconsin marching band from 1916 until 1918—his instrument is not recorded, but it was probably the flute, which he played later as a young assistant professor. His well-known interest in railroads began early: It was when he was about seven that he first spent a period of recuperation on one of his parents' European trips learning the relevant railroad schedules, and thereafter the family never again had to consult a timetable. In college, an early interest in French was turned off by his self-confessed "miserable" accent, and in geology by the obtuseness of a professor who "required all triangles to be solved graphically."

In fact, until late in his college career, Van seems to have seen himself as a bit of a dilettante. His youthful preference had been not to go into the academic life, and he recorded in his reminiscences that "serious young men took engineering rather than math or physics, where most of the students were girls." Just as he rebelled at solving triangles graphically, he evaded the physics senior thesis—involving experimental work, including, worst of all possible fates, glassblowing—by joining a debating team and arguing successfully against the government ownership of railroads. His stated reason (in

"Reminiscences of the First Decade of Quantum Mechanics," 1971) for taking physics was the otherwise light course load and the idea that mathematics would involve him in his father's courses, which would not be "cricket." He did, however, make Phi Beta Kappa in his junior year, so one is permitted a great deal of skepticism about the rather frivolous motivations he gives for his choice of careers. I note also the inclusion of an "unlisted reading course in Molière" in the accelerated program he finally completed—surely not a common interest for a young scientist in the Middle West.

EARLY CAREER IN PHYSICS

Van's consciousness of physics seems to have been first raised by the experience of a course on kinetic theory taken from L. R. Ingersoll in his third year at Wisconsin. He was not well prepared because he had put off the calculus to this same year, but he sat near Warren Weaver who was taking the course (though on the junior faculty) and was a very vocal critic of the textbook used. This stimulated Van's interest enormously. In the next year he took a course from Professor March on dynamics that was his introduction to genuine theoretical physics and showed him very clearly the course of his future career.

A fortunate accident took him to Harvard for the last semester of the 1919–20 college year: His father was spending a sabbatical there, and urged him to finish up at Wisconsin in three-and-a-half years and come take courses at Harvard. He started out with three courses in math and physics and one in railroad administration. This latter convinced him that he "would not get on very fast in the railroad business." The courses that influenced him positively were by Bridgman and Kemble. He felt Bridgman's operational philosophy, while not explicitly stated, was very much implicit in the attitude to physics that he acquired, while Kemble was one of two or three teachers in the United States concerned about quantum theory. He was influenced negatively as well by a formal mathematical course in differential equations, a replacement for his father's course, which he had been unwilling to take; this added to his lifelong distaste for empty formalism.

Thus, almost without conscious decision, he found himself a full-fledged Ph.D. student at Harvard. He quickly completed the requirements for an M.A. (1921) and finished a Ph.D. thesis under Kemble (1922). He stayed on as instructor with Kemble for one more year, leaving for Minnesota in 1923.

EARLY DAYS OF THE QUANTUM THEORY: HARVARD (1920–23) AND MINNESOTA (1923–28)

His thesis topic attacked one of the truly knotty problems of the old quantum theory: the attempt to come up with a method of quantization that would give reasonable results for the helium atom. Once the hydrogen atom was more or less dealt with, and simple harmonic motion understood with Debye's and Planck's laws, the next stage was clearly more complex atoms. At about the same time, several European physicists were attempting the problem, and Van's paper was essentially equivalent to results of Bohr, Kramers, and Kronig. In short order there followed a sequence of papers on various aspects of spectroscopy using the old quantum theory, and then a book, *Quantum Principles and Line Spectra,* finished in late 1925.

During the Harvard years, Van was in constant touch with John Slater, who finished a mostly experimental thesis with Bridgman at about the same time, and then left for Copenhagen to work for a year with Bohr. I think it is fair to say that this little American group kept in remarkably close touch with the enormous activity centered in Central Europe that was to lead to the new quantum mechanics, and it contributed at least two papers of real significance, one of which was Van's paper of 1924 on the correspondence principle for absorption. This paper comes tantalizingly close to the kind of considerations that led to Heisenberg's matrix mechanics; it was one of the few papers to attack the intensity question that was the key failure of old quantum theory. Van maintained close contact with European physics, and was fortunate enough to accompany his parents to Europe in 1923; during the trip he made time to visit Bohr and had extended discussions with Kramers in Holland.

The University of Minnesota invited both Van and Gregory Breit to come as assistant professors in 1923. This was a unique opportunity. Only graduate teaching was required, and he would have someone to talk with, because Breit's interests were very close to Van's, so he left the annual instructorship available to him at Harvard with no apparent reluctance. (Slater got the only permanent faculty job!) He was at Minnesota for five years, rising to associate professor in 1926 and full professor in 1927. On June 10, 1927, he married Abigail Pearson of Minneapolis. Of his courtship he remarked that they both loved dancing and that her ignorance of his work assured him that she would never interfere in it. He joked later that he made up his mind when, at a dance, she introduced him as the professor of chemistry.

