Looking back on books and other guides

PHYSICS TODAY asked US scientists who have won Nobel Prizes in physics or for work close to physics to comment on what books—or what discoveries, people or issues—of the last 50 years have most decisively influenced them. The following are the responses that we received by our press date, arranged by date of Prize.

Carl D. Anderson

In doing scientific research it is, of course, important to start off on the right track.

My thesis work at the California Institute of Technology (1927–30) consisted of a cloud chamber study of the space distribution of photoelectrons produced by x rays. During this period, C. Y. Chao, in a room close to mine, was studying the absorption and scattering of ThC″ gamma rays in lead using an electroscope. He found an excess in both the absorption and scattering of the gamma rays over that predicted by the Klein–Nishina formula. His results interested me greatly and I decided I wanted to build a cloud chamber operated in a magnetic field to study in detail the gamma-ray-produced secondary electrons ejected from a lead plate inserted inside the cloud chamber.

When it was clear that I was to receive my PhD in 1930, I made an appointment with Robert A. Millikan to see if I could spend another year at Caltech to carry out this experiment. He informed me that I had had both my undergraduate and graduate work at Caltech, was very provincial and should apply for a National Research Council fellowship and carry out the experiment elsewhere.

Having no choice but to apply for a fellowship, I wrote to A. H. Compton, who cordially invited me to spend a year at Chicago to carry out my experiment. As time went on, a sojourn at the University of Chicago appealed to me more and more.

One day Millikan summoned me to his office and said he wanted me to spend one more year at Caltech to build a cloud chamber in a magnet field to measure the energies of the electrons present in cosmic rays. (Although primitive studies of the penetrating radiation or "ultrastrahlung" had been carried out in Europe, he was the first American physicist, I believe, to study them, beginning in the early 1920s. He proved they were of cosmic origin and named them *cosmic rays*.) Since by then I preferred Chicago, I used all the arguments he had previously used as to why I should not stay on at Caltech. He said these arguments were valid, but that my chance of receiving an NRC fellowship would be much enhanced after an additional year at Caltech. He was a member of the selection committee at the time.

So, again without any choice, I immediately set to work on the design and construction of the cosmic ray magnet, which would require a very powerful magnetic field because the cosmic ray electrons were expected to have energies in the range of several hundred MeV or more, rather than about 1 or 2 MeV for the ThC″ experiment.

I am firmly convinced that had I been able to carry out the ThC″ experiment, the positron would have been discovered at an earlier date than its actual discovery, since at least 10% of the electrons emerging from the lead plate would have had a positive charge, would have stuck out like an equal number of sore thumbs and could not possibly have escaped detection.

The magnet cloud chamber to measure cosmic rays was completed and first operated in October 1931. The early pictures were wholly astounding, as an equal number of high-energy positive and negative particles were observed rather than only electrons, produced in Compton collisions, as one would expect. I am sure that Millikan expected to see only electrons, because of his firm belief at that time that the primary rays consisted of high-energy photons explained by his atom building hypothesis.

Millikan was in Europe at the time, and in November 1931 I sent him a set of photographs. My own excitement was clear from the concluding sentence of the accompanying letter: "It promises to be a fruitful field and no doubt much information of a very fundamental character will come out of it."

This promise, later fulfilled, clearly shows the difference between Millikan's choice of a magnet cloud chamber and my own. If I had built my magnet cloud chamber, I have no doubt it would have discovered the positron in a very direct way—but it would have been only a *one-shot* piece of equipment. However, I built Millikan's idea of a magnet cloud chamber—which proved to be a *multi-shot repeater*.

First I discovered the positron with it in 1932, but in a somewhat roundabout way. Then, Seth Neddermeyer and I discovered the first mesons with it in 1937—this time necessarily in a roundabout way, consuming some four years in pursuing many puzzling clues and resolving tricky paradoxes.

Although this magnet cloud chamber was the first to be used in cosmic ray studies, many others were built in differ-

ent parts of the world, including P. M. S. Blackett's in the early 1930s, which was the first Geiger-counter-controlled cloud chamber.

After several years of active work on studying the properties of hyperons and heavy mesons, we found cosmic rays could not compete with the large accelerators, and our cosmic-ray work gradually came to a halt.

