American Physics Comes of Age

By J. H. Van Vleck


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J. H. Van Vleck, Hollis professor of mathematics and natural philosophy at Harvard University, was honored by Case Institute of Technology on December 11, 1963, when he became the first recipient of the new Albert A. Michelson Award. Professor Van Vleck, whose address on that occasion appears below, was cited for his pioneering contributions "to theories of magnetism which provide the essential understanding of solids and have led to important scientific and engineering developments".

Although in 1919 I was an undergraduate at the University of Wisconsin, my parents properly felt that it would be a salutary experience for me to spend a summer on another campus. At that time, the University of Chicago had the preeminent graduate summer school in physics; so I went there at the end of my junior year, and took as part of my program a course in optics under Professor Michelson. Although he was 67 years old, he was vigorous both mentally and physically, even playing tennis at the Quadrangle Club. He had a pedagogical system of his own. There was no final written examination; instead, every week he would interrupt his lectures to send some quivering student to the blackboard to explain a topic that he, Michelson, had previously presented. This system has much to be said in its favor. It insures that the students keep abreast of the lecture material and are clear in their exposition. I can remember that when Michelson felt the presentation was not lucid, he made some such remark as, "How would you expect a beginner to understand what you are saying?"

I have been asked to give an address of a half-hour's duration on "a philosophical and scientific subject", intelligible to a lay audience. This mandate is more than I can fulfill, and my talk will instead have a mainly historical theme but with, I hope, some philosophical and scientific overtones. This choice seems to me appropriate, since this is the first Michelson address, and not much longer will it be possible to have a speaker whose student days hark back to the time when Michelson was active in American physics and who has recollections of the scientific clime at that time.

I shall begin by trying to take stock of the state that physics was in in the early 1920's when I was a graduate student and how, in particular, our own country stood. As you all know, there have been two outstanding developments in theoretical physics in the twentieth century: one is relativity; the other, quantum mechanics. By the time I was a student, the so-called special theory of relativity stood on pretty firm ground and was generally accepted, although there were a few holdouts. The discovery of relativity differs from that of quantum mechanics in that it was an affair of a few men and a few papers. On the theoretical side, physicists immediately think of the paper in which the so-called Lorentz transformation was proposed in 1904, and the one in which Einstein read into it the concept of the relativity of space and time in 1905. On the experimental side, the Michelson-Morley experiment performed here in Cleveland in 1886-7 comes particularly to mind. In his biography of Michelson for the National Academy, Millikan refers to the Michelson-Morley experiment as "the most fundamentally significant experiment since the discovery of electromagnetic induction by Faraday in 1831. The special theory of relativity may be looked upon as essentially a generalization from it". In the nineteenth century there was no relativistic philosophy or mathematical framework, and the results were usually naively interpreted in terms of an ether drag. The advent in 1905 of the relativity theory of Einstein furnished a far more logical and less contrived interpretation.

In the early 1920's, the other new major theoretical development of the century besides relativ-

Albert A. Michelson, in whose memory the Case Board of Trustees has established a $5000 international award in science and engineering, was Case's first professor of physics and America's first Nobel laureate in science.

ity was that mysterious thing called the quantum. From the failure of classical physics to explain adequately radiation from hot bodies, Planck had suggested the idea of the quantum of action as early as 1900. The work of Thomson, Rutherford, and Millikan early in our century had established the nature of the elementary particles composing the atom as a heavy nucleus and a swarm of lighter electrons, somewhat like the sun surrounded by its planets. Bohr had proposed in 1913 his model of the atom which accounted for the Balmer series of hydrogen. The data one had to go on in evolving a theory of atomic structure were mainly of a rather limited type—the radiations emitted when the atoms were subject to rough treatment. Someone has said it was like trying to construct a model of a piano when all one knew about it was the sounds it emitted when dropped downstairs. Over the years, physicists have succeeded in this intriguing kind of detective work, but in the early 1920's the puzzle had not been solved. The prime effort in trying to clarify the mysteries of quantum theory was centered in Germany and Denmark. Our American journal, *The Physical Review,* was only so-so, especially in theory, and in 1922 I was greatly pleased that my doctor's thesis was accepted for publication by the *Philosophical Magazine* in England, as we all felt it would have many more readers. The American Physical Society was a comparatively small organization, with only 1400 members in 1921. The national secretary was your Professor Dayton C. Miller, and it was to him I communicated the abstract of my very first paper in that year. The papers at that time were sufficiently sparse that parallel sessions were not necessary, and only a small number of the communications were theoretical. Very few physicists in this country were trying to understand the current developments in quantum theory, but there were a few exceptions. One of them was a Case graduate, E. C. Kemble, later on the faculty of Harvard, and I am proud of the fact that I was his first PhD. The problem I worked on was trying to explain the binding energy of the helium atom by a model of crossed orbits which Kemble proposed independently of the great Danish physicist, Niels Bohr, who suggested it a little later. In those days the calculations of the orbits were made by means of classical mechanics, similar to what an astronomer uses in a three-body problem. The Physics Department at Harvard did not have any computing equipment of any sort, and to get the use of a small hand-cranked Monroe desk calculator, I had to go to the business school. I felt very blue when the results of my calculation did not agree with experiment. Similar negative results were also obtained independently by Kramers, and the failure of what is now called the "old quantum theory" to achieve the same success in helium that it did in the simpler case of hydrogen, a two- rather than three-body problem, was one of the debacles that led physicists to try a completely new tack.