His first book was published as a Bulletin of the National Research Council. At that time NRC committees were occasionally formed with the express purpose of informing American scientists about important developments, and this book was a report of the Committee on Optical Spectra, with Paul Foote as chairman. It was well received, but by the time it was issued in early 1926 much of it had been superseded by the enormous explosion of results from the new quantum mechanics. The exercise, however, was of considerable value to the author; his remarkably clear writing style had been formed, with the acknowledged help of his father, and his very physical understanding of classical dynamics and of the workings of the correspondence principle, which were to serve him well in his later career, had been developed.

Van learned with great excitement of the new quantum mechanics through correspondence and the early published papers of Heisenberg on the matrix mechanics, which remained for a number of years his preferred form (in this, as in many other things, his style was unique). He sent in his first paper using it in early 1926, showing that the classical symmetry factor 1/3 in the magnetic susceptibility is restored in the new theory. *Nature* asked him to shorten the piece, which slowed him down to what he called a "quadruple tie" in publication. He sailed for a visit to Copenhagen that summer and completed another paper on the boat calculating mean values of inverse powers of r by matrix mechanics—only to learn that he had been beaten by practitioners of Schrödinger's wave mechanics. Nevertheless he wrote no less than four papers that year on the new mechanics, and for several years he continued to write papers discussing its nature and interpretation (such as [18]).

The little note in *Nature* [10] was to set the theme for much of the rest of his career: the use of the new quantum mechanics to elucidate electromagnetic properties of matter. This had been an interest of his since an early seminar on the Weiss theory of ferromagnetism, and it was implicit in his correspondence principle paper; but from now on his interest led to a stream of fundamental papers, and it became clear that he had chosen this as his particular portion of the great work of verifying and using the new quantum theory. Bohr remarked, with characteristic insight, that while the spectroscopic successes of the quantum theory were more spectacular, the macroscopic ones such as Van worked on were in some ways "more satisfying and more fundamental." The most fundamental immediate results were his demonstration of the general formula (which he always carefully called "Langevin-Debye") for the susceptibility (papers [10], [11], [15] in the accompanying bibliography) and the detailed application to O_2 and NO [12]. But at this time, he had already begun the work on molecular and other spectra in the new mechanics that was a second major theme in his life with a paper with Hill on rotational distortion [16]. He was asked by the chemists to review the new quantum mechanics [17], and in general was an important teacher and proselytizer of the new knowledge in the American context. His worldwide reputation was assured by the series of remarkable experimental verifications of his results on O_2 and NO in Leyden, MIT, and Zurich.

WISCONSIN (1928–34) AND PREWAR HARVARD: MAGNETISM, MOLECULES, CRYSTAL FIELDS, AND THE ORIGIN OF MAGNETIC RELAXATION

Although Minnesota was, as he later said, very congenial, with Breit as a coworker, Tate and other experimentalists encouraging him, and a number of bright auditors interested in his lectures (Tate himself, Bleakney, Brattain, and others), physics was more active at Wisconsin. Van accepted an offer of a professorship at the University of Wisconsin in 1928, remaining there until 1934. This gave him the pleasure of overlapping for one year with his father and renewing several old associations. It was at Wisconsin that he conceived and finished his book, *Theory of Electric and Magnetic Susceptibilities*, although much of it was written on a European trip in the summer of 1930. His stay in Zurich was particularly productive; arriving in vacation time, he worked until Pauli returned and, with characteristic rudeness, said: "I don't republish *my* papers into a book!" In fact, the book, aside from being arguably the first book in the modern field of solid-state physics, contained—as Van defensively remarked—much new material aside from the contents of the several papers on Langevin-Debye, "Van Vleck paramagnetism" (the second-order contribution of what he called "high-frequency matrix elements"), and the magnetism of the rare earth and iron group ions in salts (with Miss Frank [23]), which had already appeared. There were new discussions of the averaging process for obtaining the fields in materials, of Heisenberg's theory of ferromagnetism, of aspects of dielectric local fields and of dielectric theory in general, of Landau diamagnetism and its relationship to the classical theory, and the like. It is marked—perhaps even slightly marred, as a modern text for physicists poorly trained in classical mechanics—by careful discussion of the ways in which quantum mechanics, the old quantum theory, and classical physics differ. It just missed a number of very important developments, notably Van's own crystal field theory work and Néel and Landau's concept of antiferromagnetism, but in one sense it is not dated in the slightest: All results quoted in the book are, to my knowledge, correct to this day, needing no revision, only expansion. It was an enormously influential book and set a standard and a style for American solid-state physics that greatly influenced its development during decades to come—for the better. We have, incidentally, R. H. Fowler's suggestion that Van write for the Oxford University Press to thank for the book's existence.