I wish to mention one of Millikan's remarkable characteristics—his uncanny ability to sense out, in the very early stages, nascent fields of physics that were destined to become large and extremely important areas of research. This may even be said about his own distinguished personal research. I had an opportunity one day to ask Dr. Millikan how he could detect so many new fields of physics before most other people recognized their importance. His immediate and apparently serious reply was: "Oh, I read *Science Abstracts*." Well, this explanation was of absolutely no help to me—for I, too, read *Science Abstracts*.

Linus Pauling

In September 1922 I became a graduate student in chemistry at the California Institute of Technology; in June 1925 I received a PhD degree with a major in chemistry and minors in physics and mathematics. It is clear that my activities during this three-year period pointed me toward the course that I have followed ever since. Among my teachers were several very able men, including Harry Bateman, with whom I studied vector analysis, functions of a complex variable, Newtonian potential theory and integral equations. The two who had the greatest impact on me where Roscoe Gilkey Dickinson and Richard Chace Tolman.

Dickinson, who was seven years older than I and who was the first person to obtain a PhD degree from the California Institute of Technology (in 1920), introduced me to scientific research. He had a remarkably logical and critical mind. He showed me how the structure of a crystal might be determined by a process of reasoning based upon the experimental information provided by the x-ray diffraction pattern. At each step in the process he made an assumption about reliability and completeness of the experimental information, and then, on the basis of this assumption, he rigorously eliminated certain sets of possible structures for the crystal, until finally, in some cases, only one structure was left. During the years following 1920 this experimental technique, providing a great amount of reliable and precise information about the interaction of atoms with one another in crystals, laid the basis for the development of modern structural chemistry.

Tolman, just twenty years older than I, had come to Pasadena as professor of chemistry and physics. From him I learned a great deal about thermodynamics, statistical mechanics, quantum theory, and the electronic structure of atoms and molecules. Each year he led a seminar devoted to the study of quantum theory and the structure of atoms. The book that we used 1922–23 was *The Origin of Spectra* by Paul D. Foote and F. L. Mohler, both physicists with the US Bureau of Standards. This book was published in 1922 by the Chemical Catalog Company. It contained a discussion of the Bohr theory of the hydrogen atom and of energy levels and the frequency of the spectral lines corresponding to the transition from one energy level to another, of ionization and resonance potentials, x-ray spectra and thermochemical relations.

In October 1923 I bought a copy of the English translation, *Atomic Structure and Spectral Lines*, of the third edition of the book *Atombau und Spektrallinien* by Arnold Sommerfeld for use in the course, and in October 1924 I obtained a copy of the fourth edition, in German, which was the textbook for that year. The Sommerfeld books were far more complete (626 pages and 861 pages, respectively) than the Foote and Mohler book (250 pages), but I was not able to get a good understanding from them of the nature of line spectra and the structure of atoms, because these subjects

had not yet been well developed. The principal value of these books lay in their providing a good background of information about quantum theory and about the structure of the hydrogen atom. For example, one of the two systems of energy levels of helium that do not combine with one another with emission of spectral lines was assigned to parahelium and was described as consisting of single lines, whereas the other, orthohelium, was said to give rise to a system of doublets. It was not until 1927, when Werner Heisenberg explained the helium spectrum, that it was recognized that the orthohelium spectrum is a triplet spectrum rather than a doublet spectrum. Sommerfeld had split the azimuthal quantum number into two quantum numbers, which he called the inner and the outer azimuthal quantum numbers. In 1923 he described this introduction of two azimuthal quantum numbers as "a new and fundamental step." While he did not indicate that he was puzzled by a description of an elliptical orbit for an electron that involved two different values for the eccentricity, by 1924 he wrote that this matter had not yet been completely clarified. I do not remember that I noticed this admission in 1924; instead, I think that I attributed my failure to have developed a satisfying picture of the electronic structure of atoms to my own inability to understand what Sommerfeld had done rather than to a defect in the theory.