Then in 1925, something wonderful happened. The real quantum mechanics was first discovered through two different routes—the matrix mechanics of Born and Heisenberg, and the wave mechanics of Schrödinger, the outgrowth of a highly imaginative earlier paper by de Broglie. Still another approach was developed by Dirac and by Jordan. I will not attempt to describe the difference between quantum and classical mechanics, except to emphasize one point: namely, that the true quantum mechanics which describes what is going on inside the atom is essentially statistical in character. This is epitomized in the so-called Heisenberg uncertainty principle which states that there are limits in principle to the precision with which one can measure simultaneously the position and velocity of a particle. If one knows *exactly* where it is, one cannot tell how fast it is going, and vice versa. One can measure both properties simultaneously with a certain degree of fuzziness, but there is always a lower bound to the product of the uncertainty in the two quantities. As a result, the outcome of an experiment on an individual particle in the atomic domain can be predicted only in a statistical way—like, say, the probability of twins, triplets, and quadruplets in a given human birth. In classical mechanics, one had a rigorous causality principle; if the position and velocity of a particle were known, then its future trajectory could be calculated rigorously. The tremendous philosophical import of the uncertainty principle and the opportunities it can provide for unbridled speculation are well described by quoting from an article which my colleague, the late Professor Bridgman, wrote for *Harper's Magazine* in 1929.* He said,

> "The revolution that now confronts us arises from the recent discovery of new facts, the only interpretation of which is that if we sufficiently extend our range we shall find that nature is intrinsically and in its elements neither understandable nor subject to law. . . . By far the most important effect of this revolution will not be on the scientist but on the man in the street. The immediate effect will be to let loose a veritable spree of licentious and debauched thinking. This will come from the refusal to take at its true value the statement that it is meaningless to penetrate much deeper than the electron, and will have the thesis that there is really a beyond, only that man with his present limitations is not fitted to enter his domain. . . . It will be made the substance of the soul; the spirits of the dead will populate it. God will lurk in its shadows; the principle of vital processes will have its seat here and it will be the medium of telepathic communication. One group will find in the failure of the physical law of cause and effect the solution of the age-long problem of the freedom of the will; and on the other hand the atheist will find the justification of his contention that chance runs the universe."

I will not myself try to answer the question as to whether nature is basically indeterministic, or whether behind the curtain it is really deterministic and the trouble is simply that the initial conditions are inevitably spoiled by the experiment so that the causality is lost to mankind. This is a question of metaphysics, not physics.

I should, however, emphasize the fact that the limits of error imposed by the uncertainty principle are far too small to appear in conventional experiments on bodies of observable size, so that it need not contradict one's physical intuition if one has a sufficiently pragmatic approach. It need not worry you in driving an automobile. However, the fact of the matter was that in the late 1920's the majority of the older theoretical physicists (Bohr and Born were exceptions) were greatly troubled by the probabilistic nature of the new quantum mechanics, and could never fully reconcile themselves to it. Einstein is reported to have remarked, "God doesn't throw dice." (Nevertheless, some might claim that God knew how they would land, but didn't let us in on the secret.) Schrödinger is known to have said that he wished he had never discovered his wave mechanics if it were going to lead to a statistical model of the universe.