During the same Guggenheim fellowship trip that took Van to Zurich in 1930, he was invited to be the only American at the Solvay Congress. In Holland, he spent a number of days talking and walking with his friend Kramers, who pointed out to him Bethe's recent work on group theory. This led to a lifelong interest in group theory, which he still taught in inimitable style—and from Wigner in the original German—when I was at Harvard in 1948. It also led to a series of applications that Van seemed to feel were among his best work: to the spectroscopy and susceptibilities of magnetic ions in solids (papers [25], [36], [43], [45], [53], [54], [60], [61]; and papers by his students M. H. Hebb, R. Schlapp, and W. Penney, later Baron Penney, on most of which his name, characteristically, did not appear). In these papers he introduced the "crystal field" concept in which a magnetic ion is envisaged as behaving more or less like a free ion perturbed by the anisotropic potential of the surrounding ions and atoms. The orbital angular momentum in most iron group ions in solids is "quenched" (a typical Van concept) by such fields, because of the weakness of spin-orbit coupling relative to the crystal field splittings, while rare earth ions retain free ion character but respond to the local symmetry by the appropriate splittings of the energy levels. This concept correlates enormous masses of spectroscopic, magnetic, and even chemical data on these compounds (the chemical energetics were developed by Penney and by Orgel and Ballhausen, much later). It provides an absolutely essential starting point for the understanding of all the technically important insulating magnetic materials, such as ferrites, garnets, ruby, and the like. Van initiated both of the main branches of the theory of crystal fields, the naive electrostatic version and the "ligand field" theory (now more generally accepted) in which the emphasis is on semicovalent bonding to the neighboring "ligand" ions or groups; and with his characteristic flexibility he demonstrated (paper [43]) that both led to essentially the same experimental results. In the chemistry and spectroscopy of these ions, Van's (or Schlapp and Penney's) name for the field strength parameter, "*Dq*," is still used. Of the crystal field theory it has been said by Moffitt and Ballhausen (perhaps a bit extravagantly, but Van liked to quote this statement): "It will be a long time before a method is developed to surpass [it] in simplicity, elegance and power."

Other applications of group theory in paramagnetism first brought out by Van in the 1930s were the importance of Kramers' degeneracy (a consequence of time-reversal symmetry) in leaving at least two degenerate levels for odd ions, and the application of the Jahn-Teller theorem to deduce small distortions from perfect symmetry in certain cases, distortions that could lead to complex mixed electronic-vibrational states (now called "vibronic") due to tunneling among symmetry-equivalent configurations ([54]).

Two experimental developments sparked his continuing interest in paramagnetism: the adiabatic demagnetization method, especially as practiced at Oxford to reach temperatures below 1°K, and the paramagnetic relaxation work going on at Leyden. The former stimulated several of the papers already quoted, as well as [50]; the latter led to Van's remarkable and prescient calculations of the paramagnetic relaxation caused by lattice modulation of the crystal field parameters ([59]), as well as his invention of the "bottleneck" concept ([66], [67]), which sparked many important experiments after the war. The sum total of his achievements in magnetism in the 1930s after his book appeared can best be appreciated by reading his 130-page Institut Henri Poincaré lectures of May 1939. These were given in French (he remarked that he had had to go to the Riviera to recover, and he did not know what had been necessary for the auditors) and because of war conditions not published until 1947 ([77]). To summarize, he left in place, as a result of this body of work, the conceptual structure from which the science and technology of quantum electronics and much of the science of magnetism arose in the next two decades.

To finish out his contributions to magnetism in this period, we mention his paper on ferromagnetic anisotropy [49], a noteworthy first discussion of this difficult subject, further treated in Harvey Brooks' thesis; and his important clarification [63] of the Néel-Landau theory of antiferromagnetism, putting the subject in vector model form. This made the theory much more suitable for experimental comparisons than the qualitative form given by Néel and Landau. As is often the case, the existence of a formal model stimulated theoretical interest, and the so-called "Heisenberg antiferromagnet," which Van first introduced, has become a favorite subject of theoretical investigation, beginning with Kramers' and my own work on the antiferromagnetic ground state.

Two other subjects, also related to his interest in group theory, constituted much of the remainder of Van's work in this period. These were molecular spectroscopy and the theory of chemical binding. A number of papers ([24], [28], [29], [38], [40], [52], [56], [57]) showed his continued interest in molecular spectra, which dates back to his early work on Λ-type doubling and was a lifelong theme of his work. There is also an important group of papers on CH_4 and on valence theory ([26], [27], [31], [32], [41], [42]), which are the only prewar attempt, to my knowledge, to understand directed valence bonding from a fundamental point of view, rather than to simply postulate it as Pauling did.