The whole matter was, of course, cleared up when the spin of the electron was discovered, which permitted the angular momentum quantum number to assume two values. In fact, by the spring of 1927, when I spent a few weeks in the Bohr Institute in Copenhagen collaborating with Samuel Goudsmit in developing the theory of the hyperfine structure of spectral lines in relation to the magnetic moments of nuclei, I had become so well satisfied with the theory of the electronic structure of atoms and the nature of atomic energy levels as to continue the collaboration with him in producing our 1930 book *The Structure of Line Spectra*.

Much of my work during the last 59 years has dealt with the structure of molecules. I have been fortunate to live through the period during which the content of this field has changed from structural formulas for molecules that chemists had learned to write 100 years ago to the detailed information we now have about the precise ways in which atoms are arranged relative to one another in molecules and crystals. In the 1922 book by Foote and Mohler there is a discussion of the structure of the hydrogen molecule. Bohr had suggested that in the normal state of this molecule the two electrons move in circular orbits with one Bohr unit of angular momentum in planes at an angle of 120° with one another. Foote and Mohler point out that this structure is unacceptable because it corresponds to paramagnetism rather than to diamagnetism and leads to an incorrect value of the bond energy. The quantum mechanical theory of the chemical bond began in 1927 with the publication by Carl Burrau of his calculation of the electronic structure of the hydrogen molecule ion and the calculations by E. U. Condon and by W. Heitler and F. London on the electronic structure of the hydrogen molecule.

Walter H. Brattain

In my studies, the professors that really taught me were Benjamin H. Brown (physics), Walter A. Bratton (math) at Whitman College; W. E. Milne (math) at the University of Oregon; and finally John T. Tate, J. H. Van Vleck and Joseph Valasek, all in physics at the University of Minnesota. All of this was excellent teaching by lecturing because there were no good books at that time.

When I went to Bell Laboratories (1929) the work I did was not to be found in books. Arnold J. W. Sommerfeld had explained the behavior of electrons in metals using Fermi–Dirac statistics in germanic language (1928). Sommerfeld was in the United States in

1931 and gave a course on electron theory of metals at the University of Michigan to which I went. I did look at papers on physics of the type that I was working on. The very few that were worth it included ones by A. H. Wilson leading to the concept of energy bands in solids and by N. F. Mott, W. Schottky and B. Davydou working out a theory for the surface of a semiconductor in contact with other conductors.

After the Second World War was over a group started with John Bardeen, William Shockley, Gerald T. Pearson, Robert B. Gibney, and myself. Pearson and I were the only ones of the group that had long experience with semiconductors. We soon found out that there were four or five things that did not fit with the Mott group theories. It was Bardeen who saw that the surface of the semiconductor was not the same as the inside, and this agreed with our results. The finding gave me a chance to try different things, which ended with Bardeen and me finally running into a result we did not at all expect. None of us had any idea of this until it popped up. It was the effect that made it possible for our semiconductors to both amplify and oscillate. We named the effect and what it could do, a transistor. It was the single discovery that has had the greatest impact on the work I have done.

Emilo Segrè

If I think of the books that have been important for my scientific development, I must start from the earliest: A. Ganot, *Treatise on Physics*, Italian translation of 1863, which I found when I was about 10 years old among the books of an uncle. It was a high school text, very well done, in which Michael Faraday was treated as Mr. Faraday because he was still alive. In 1916 my uncle, seeing my interest in the book, gave me a more recent edition, inscribing it with the wish that "physics would soon serve the arts of peace." This happened during the first world war.

When I decided to study classical physics systematically, around 1927, I went through the five volumes of Max Planck's *Introduction to Theoretical Physics*. It was a work already a little dated, but sound. Later, more modern books covering the same subject appeared, but I studied on Planck. At that time, of course, atomic physics was at the center of interest, also for me, and here I studied thoroughly the *Introduzione alla Fisica Atomica* by Enrico Fermi. In parallel I read Arnold Sommerfelds *Atombau*, Schrödingers memoirs and the various classics of quantum mechanics.

By then I was doing research and my work horse was the *Handbuch der Physik* of Hans Geiger and Karl Scheel. Later, Hans Bethe's articles on nuclear physics in *Reviews of Modern Physics* were my text, as they were for many other physicists of my age.