All told, the years 1925-26-27 witnessed one of the greatest revolutions of all time in the history of physics. Historians of science in future generations will no doubt study assiduously the correspondence and other documents of the leading figures in this revolution, just as today they do those of Kepler, Newton, or Leibnitz. I might mention that to see that we preserve for posterity whatever documents are available, along with tape recordings of interviews with the still-surviving major contributors, a project to do just this was inaugurated three years ago under joint sponsorship of the American Physical Society and American Philosophical Society, with the financial backing of the National Science Foundation. Even though the 1920's may seem a minuscule time away from the usual historical standpoint, this program got off to a late start, for men like Schrödinger, Fermi, and von Neumann were no longer living at its inception, and Professor Kuhn, the director of the project, was able to have the benefit of only a few months' collaboration with Niels Bohr, who took a great interest in it before his untimely passing in November 1962.

After 1926 the rules of the game of atomic physics became known, so long as one did not seek to probe inside the nucleus and enter the realm of high-energy physics, which I am not discussing tonight. It is appropriate at this point to quote from a paper by Dirac written in 1929 which takes stock of the situation as of that date:

> "The general theory of quantum mechanics is now almost complete, the imperfections that still remain being in connection with the exact fitting in of the theory with relativity ideas. These give rise to difficulties only when high-speed particles are involved, and are therefore of no importance in the consideration of atomic and molecular structure, and ordinary chemical reactions. . . . The underlying laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known, and the difficulty is only that the exact solution of these laws leads to equations much too complicated to be soluble."

Since the correct equations to use for atomic and molecular structure were at long last known, it was natural that in the years following 1926

* Reprinted in *Reflections of a Physicist*, by P. W. Bridgman, Philosophical Library, New York, 1950 and 1955; see chapter entitled "The Recent Change of Attitude Toward the Law of Cause and Effect".

there would be a rash of papers applying these equations to various problems such as the frequencies of spectral lines, dielectric constants, magnetic susceptibilities, the chemical bond, etc. I remember Dirac's remarking that "anybody could write a paper in those days", and it is but natural that the most important contributions of nearly all theoretical physicists who were active in the decade following 1925 occurred in that period. The situation is admirably summarized in a remark once made to me by Ralph Kronig, a prominent young theoretical physicist in the first days of quantum mechanics and recently rector magnificus of the University of Delft, when he said: "I feel that those of us who experienced the early days of quantum mechanics lived through an orgy." Two or three years ago, a press conference was held with a number of scientists at Harvard by a representative of a prominent Eastern newspaper on the subject of Russian contributions to science over the past few decades. When my turn came to speak, I explained how, in Kronig's terms, there had been an "orgy" in theoretical physics in the last half of the 1920's and that there had been a limited Russian participation in it—namely, that one of the important early papers concerned with computational methods of application was by a Russian physicist, V. Fock. I noticed that the reporter took no notes on what I said, and decided he felt it was too trivial to mention. Actually, to my great embarrassment, I was quoted by his newspaper as saying that there had been a Russian orgy in theoretical physics. The orgy was certainly not of Russian nor, alas, of American origin. It centered around Western Europe almost entirely. I do not mean that American physicists did not make some important contributions, mainly experimental, in the years immediately preceding or contemporary with the discovery of quantum mechanics. Arthur Compton and C. J. Davisson won Nobel prizes for experiments showing respectively that light had some corpuscular aspects and matter some wave properties—both heresies from the classical standpoint. Still, the discovery of the new fundamental equations of quantum mechanics was primarily a European achievement. Then, fairly suddenly, at about the time these basic equations were established and many applications to specific problems were possible, America came of age in physics, for although we did not start the orgy of quantum mechanics, our young theorists joined it promptly. It is impossible to date the period when we came of age very accurately—or even to know whether to correlate it with the peak of the boom in the latter part of the Roaring Twenties or the depths of the depression of the early thirties, since each year our output was increasing. As early as 1926, an American theoretical physicist, Carl Eckart, wrote an elegant paper correlating the wave and matrix formulations of quantum mechanics. The new mechanics furnished the theoretical cement needed for the chemical bond, and the work of Mulliken, Pauling, and Slater in this connection won them worldwide attention. In my own area of magnetism, my group at the University of Wisconsin laid some of the foundations of the so-called crystalline or ligand field theory around 1932.

I should explain more explicitly what I mean by coming of age in physics: I mean a combination of quality and quantity, so that a country is in the front ranks in total productivity. In earlier days the United States had certainly from time to time produced a few outstanding geniuses, like Joseph Henry, Willard Gibbs, Rowland, Michelson, and Millikan, whose work brought America world renown. These men I regard rather as great pioneers or trailblazers whose voices crying in the wilderness made it possible for America to assume later a leading role in the realm of physics. Also, except for Gibbs, these men were all experimentalists; had our tradition in theory been the equal of what it was in experiment to round us out, we would have come of age earlier.