Van particularly enjoyed trying to reconcile opposing schools of thought on the important questions. As I remarked, he actually originated both schools in the case of crystal and ligand field theory. He tried, in this sequence on chemical bonding, to show that Hund-Mulliken molecular orbital theory, and Heitler-London valence band theory as adapted by Pauling (and called the Slater-Pauling theory) could lead to the same results for directed covalent bonds such as in CH_4. This work was unaccountably neglected, and it is not until very recently that a real attempt has begun to reach the level of fundamental conceptual understanding of the chemical bond that Van was seeking. Later, as we shall see, he tried to build the same kind of "Van Vleck bridge" (as Purcell termed it) in the theory of ferromagnetism.

Van picked up and contributed to whatever subject was at hand. The local field corrections in dielectric and magnetic media remained an interest ([35], [46], [47], [48], and especially [64], which is a very important clarification of the local field problem which I, at least, found very useful later). He contributed to questions of the interpretation of quantum mechanics ([18], [23], [65]), to nuclear physics and the problem of neutron diffraction ([39], [58], [68]), and to the theory of ferromagnetism ([44], [51]). As student or postdoc problems he touched on atomic energy level theory ([30]), on band theory ([55]), and on intermolecular forces ([56]).

In 1934 Van had accepted an offer to take up a full professorship at Harvard. Initially, his courses were listed jointly in the mathematics and physics departments, since his appointment was in mathematical physics. He was asked by the then president of Harvard, J. B. Conant, to foster education on the less abstract and more applied side of mathematics. Conant believed that his mathematics department had become too remote and that the courses at the graduate level were of little use then (as in my day as well) to physicists, chemists, and others who needed advanced mathematical training. This is not a tendency that even Van could stem (as experience in many other institutions attests), and twenty years later, when he took up his appointment as dean of Applied Science, he resigned sadly from the Mathematics Department, and the Division is the present home of applied mathematics at Harvard.

During this period it is interesting to see Van's interests actually moving progressively away from the abstract or mathematical aspects—the questions that had concerned him initially having been solved by the new quantum theory—toward applications and his eventually most important role as an experimental consultant. Van regretted audibly the drift of the majority of his colleagues in theoretical physics toward more abstract mathematics and toward nuclear, and then particle, physics, although he had at least one student at Wisconsin, R. Serber, who later excelled in that domain, and he was to build up Harvard's strength in that area of physics while chairman of the department.

From the first Van made a point of minimizing his role in his students' and associates' work. With some—especially of the better known, such as Serber and Brooks, as well as with Hurwitz, Jordahl, and myself—there were no joint publications at all. With others, such as Hebb, Schlapp, and Penney, the bulk of the work, though wholly inspired by Van's ideas, was not published under his name. This was even more true of postdoctoral associates; he brought John Bardeen and Nico Bloembergen to Harvard as junior fellows, as well as many visitors in other capacities, such as Broer, Van Kranendonk, Gorter, Abragam, and others with whom his name is not usually closely linked.

HARVARD: WAR AND IMMEDIATE POSTWAR YEARS VAN AS CONSULTANT, GREY EMINENCE, AND MIDWIFE TO THE BIRTH OF QUANTUM ELECTRONICS

Although Van continued some teaching at least through 1943, he carried out several roles, especially that of head of the theory group (after 1942) at Harvard's Radio Research Laboratory, a smaller, closely linked cousin of MIT's giant Radiation Laboratory (at which he spent some time in 1942–43). The two laboratories played a very important role in the development of microwave radar, working closely with Bell Labs and other military and industrial laboratories. (Van had early contact as an adviser with the uranium project but was much more closely associated with radar work. Perhaps some future historian of science should trace the rather marked influence that the radar laboratories had on Harvard and MIT physics, as contrasted to Chicago, Cal Tech, and Berkeley, which were heavily involved in Los Alamos and the bomb project and developed in a very different way.)

Several wartime projects strongly influenced by Van can be identified through later papers: one on the theory of WINDOW (clouds of metal foil strips used to fool radar), an unlikely collaboration with Morton Hamermesh and Felix Bloch ([73]); a classic and technologically vital pair of reports on identifying pulses in the presence of noise, carried out with David Middleton ([72]); and, particularly, his discovery that the ill-fated "K-band" radar operating at $\lambda = 1.25$ cm would be a fiasco because of atmosphere absorption by molecular lines of O_2 and, particularly, water. In order to work out this conclusion quantitatively it was necessary to revise accepted theories of collision broadening (since the relevant lines would be severely broadened) in such a way as to include induced emission as well as absorption, and the appropriate revision was carried out with V. F. Weisskopf ([70]). This paper was of the utmost importance both experimentally and theoretically in the immediate postwar years. Its experimental significance is evident and well known: It was a chief tool for interpretation of the mass of experimental data that were produced in the great outburst of radio-frequency spectroscopy after the war—in Bleaney's lab at Oxford, Wilson's and Purcell's at Harvard, Townes' at Bell and Columbia, and Gordy's at Duke, among others. But it had an even deeper significance, in that it was the precursor of my own work (itself stimulated by Van) on pressure-broadening and line-breadths in magnetic resonance spectroscopy, and of other early versions of the calculation of physical results using fluctuation–dissipation methods, and hence sparked one of the earliest lines of inquiry into what became the many-body theory.