It will be noted how much German I read: Planck, Sommerfeld, *Handbuch der Physik*, as well as most of quantum mechanics were in that language. Each of these books had its importance for me at the time. They helped to form my taste and to give me a background. The list of course is incomplete. I should add also several chemistry and mathematics books such as Richard Courant and David Hilbert's *Methods of Mathematical Physics*. They all contributed vitally to my scientific formation. However, the personal contacts I had with some of the authors who were my contemporaries, the studying of single papers, the persuing of numerical data, had even greater importance.

Robert Hofstadter

P. A. M. Dirac's book on quantum mechanics had the greatest influence on my thinking about physics. This book presented to me a new conception of what the physical theory of quantum mechanics really was all about in the period when I was learning the subject. Prior to reading Dirac's book on quantum mechanics, a subject which was really only about ten years old when I was introduced to it, my understanding of this

field was very murky. I perceived it as a series of *ad hoc* assumptions poorly put together. This book showed me that physics can be a clearly, and even beautifully, thought out subject reaching into the experimental side of the field for the verification that it needs.

The person who had the greatest influence on my career was Edward U. Condon, with whom I worked as a graduate student at Princeton in 1936 and with whom I wanted to do a theoretical thesis in atomic physics. At about this time Condon decided to leave Princeton for work at Westinghouse in Pittsburgh, and there was no way I could continue my graduate work with him. Because of his interest in infrared physics at that time, he suggested that I work in this field at Princeton with R. B. Barnes, who was attempting to verify Condon's predictions. Somewhat reluctantly, I agreed to do experimental work in infrared physics and actually continued on to do a thesis in this field. Barnes left Princeton before I finished my work, and for a while I worked alongside L. G. Bonner, an NRC fellow at Princeton. Subsequently I obtained my degree by independent work, without an adviser, in infrared spectroscopy. Thus I became an experimentalist. I owe this transformation to Condon. I still can't say I am entirely happy about the transition because I really wanted to be a theoretical physicist. On the other hand, experimental physics offered me an opportunity to get at the roots of physics, which I enjoyed all during my career.

On the matter of a thing, I can say that the NaI(Tl) scintillator, which I discovered in 1948, has had the greatest influence on my career. A bit later, J. A. McIntyre, who was my graduate student at Princeton in 1950, and I discovered that gamma ray spectroscopy could be carried out with well-formed crystals of NaI (Tl). We had a tremendous experience together in seeing the gamma ray lines of various radioactive species reveal themselves for the first time. Subsequently my colleagues at Stanford, particularly E. B. Hughes, and I used NaI (Tl) in a number of interesting configurations to study many high-energy and particle physics problems at accelerators. We also hope to employ this material in an experiment in gamma ray astronomy, a field that seems to me to have much promise.

Charles H. Townes

The effect of a book depends both on its innate qualities and on the context in which it is encountered; as a result the books that have impact on a career or are particularly memorable can be quite varied. This has been the case for me.

As an undergraduate during the first half of the 30s, I was fortunate that George Jauncey's *Modern Physics* introduced me to the ideas, personalities and excitement of modern physics. I also profited from review articles on the new field of nuclear physics written by Karl Darrow with his characteristic charm and care for language and published in the *Bell System Technical Journal*. The *BSTJ* may seem an unlikely source of inspiration for the uninitiated, but it was donated by "The System" to my local public library and at the time provided my only contact with ongoing discoveries in physics. I also read somewhere about Karl Jansky's strange discovery of radio waves emanating from the galaxy. (I had thought it was in Jauncey's book until I recently failed to find it there)—a discovery that immediately became imprinted in my mind and whose import has enticed me back several times to research in radio astronomy.

There are certain texts over which I spent many hours as a graduate student and which have continued for a long time to be useful references. These include such classics as E. T. Whittaker and G. N. Watson's *Modern Analysis* and William Smythe's *Static and Dynamic Electricity*. As Smythe's graduate student, I profited from being asked to perform some midwifely services for production of his book. A carefully developed and systematic treatment of almost any major field, when completely familiarized, I find invaluable. The unpublished notes of William V. Houston's course in quantum mechanics were my

real introduction to the field and stayed with me a long time. Houston's treatment was clear and penetrating; my memory of his exposition of stimulated and spontaneous transitions in an electromagnetic field, perhaps along with an interest in classical electromagnetic waves, made an easy bridge between classical and quantum wave phenomena that, at least during the early days of quantum electronics, seemed not so easy for all physicists.