One measure of a country's prowess in science is the stature of its journals. By 1930 or so, the relative standings of *The Physical Review* and *Philosophical Magazine* were interchanged as compared with the earlier period that I have cited. Furthermore, the expanding pressure for publication caused the founding of new journals. Professor Tate started the *Reviews of Modern Physics* as a supplement to *The Physical Review* in 1929, and Harold Urey instituted the *Journal of Chemical Physics* in 1933. Prompt publication, beginning in 1929, of "Letters to the Editor" in *The Physical Review* and the interest which they attracted internationally, obviated the necessity of sending notes to *Nature,* a practice previously followed by our more eager authors.

If I have properly dated when America came of age in physics, and this is something that could be argued (some might put it a little earlier or a little later), it is interesting to compare when we reached this status in physics as compared with other disciplines. I think it is generally agreed that the United States became a world power politically at the end of World War I, about a decade earlier than in physics. If we look at the sciences, one might expect that we came of age much earlier in geology, as we did, since our mountains, mineral wealth, and vast territory furnished a natural laboratory. Perhaps this is what attracted the great Agassiz to America. In the nineteenth century, the frontier was not yet closed, and in a frontier country there is apt to be, partly through necessity, a particular emphasis on engineering and the applied sciences. In that century, our contributions to technology were indeed magnificent—Fulton and his steamboat, Eli Whitney and his cotton gin, Edison and his electric light, and, early in the present century, Langley, Wright, and the first aeroplane. By the same token one might expect that we would have gotten off to a later start in mathematics and astronomy than in theoretical physics, since mathematics is the purest and astronomy the least terrestrially practical of all the sciences; but the reverse is true. Early in the century, our country had international éclat in mathematics through the work of E. H. Moore and his colleagues at Chicago, and such figures elsewhere as Bôcher and Birkhoff. In astronomy at that time, the Yerkes, Mt. Wilson, and other observatories were world leaders. I am told that in chemistry we came of age at the end of World War I, if not before. In particular, isolation during the war period fostered research and development in the organic area. Prior to that time, the work on chemical thermodynamics by G. N. Lewis and his school at Berkeley, and on atomic weights at Harvard, was outstanding. So it seems that we reached maturity later in physics than in most of the other physical or earth sciences.

One can seek to explain just why America became of age when it did in physics, and when I try to do this, I am reminded of a conversation which my friend Professor Knaplund, a historian at the University of Wisconsin, told me he had with his colleague, Mikhail Rostovtzeff, later at Yale, who is generally regarded as the leading historian of our century on the subject of the Roman Empire. Knaplund has recently refreshed my memory by writing as follows: "One evening in 1925 when I called on the Rostovtzeffs, he exclaimed, 'Paul, it is finished' (referring to his massive *Social and Economic History of the Roman Empire*). I then asked, 'Tell me, Mikhail, what really caused the decline and fall of Rome?' To this, he replied, with strong emphasis, 'We do not know,' but then he launched upon a brief and brilliant discussion of the factors which precipitate the collapse of civilizations and empires. He assigned as a primary cause psychological influences which cannot be precisely defined."

In much the same vein, when I am now confronted with the question, what made American physics come of age when it did, I am tempted to say, "We do not know," and that here also there are psychological influences, but there are also other factors. The pioneer contributions of brilliant figures of earlier generations I have already alluded to. The fact that after the discovery of the new quantum mechanics in Europe we were all more or less starting from scratch with the applications probably helped, and especially the realization that the tools were now available for cracking problems galore of great importance in the atomic and molecular domain. Maybe the positivistic philosophy of the new quantum mechanics particularly appealed to the American mind, though I doubt it. At least this interpretation, in my opinion, makes more sense than the thesis advanced by a Yale philosopher that the abandonment of the causality principle in quantum mechanics was responsible for the rise of Marxism in Eastern Europe. You immediately ask whether the importation of distinguished European physicists was not the prime factor, but the point I want to make is that most of them had not arrived at the time we had begun to reach critical size. Although Uhlenbeck and Goudsmit moved from Holland to Michigan as early as 1927, Wigner and von Neumann did not come permanently until 1930, and Fermi, Bloch, Bethe, Teller, Einstein, and others were several years later, when the Machiavellian plans of Hitler and Mussolini were becoming increasingly clear. I would say that these distinguished Europeans were responsible, not for giving us maturity, but rather for carrying us still further to pre-eminence, at least at an earlier date then otherwise. Lord Hailsham to the contrary, American physics is not, in my opinion, parasitical. It is, however, true that American physicists benefited, especially in the late 1920's, from opportunities to study in Europe with Guggenheim fellowships and the like, and also from substantial visits to America of distinguished Europeans. In particular, the University of Michigan summer symposia, which assembled a bevy of stars from throughout the world, became a sort of Mecca for theoretical physicists. Also, psychological factors should not be overlooked. Somehow, in the 1920's America became interested in physics. Handsome new physics buildings arose on many campuses, such as Michigan, Minnesota, and Harvard. Of course, bricks and mortar do not insure an adequate staff, but usually there were corresponding new research positions or chairs. As evidence of the increased public interest in physics, I might mention that when Einstein spoke at a meeting of the American Association for the Advancement of Science in 1934, admission was by ticket only and some newspaper reporters broke through the windows to get in.