In my opinion Van's status as a figure of major importance to the history of science rests most securely on his role in the immediate postwar years at Harvard, rather than on his pre-war achievements, massive as these are. During this time, for instance, he was closely associated with no less than four Nobel prizes (those to Purcell, Townes, Bloembergen, and the joint prize in which we participated). I suppose that with his no-nonsense attitude to scientific credit and to who did what, and with his persistent belief—against all odds—that all his associates were as quick and perceptive as he was, Van himself would never have realized the important role that he played as adviser and consultant, but all of those who worked near him at that time, especially the experimentalists, gave him much credit for the very rapid progress they made.

It is hard to take one's mind back to that time and recognize the mental leap that coherent spectroscopy required. Rabi's molecular beam methods, of course, have often been cited as the original for radio-frequency spectroscopy, but the peculiar environment and detection methods made that a rather esoteric specialty. It was really Van who noticed the early measurement of Cleeton and Williams on ammonia (its importance to him he mentioned in his charming little article, not in the bibliography, entitled "Molecular Spectroscopy in Ann Arbor and Outer Space," although I never heard him refer to Rabi's method as seminal) and, during the war, single-mindedly pushed on with identifying the levels of O_2 and H_2O that could lie in the centimeter wave region and hence interfere with the proposed "K-band" radar ([74], [75] were based on this work). At the same time he maintained close rapport with the Leyden group, which was pushing relaxation spectroscopy up in frequency from the radio-frequency end, culminating in Gorter's work during the war and shortly after, and which was the most technically close (because of its emphasis on line-width and relaxation, as well as its instrumentation) to the Purcell group's discovery of nuclear magnetic resonance, and especially to electron paramagnetic resonance. Most of those who later took part in the explosion of coherent spectroscopy after the war met at wartime conferences at which Van's work was discussed. (I was at one such where Bleaney, Van himself, and Townes, at least, were present.)

In any case, he was a central figure—perhaps *the* central figure—in taking the first steps in establishing the field that eventually came to be known as quantum electronics. In essence, this field can be defined as the physics of the interaction of coherent electromagnetic radiation with atomic, molecular, or solid-state systems of quantized energy levels. The first step, of course, is the mental leap of recognizing that suitable systems of energy levels exist in reasonable profusion, and with energy level breadths that do not overwhelm the quantization of the levels and leave one with a featureless classical smear. Van brought together the knowledge of molecular energy levels, the appropriate techniques (Waller's moment method as applied by Broer in Holland during the war, and then by Van [79] and the Van Vleck-Weisskopf pressure-broadening theory) for estimating line breadths, and an encyclopedic knowledge of the physics of electric and magnetic interaction that were necessary to get a start in this field. One finds his help acknowledged and his papers quoted in early papers in all the coherent spectroscopies: NMR, EPR, molecular microwave spectroscopy, and later ferromagnetic resonance spectroscopy as well. His name even appears on an experimental paper in molecular microwave spectroscopy, the discovery paper (by Dailey et al.) of hyperfine structure in this field, which also determined the quadrupole moment of N^{14} [70]. Other papers directly on the various quantum electronic spectroscopies were Van Vleck and Weisskopf (already mentioned), the discovery of exchange narrowing with Gorter [76], his beautiful overall summary of dipolar broadening and exchange narrowing [79], a paper on ferromagnetic resonance [83], [87], and a pair on pressure broadening ([82], [97]). A number of other papers continued his prewar interest in calculating molecular, ionic, and atomic energy levels: [78], [80], [89], [93], [94], and [95], with some emphasis on the new spectroscopies.

There were at least two other contributions of major importance during this immediate postwar period. Tom Kuhn, later to become very well known as a philosopher and historian of science, collaborated with Van in inventing a new technique, the quantum defect method, for using spectroscopic data directly to calculate band energies in the alkali metals [84], [96], [98]. This was an important forerunner of modern pseudopotential methods, and is very close to the most recent "norm-conserving pseudopotential" method. It was refined extensively by Frank Ham, and the elegant mathematical machinery has been a source of a number of developments.