Certain books of great use to me were encountered only during the process of research. A systematic seminar on Linus Pauling's book *The Nature of the Chemical Bond* with about ten of the brightest individuals then at Bell Telephone Laboratories permanently oriented my thinking towards molecular phenomena. Gerhard Herzberg's series of books on molecular spectra have ever since been my steady companions as has, over a more limited period, Edward Condon and George Shortley's epoch-making *Theory of Atomic Spectra*.

I owe special and peculiar thanks to two unpublished manuscripts of John Van Vleck. One was a memorandum of limited circulation during the early 1940s on the Kramers–Kronig relation, written at a time when it was scarcely known to physicists and in a way I found beautiful, inspiring and useful. The other was Van Vleck's unpublished memorandum on the theory of water vapor absorption in the atmosphere, written during the latter half of World War II to clarify the possible effects of such absorption on radar. This memorandum both convinced me that the radar then under intensive development at 1.25 micron wavelength was inappropriate, and also, in combination with Van Vleck and Victor Weisskopf's paper on lineshapes, helped me envisage the broad and exciting field of microwave spectroscopy to which this wavelength provided a natural beginning.

Thus, when I had an opportunity to really initiate my own research career at the end of World War II, I was pulled in two directions. One was toward microwave spectroscopy, stimulated by Van Vleck's work and my experience with radar; the other toward radio astronomy, stimulated by my undergraduate memory of Jansky's discovery. First I did a little of both; then laboratory microwave spectroscopy rather completely claimed my attention for a while, only to be superceded more recently (after a substantial interim period with quantum electronics) by radio and infrared astronomy, which combine a bit of everything. In changing to a new field whose details are not at one's fingertips, nothing can be more useful than a careful compendium of pertinent information and references. Clabon Allen's excellent *Astrophysical Quantities* has been invaluable in this respect. This and Herzberg's series on molecular spectra are examples of an all-too-rare form of literature that greatly facilitates the building of new science on the old by evaluating critically a multitude of research results and putting the best of what is known into a compact system.

Hannes Alfvén

During the past 50 years—which happens to coincide with my time as an active scientist—there have been many important achievements, but these must be seen as parts of great team works. From my point of view they group themselves together into the beginning of two new "ages": the space age and the nuclear age.

▶ From a philosophical-scientific point of view the space age has focused our attention on the cosmic phenomena that have made it possible for humans to appear: the "big creation" (if there was a creation!), which is treated in cosmology, and the "small creation," the somewhat more technical problem of how our solar system, including our Earth, was formed and what made it possible for ourselves, a product of the cosmic forces, to dare to explore these.

What has attracted most interest is that from spacecraft we can observe our large-scale environment over the whole electromagnetic spectrum, from radio waves to gamma rays. However, observation and understanding are not synonyms. A space age result, at least equally important, is that *in situ* measurements in the magnetospheres and heliosphere have shown that the properties of the space medium are drastically different from what they were generally believed to be less than five or ten years ago. This is now starting a dramatic revision of generally accepted theories, which is likely to cause a breakdown of the old "paradigm" of space physics. My prediction is that the new paradigm will not leave much of the present theories unchanged 10 years from now. This applies also to the "big bang" hypothesis and the Laplacean solar nebula concept of the formation of the solar system.

▶ The nuclear age started with the Manhattan Project. At the same time it becomes possible to get a better understanding of how we originated we begin to understand how we will end. Also the image of the scientist is changing. Up to Manhattan we were often regarded as semigods, giving humanity a *Schlaraffenland* that earlier epochs could only dream of. Now we are increasingly regarded as devils—often without "semi"—who daily invent more cruel ways of killing people, indeed torturing innocent people to the terrible radioactive death. And every scientist who really analyzes the role we play must admit that to some extent we deserve this image. More than half of us are working for destruction. Many of us do what we can to fuel the hatred that is a necessary background for getting increased funds for devising still more horrible arms. In the history of humanity there has been no arms race in any way similar to the present one that has not exploded into a war. It is difficult to avoid the prediction that the nuclear arms race is likely to do the same within less than 10 years. This war will be "omnicide," everything will be ended, including all science.