I will finally pass over only briefly the twenty-five years or so from the time America came of age in physics until the present day. Most of the chapters in this story are well known to you. The rise of Hitler and Mussolini sent to our shores many of the outstanding physicists of Europe. As you know, it was this group that was in large measure responsible for triggering the vast effort that culminated in the atomic bomb. The end of the war left us on the forefront of the realm of physics, as well as of politics. We had not been invaded, our laboratories were not destroyed, and in terms of scientific personnel we had skimmed some of the cream from Europe. In microwave physics, specifically, we were helped both by surplus equipment originally manufactured in connection with radar and by experience in the use thereof. Furthermore, the government, through the armed services, has been generous in helping with the great financial burden of modern research. It is small wonder that we were thus able to gain and hold our leading position. I would like to take this occasion, however, to pay tribute to the magnificent showing in physics which has been made by nations who emerged from the war under circumstances quite different from our own. Japan, a defeated nation in the Second World War, has in particular become in recent years a first-class power in physics. So has the Soviet Union, although it suffered terribly during the war years.

Physics at the present time is a large operation in almost any country. Journals continue to proliferate. Within the last month it was announced that *The Physical Review* would come out once a week instead of fortnightly. Where forty years ago I eagerly awaited the next issue of the *Zeitschrift für Physik,* which would reveal almost anything of significance in theoretical physics, there are now dozens of journals, in any of which there can be some excellent physics. One feels, although it is doubtless an overstatement, that it takes about as long now to codify and file the various preprints and journals which come into one's office as it did in early days to keep really abreast of the literature. The American Physical Society now numbers 20 000 members and about doubles every ten years. When one includes other countries, if this rate of growth is sustained, the number of physicists will exceed the world's population in two centuries. It is obvious that sometime before then, a leveling off must take place.

With such a state of affairs, it is inevitable that physics becomes increasingly compartmentalized with specialists who can't escape being somewhat vulnerable to the familiar charge of knowing more and more about less and less. Solid-state physics, for example, is a discipline in itself, with its own purely aesthetic satisfactions and its commercially important transistors, masers, and lasers. The field of molecular spectroscopy, so fashionable for physicists in the 1920's, has been largely abandoned to the chemists. And so it goes. Since there is more and more to learn, it is a good thing that our students are generally getting brighter and brighter, or perhaps trained in a more effective and accelerated fashion. One wonders how one would do oneself as a new graduate student. Similarly, one might try to compare present-day experimenters with those of Michelson's era. Modern experimenters seem to be extraordinarily conversant with theory, themselves developing much of the theory for the interpretation of their experiments, and leaving rather meager pickings for us poor theorists, like firemen on diesels. On the other hand, modern physicists do not have to construct nearly as much apparatus as did those of the early days. One gets catalogs and catalogs of companies that build such sophisticated devices as precision infrared spectrometers, Varian magnets, all kinds of oscilloscopes, a host of electrical-measuring or even nuclear-counting instruments, mass spectrometers, and such large machines as Van de Graaff generators, Collins liquefiers, and digital computers. Dr. Goudsmit, the managing editor for the American Physical Society, speaks of the earlier, pre-cyclotron times in physics as the string and sealing-wax days, and confesses nostalgia for them. However, when one looks at the Michelson interferometer designed over eighty years ago, one feels that a more apt comparison with modern apparatus is that of a finely jeweled miniature watch with a bulldozer. Even after this interval, the interferometer is still one of our most precise optical instruments. For example, at a conference on magnetism less than a month ago, I heard a description of especially accurate infrared measurements made with a Michelson interferometer. When one views the accomplishments of some of our great American pioneers in physics in the light of the environment in which they lived, and how much they nevertheless achieved, one can only conclude that there were giants in those days, though very few in number, and certainly Michelson was one of them.