Van's student Hurwitz had written an unpublished thesis about the many-body theory of ferromagnetism in metals. What Van and he wanted to do was to strike a middle ground between the free-electron theory purists, especially Stoner, who were determined to apply the pure Bloch band theory of ferromagnetism and ignore the necessarily strong electronic interactions otherwise; and the naive "Heisenberg model" theorists who proposed that the magnetic electrons stay in purely atomic states with no itinerant character at all. Van's middle ground, expressed at a seminal meeting in 1952 and written up for *Reviews of Modern Physics* in 1953 ([91], [92], [110]), had the essential components of the correct way to treat strongly magnetic itinerant electrons. Though little regarded in the heat of battle of the time, it formed a basis for important work by Hubbard, Gutzwiller, Herring, myself, Moriya, and others that advanced this difficult problem greatly in the years to follow. This work, in which he was groping toward the same set of concepts being considered by Mott at the same time with regard to the Mott transition but not to magnetic phenomena, makes their joint Nobel prize seem a little less arbitrary.

HARVARD: THE LATER YEARS SCIENTIST-ADMINISTRATOR

After the great burst of creative energy that I have just described, Van was never again to be at the forefront of several active scientific areas at once, as he had been throughout the quarter century from 1925 to 1950. One may speculate that this—purely relative—slowing down coincided with his assumption of two demanding administrative jobs, chairman of the Physics Department (1945–49) and dean of Engineering and Applied Physics (1951–57). He had been, from the start, involved in the discussions that led to the combining of the Engineering School with the Department of Applied Sciences to make the School, and it was only natural to give the new entity a good start by making him its first dean; its second was his student Harvey Brooks. Also during 1952–53 he was president of the American Physical Society. He was closely associated with the work of the APS, and Bill Havens, the present secretary, remembers his help with much gratitude, both at that time and later. As chairman of the Physics Department, Van presided over the admission—and recruitment—of an extraordinary group of students in the first few years, brought back at irregular times and from the ends of the earth, mixing refugees, returning servicemen, and bright young products of the wartime accelerated courses. It surprised me to find how many of us felt that he personally intervened to get us to Harvard. At the same time he was recruiting junior and senior faculty: Bloembergen, Pound, Schwinger, Ramsey, and Purcell, among others, were his appointments. Those years were a golden age of Harvard physics, and few of us who participated in them can have been unaware of that fact.

Even more demanding was the creation of the Engineering and Applied Physics School. The no-man's land between engineering and physics, or more accurately, bordering mathematics and chemistry as well, is a very sensitive region, with delicate problems of defensiveness, snobbery, and intellectual fragmentation. To get the new entity going, to keep the lines of communication open, and in particular to make the resulting unit operate well with the Physics Department, was a remarkable achievement. Van has left Harvard, as his most enduring legacy, one of the world's strongest groups in these areas, particularly in condensed-matter physics and materials science, the successor areas to his own interests. One of his stratagems in doing this was the "Van Vleck Bridge," an actual physical connection between Cruft Laboratory, where many of the applied scientists were, and the Jefferson Physics Lab.

After six years as dean, Van returned in 1957 to more or less private academic life, although first he served as vice president of the IUPAP, 1958–60, and then he took up two visiting professorships: the Lorentz Professorship at Leyden in 1960 and the Eastman Visiting Professorship in Oxford, 1961–62, where Abigail and he were the first to occupy the new residence provided. Then he returned to Harvard until his retirement at the age of seventy in 1969.

All during this period, of course, he was maintaining a level of scientific publication that would have seemed high for anyone else, especially if one includes the many review papers he was asked to give, and the increasing stream of scientific reminiscence and commentary that began to flow.

His strictly scientific output resumed its normal flow in 1957, and dealt mainly with aspects of magnetism, a field that increasingly claimed his scientific attention. He was delighted to find his old friends, NO and O_2, captured in 1957 in a solid-state "clathrate" compound by H. Meyer, and to interpret the results of measurements on them ([102], [120], and a similar measurement by Huang, [153]). He wrote a stream of papers on various aspects of rare earth magnetism, including some basic papers on the now technically important garnet materials (used in bubble memories), the first of these in 1960 with Werner Wolf ([113], [119], [123], [127], [128], [130], [131], [132], [134], [138], [139], [140], [147], [164], [166]; the last two in 1974 and 1975, well after his retirement). Other rare earth materials discussed included europium metal [114]; Sm intermetallic compounds [118]; EuO, where he helped interpret the surprising ferromagnetic behavior discovered by his old friend Richard Bozorth and Bernd Matthias [121]; Eu_2O_3 [150], [152]; and Ho-Er alloys [159].

He retained an interest in the theory of magnetism in metals, and continued to the end to be properly skeptical of reports of progress. (I well recall being asked repeatedly: "Do you *really* think the 'U-T' model explains magnetism?"—his name for Hubbard's model based on his own nomenclature of 1953 for the relevant parameters and on his private joke about the University Theater in Harvard Square.) His own work included mostly review and discussion papers ([103] in 1957; a set of Varenna lectures, [110], [125], [141], all given at meetings, the last at a Sanibel symposium in his honor in 1966). His interest in magnetic anisotropy and other weaker effects led to a few papers: a review article at the first "Bozorth" conference in 1956 [99], and [112]; these in addition to several of those on the rare earth materials.