Perhaps there is still a possibility to avoid this, but only if we as a group understand the consequences of what we are doing. This requires that we are only intelligent but also wise. Are we?

Ivar Giaever

"Should I write about Feynman?" I asked my friend. "What for?" he said, "to make Feynman famous?" It is difficult to say what for except that I really love the three big red books.

I had the fortune (misfortune?) of being exposed to physics for the first time when I was 29 years old. I found it frustrating because I could not find any books I liked. To my complaining, my friend said, "What did you expect, a graduate physics book written for Norwegian engineers?" I mumbled something intelligent. "In the States," he continued, "we learn physics many times. Because it is regurgitated so often, in graduate school we concentrate on special points, and sometimes rather small ones at that." Not much comfort to someone who did not know Planck's constant—not only the value but also the concept—and who marveled that such famous physicists as Werner Heisenberg were still alive (I think).

Unfortunately, the *Feynman Lectures* were not published soon enough to help me out of my misery. However, when they appeared I slept with them under my pillow. (For this purpose, the paperback version is recommended.) I confess I have still not read every word in the books (probably neither has Feynman), but I have many favorite places.

Take, for example, Feynman's delightful description of electrical power technology: "Out of Boulder Dam come a few dozen rods of copper perhaps the thickness of your wrists that go for hundreds of miles in all directions. Small rods of copper carrying the power of a giant river. . . ." For someone interested in applied physics, it did not seem the average small point!

Or, approximately in the middle of the second volume, he has collected *all* the equations of classical physics on *half* a page. How pleasurable to have all that knowledge distilled into a few equations, to know and understand them or at least have them look vaguely familiar. A little later in the book he shows that all of physics can be condensed into a master equation

$$U = 0$$

(here left as an exercise for the serious student). Suddenly some limits of condensing physics with equations occur to the reader.

And this brings me to the point: *The Feynman Lectures on Physics* contain practically all the physics needed to do industrial research very well. My old physics teacher used to lament the fact that today's graduate students are experts in applying the Green's function method but get stumped if you ask them what the pressure is at the bottom of a swimming pool. There are lots of swimming pools in the application of physics. If you want to join me for a swim, you can float on the *Feynman Lectures*.

Samuel C. C. Ting

Although I was born in the US, my family returned to China when I was three months old; we moved to Taiwan in 1948, where I began my formal studies. When I returned to the US in 1956 I had practically no working knowledge of English. In my first year at the University of Michigan I enrolled as an engineering student and took the average courses expected of the first year student. During the beginning of my second year I made a request that the University allow me to take some more advanced courses in mathematics, physics, and physical chemistry. I was told that doing this required my leaving the engineering department and joining the physics department. I had also been influenced by the book *Atomic Spectra and Atomic Structure* by Gerhard Herzberg, which acted as a catalyst in my decision to concentrate my studies in physics. I left the engineering department and became a physicist. At the University I had the opportunity to think through physics very carefully and understand the implications of physical phenomena beyond what was described in lectures and most textbooks. For matters I did not understand I never hesitated to ask questions. Despite my lack of facility with the English language I asked more questions than the majority of the other students. I believe that trying to pursue a subject to a deeper level beyond the textbooks, being able to independently think out the various physical phenomena, and not being afraid to ask questions in the face of overwhelming odds are very important to a scientist.

After receiving my PhD degree I accepted a position that enabled me to pursue further studies at the European Organization for Nuclear Research (CERN) in Geneva. At CERN I had the good fortune of meeting G. Cocconi. His style in choosing physics topics has had a profound impact on me. He has opened many fields of important research.

After one year at CERN I accepted a position at Columbia University as an instructor in the physics department. Columbia was, at that time, the best university in which to study physics. There were many distinguished and accomplished physicists and Nobel Laureates there such as I. I. Rabi, Polykarp Kusch and T. D. Lee. They all had great taste in physics and had strong views and commitments to physics. The experience I gained during my year at CERN and two years at Columbia formed a strong influence on my future work.