Finally, he continued his interest in spectroscopy and relaxation, and particularly in the new concepts being introduced by Bloembergen and Abragam of spin temperature and in his own old idea of the Van Vleck bottleneck at the phonons, which led to several reviews and some original papers: another set of Varenna lectures of 1957 [105]; [103], [108], [116], [117], [122] (a paper at a Quantum Electronics conference in 1961: the field now had a name); [128], and [135].

Reviews proliferate in his later work; in addition to those we have already mentioned, there was one on spin waves, with Van Kranendonk (who also took an interest in his old field of pressure broadening, [106]); EPR and magnetism [90]; exchange [109]; antiferromagnetism [86], (where he kindly publicized my own early work); rare earth magnetism [137]; and line breadths (with David Huber, his last contribution of some ten or more to *Reviews of Modern Physics,* [167]).

One very small group of papers commemorates a rather bizarre incident in Van's life. These are the papers on "Hidden momentum" [148] and [151]. (The first is a collaboration, delightful to behold, between one of the department's youngest and most famous particle theorists, Sidney Coleman, and Van, about to retire in 1968.) Van had been asked to referee a paper by the now notorious Nobel prize winner, Bill Shockley, proposing a new point of view on energy and momentum transport by light in the presence of polarizable media. This esoteric question had some small experimental interest in view of the availability of high-power lasers and the capability of measuring their effect on media, but on the whole the subject did not merit the extreme importance Shockley placed on it as an example of the success of his so-called "Try-simplest-cases" way of carrying on scientific investigation (I always wondered what he thought the rest of us do), and of the short-sightedness of the scientific establishment. Shockley somehow seemed to identify the physical science establishment with that primarily biological one that opposed his views on heredity and race (a subject in the complication of which the application of "Try simplest cases" can be a disastrous blunder). In any case, I presume Van had been an outspokenly opposed referee on Shockley's paper, and after a rather bitter correspondence not marked on Shockley's side even by the normal perfunctory courtesies, Van felt moved to write a refutation. The bizarre event was that Shockley then threatened both Van and the *Physical Review* with a lawsuit for libel, on the basis that this paper called his scientific reputation into question, a reputation which was of immense value to him in the great work he felt he was doing on behalf of "human quality." I believe this is the only public controversy in which J. H. Van Vleck has ever been involved, and the story does him honor.

TEACHING AND OTHER PERSONALIA

I don't know if there exists anywhere a full list of Van's graduate students. It doesn't really matter, because some of those most influenced by him, such as John Bardeen, Walter Brattain, W. G. (Baron) Penney, and Nico Bloembergen, were not formally his students. As I have already remarked, most of his students published their work independently, so it is rather hard to trace them; a fair list is given in Bleaney's memoir. Among those who were, formally, or considered themselves, his students, are at least two eminent administrators of science and technology, one a peer of the realm in England and one his successor as dean at Harvard; one of the country's premier historians of science; an eminent and widely respected particle theorist; an eminent plasma theorist; and a Nobel prizewinner. Just as Van wrote the first American thesis on the old quantum theory, his student E. L. Hill wrote the first one on the new quantum mechanics. Van was a master of that delicate judgment that allows the student just the amount of freedom he can manage, while at the same time the student remains conscious that there is a "safety net" in case he cannot make it on his own. Van seems to have been remarkably successful at the process; there were very few failures. (I know of none, in fact, but merely suppose there must have been some.) The decision to work with him was one of the wiser choices of my life.

Van's early lectures at Minnesota, Wisconsin, and at the famous Michigan summer schools of the early 1930s where, as he said, American physics came of age, had a great influence historically. By the 1940s, however, his teaching style had become unique, and is remembered with fondness by everyone I spoke to. Most of the material was written in his inimitable scrawl on the board, and he spoke in a very personal style, using such favorite phrases as "engineering approximation" (often referring to a highly sophisticated mathematical procedure quite beyond the capability of your average engineer), "hand that over to our mathematical slave," and the like. Especially in group theory, his intuitive feeling for the subject often bewildered us as he scribbled down symmetry functions in an offhand shorthand to demonstrate what we thought were exceedingly abstruse points. He had a schoolmarmish way of interrupting himself with a "what?" to the class, usually getting a murmur that he took to be the end of his sentence, and I suppose like most lecturing tricks it served to maintain a proper pace. But he did at least once come into class and start his lecture with "A clever trick is—what?"

In all of his classes, however, he used two basic techniques of the genuinely good teacher. First, he presented a set of carefully chosen problems, which really contained the meat of the subject, often with "hints" that I usually found hopeless as helps but highly useful as explanations. Second, he supplied a "crib" for examination study, which we always thought was practically cheating, saying precisely what could be asked on the exam. It was only after the fact that you realized that it contained every significant idea of the course.

He continued to lecture at summer schools until quite late in life, often in exotic places, in keeping with his love of travel. Several of his lecture note sets—the Henri Poincaré in 1939, and Varenna in 1957, for instance—are important scientifically. As time went on he was increasingly in demand for semi-ceremonial speeches, and often these, with their gentle wit and unmistakable flavor, took the form of reminiscences or remarks on the state of his beloved science, as his articles on walking with Dirac; on his "Swiss visits of 1906, 1926 and 1930"; or his deploring of barriers between minds in his presidential address. Nonetheless, more often than not he chose to emphasize technical content, to the end of his life. His wit was ever gentle; he was never sarcastic or self-important, delighting in strange juxtapositions ("Molecules in Michigan and Outer Space"), coincidences, and in riding his own incongruous hobby-horses to extreme lengths—as in using the official or football nickname for every university in the country, as "Sooner" for a person associated with Oklahoma. He never told "jokes" in the usual sense of the word, especially avoiding off-color speech.

He loved travel and knew the cities of the world well—well enough to have a favorite hotel in Hong Kong, for instance. Abigail almost always traveled with him. They were inseparable, and her wit was an excellent foil to his, slightly more personal and acerbic, occasionally expressing the impatience with people that Van never permitted himself. Bridge was a favorite avocation of theirs, and still is for her; they were excellent players. In his younger years Van was a dedicated walker, both in the fields near Madison and in the wild places of the world—Colorado, the White Mountains, the Alps, and many other places.

Incidentally, he always managed his own investments, and

I believe very much increased the reasonably comfortable fortune his father left. The only detail I ever heard revealed was that he spotted the departure of a first-rate young crystal chemist from Bell Labs for the newly formed Texas Instruments Corporation at the right time, and invested in TI, a hundred-fold winner. In any case he had enough to give generously to Wisconsin, including the gift of his father's print collection, and to Harvard.

LAST YEARS

In 1969 Van retired, remaining in Cambridge except for the inevitable travels (we find papers in this period from Melbourne, Rumania, Cambridge, London, and Holland) and writing an occasional scientific paper, as we have seen. He kept in mind until very late the project of updating his book, but surely that was too massive a task. The only replacement at a comparable level is the six-volume Rado-Suhl series, so some very rigorous selection would have been required. Honors continued to flow to him, such as the coveted Lorentz medal, 1974; Chevalier of the Legion d'Honneur, 1970; foreign membership in The Royal Society; and finally the Nobel prize. Aside from an operation, he remained in good health until about 1975, when he began to have a heart weakness that required a pacemaker. At this time the Van Vlecks decided to give up their lovely old house on Fayerweather Street for an apartment at 989 Memorial Drive, in which Abigail still lives.

I saw Van in London in 1968, at the time of his "signing the book" at The Royal Society, and his speech was as sparklingly witty and as full of new ideas about magnetism as ever. We attended a play of which I remember only that it was a bit modern and negative for the Vans' determinedly old-fashioned point of view with regard to literature, or music, or styles in science, for that matter. The result was some sharp discussion from Abigail.

Ten years later, in Stockholm, with his pacemaker, the wit was still there, but it seemed that there was a short "duty cycle"; he sparkled for an hour at a time or so, and then rested. His brief speech, as the eldest, on behalf of the physics laureates is a model of taste and grace, complimenting his hosts and his fellow laureates with a sure hand.

He continued to travel, but encountered medical problems again on a trip to the West Coast to honor his old friend Julian Schwinger. Finally, on October 27, 1980, his heart gave out for good, and we lost the grandest representative of what he himself called the "Coming of Age of American Science."

In speaking of his own father at the dedication of E. B. Van Vleck Hall at Madison, Van quoted his father's precepts: "Two qualities may be noticed as especially needed by the American [scientist]. The first is a broad, liberal culture. The pursuit of [science] in itself is doubtless narrowing . . . its abstract height tends to separate one from daily life. A wide liberal culture therefore is eminently desirable."

The second is "moral fiber and force, as exhibited in patience with students."

No one can have better satisfied these goals than Van himself.

FIRST AND FOREMOST, I have freely borrowed material from Brebis Bleaney's admirable memoir for The Royal Society. For the opportunity to read this memoir, I am very grateful. Among other unpublished material I used for background were interviews by T. S. Kuhn from the AIP Center for the History of Physics. The speeches given in memorial services at Harvard and at Wisconsin were quite useful, as well as those at the dedication of E. B. Van Vleck Hall at Wisconsin, including Van's own. Finally, his fairly extensive reminiscent articles were very helpful, as were some insights from conversations with Abigail Van Vleck, Nico Bloembergen, and others.

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