In recent years I have been fortunate to have a collaboration with excellent physicists from many different countries (Italy, the US, France, Germany, England, Holland, Pakistan, Japan and China). Together we have pursued research on two subjects: new particles and, perhaps more important, in an experimental way, the unification of all four kinds of forces in nature (gravitational, electromagnetic, nuclear and weak). Finding a grand synthesis of forces in nature has been an age-old dream for scientists that can be traced from the ancient Greek and Chinese philosophers to the great scientists of a more recent time, like Einstein and Maxwell. What we are attempting to do experimentally is first to see a link between weak and electromagnetic forces that according to our present understanding may even be attainable in the next few years.

Rosalyn Yalow

In 1946 after the Manhattan Project was supplanted by the Atomic Energy Commission, radioisotopes from the Oak Ridge nuclear reactor were made readily and inexpensively available to civilian investigators. Nuclear medicine, the use of artificial radioisotopes in diagnosis, therapy and biomedical investigation, can be said to have begun with this event. Before World War II there had been a few pioneering studies in the United States with cyclotron-produced radioisotopes. Numbered among the early pioneers were physicians such as J. H. Lawrence, J. G. Hamilton, M. H. Soley, using material from Ernest Lawrence's cyclotron, and S. Hertz and A. Roberts, using radioisotopes which Robley Evans provided from the MIT cyclotron. Yet, the parent of nuclear medicine can be said to be George Hevesy, who had received the Nobel Prize in chemistry in 1943 for his use of radioisotopes as tracers in the study of chemical processes. In 1923, Hevesy had used radium D (Pb^{210}) and thorium-B (Pb^{212}) to show the uptake and removal of lead in bean seedlings and a year later studied the distribution of lead and bismuth in animals using the naturally occurring radioisotopes as tracers. When artificial radioisotopes became available a decade later he rapidly extended his studies with these materials.

Radioactive Indicators, published just as radioisotopes became readily available, was a remarkable volume in its description of the production and detection of radioisotopes and in particular in its citation and analysis of thousands of experiments. It revealed how a new understanding of animal physiology was made possible with radiolabeled tracers. I read this book soon after I joined the radiotherapy department at the Bronx Veterans Administration Hospital in 1947 to initiate a radioisotope service. Initially, it appeared as if the newly available radioisotopes would primarily be used to replace radium for the treatment of cancer, a concept consistent with our location in a radiotheraphy service. However, this book introduced me to the remarkable diversity of applications of radioisotopic tracers in physiology. My work since has been involved in the development and application of such methodology in diagnosis and biomedical investigation. Radioimmunoassay, the major thrust of my work, is different from but has much in common with the radioisotopic dilution technique that George Hevesy pioneered and which was so admirably presented in *Radioactive Indicators*.

Arno A. Penzias

The crucial influence upon my career as a physicist came from being forced to flee Nazi Germany in 1939. Stripped of the family resources that would most probably have led me into a comfortable life in my father's business, we instead began a new life in America.

It was taken for granted that I would go to college, studying science, presumably chemistry, the only science we knew much about. "College" meant City College of New York, a municipally supported institution then beginning its second century of moving the children of New York's immigrant poor into the American middle class. I discovered physics in my freshman year and switched my major from chemical engineering. After graduation, marriage and two years in the US Army Signal Corps I applied to Columbia University in the Fall of 1956. My army experience helped me get a research assistantship in the Columbia Radiation Laboratory, then heavily involved in microwave physics, under I. I. Rabi, P. Kusch and C. H. Townes. After a painful, but largely successful struggle with courses and qualifying exams, I did a radio astronomy thesis with Townes.

In 1961, with my thesis complete, I went in search of a temporary job at Bell Laboratories, Holmdel. Their unique facilities made it an ideal place to finish the observations I had begun during my thesis work. "Why not take a permanent job? You can always quit," was the advice of Rudi Kompfner, then Director of the Radio Research Laboratory. I took his advice, and have remained here ever since, doing basic and applied physics in an atmosphere that stimulates excellence in both.

My personal experiences differ only in their details from those of countless other refugees. From distinguished scientists in senior academic positions to frightened preschool children, three generations of "genetically undesirable" physicists were uprooted from the Europe of the 1930s and resettled in America. Their role in shaping American physics in the intervening four decades is an inseparable part of the historic half-century of our science that we are celebrating. □