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JOHN RAY DUNNING

1907—1975

A Biographical Memoir by

HERBERT L. ANDERSON

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September 24, 1907–August 25, 1975

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JOHN RAY DUNNING, professor of physics at Columbia University, was a pioneer in the development of nuclear energy. From 1932, when he was twenty-five, he worked almost exclusively on the study of the then newly discovered neutron. His work led naturally to the demonstration—the first in the United States—of the large release of energy in the fission of uranium by slow neutron bombardment.

Dunning realized that by enriching uranium in the light isotope, he could make a nuclear chain reaction a practicality. His work to adapt the gaseous diffusion process for this purpose made possible the nuclear power industry as we know it today. This achievement, pursued with unique vigor and single-mindedness, places him in the ranks of outstanding scientists of this century.

After leaving active research, Dunning served with great distinction as dean of the School of Engineering at Columbia, obtaining financial support for many scientific projects.

FAMILY BACKGROUND

John Ray Dunning was born in Shelby, Nebraska, the son of Albert Chester and Josephine (Thelen) Dunning, on September 24, 1907. His father was—according to Dunning himself, quoted in *Current Biography,* 1948—a "congenial, en-

ergetic, and hearty grain dealer." He was also an amateur radio engineer. John's early conviction that it was "easier to make equipment work . . . than to save souls or prepare legal briefs" turned him away from the ministry and the law and led him to science. He was only twelve years old when he built and then operated a radio sending set, the first in his section of the country. After graduating from Shelby High School in 1925, he entered Nebraska Wesleyan University, and, in 1929, received a B.A. degree with highest honors. Between 1926 and 1929, he and his father, with the encouragement and assistance of one of his professors, built the radio stations WCAJ and KGBY, which operated on the regular broadcast bands and were later sold. Immediately after graduation, Dunning went to Columbia University, where he was an assistant in the physics department for three years and a university fellow from 1932 to 1933.

Dunning was married in 1930 to Esther Laura Blevins, now dead, who was his devoted companion throughout his lifetime. He was elected to the National Academy of Sciences in 1948. He died of a heart attack at his home in Key Biscayne, Florida, on August 23, 1975. He was sixty-seven years old. Two children, John Ray, Jr., and Ann Adele (the former Mrs. Edward Coyle), and a grandchild survive.

NEUTRON RESEARCH

The neutron, discovered shortly after Dunning arrived at Columbia, became his principal subject of research. This work was supported enthusiastically by George B. Pegram, who had resigned his post as dean of engineering to do research. Their collaboration was both close and productive, and they published twenty-four papers together on neutrons between 1933 and 1936. Dunning's drive and exceptional skill "in making things work" contributed greatly to their

joint success. One 1934 paper, "The Emission and Scattering of Neutrons," became the basis of his Ph.D. dissertation.

Dunning spent his entire career at Columbia. He was appointed to the faculty as an instructor in 1933, received his Ph.D. in 1934, and advanced to assistant professor in the following year. He became associate professor in 1938 and professor in 1946.

Granted a Cutting Traveling Fellowship in 1936, Dunning traveled extensively in Europe, taking advantage of the opportunity to meet with many distinguished physicists—among them Rutherford, Chadwick, Bohr, Heisenberg, and Fermi—to discuss his work on neutrons.

After his 1935 promotion to assistant professor, Dunning became the central figure in neutron research at Columbia. His was the leading laboratory for neutron research in the United States, complementing Fermi's laboratory in Rome. Segrè, Amaldi, Rasetti, and Fermi himself came to Columbia to work with Dunning and his colleagues. He also worked with a procession of graduate students and younger faculty members, studying, among other topics, the magnetic properties and magnetic moment of the neutron. One experiment of fundamental importance, the scattering of neutrons by ortho- and para-hydrogen, was done in collaboration with a group from the National Bureau of Standards.

PERSONALITY

What kind of a man was John Dunning? As one of his former graduate students, William W. Havens, Jr., put it:

> Dunning was a man of contagious optimism, boundless enthusiasm, and almost infinite energy. He was also an inspired experimentalist who knew intuitively the critical factors in a scientific problem. He had a real flair for getting apparatus to work. On many occasions, his graduate students would give up in despair when one of Dunning's electronic devices would not function. Dunning could then be found in the laboratory at 2:00 or

3:00 A.M. fiddling with the apparatus and by dawn it was usually working perfectly. His colleagues jokingly referred to the 'DOF' or 'Dunning Optimism Factor' when planning any project because Dunning always minimized the difficulties and emphasized the accomplishments. However, all admired the ingenuity, enthusiasm, and inspiration he contributed to any project.

My own view is very much in accord with this. Dunning had a deep conviction that, unless fundamental principles were being violated, the apparatus had to work. It was just a matter of getting it to do what it was supposed to do anyway.

CYCLOTRON

In the early days, before accelerators were common, a mixture of beryllium powder and radon gas contained in a small glass bulb was used as a neutron source. Such sources had a yield of 10^6 neutrons per second. The radon was obtained from Memorial Hospital by "milking" four grams of radium every few days for this decay product (half-life = 3.8 days). The radon was used primarily in gold seeds for implantation in cancerous tumors, but there was plenty available for the neutron work.

Still, Dunning followed the news of Ernest Lawrence's successful development of the cyclotron at Berkeley with great interest. He wanted a much more powerful neutron source than he had at his disposal, and the cyclotron was the way to go. When he heard of an 80-ton magnet like that Lawrence had used in the construction of his 37-inch cyclotron, he went after it. These magnets had been produced by the Federal Telegraph Company during World War I to be used in Poulsen arc generators, a type of radio transmission that became obsolete after the invention of the vacuum tube.

In the 1930s, no government funds were available for such a project and universities measured their budgets for research in the hundreds of dollars. But Dunning was un-

daunted. His energy, enthusiasm, and self-confidence were persuasive, and he went around raising money from foundations and obtaining gifts of equipment from industry until the magnet was shipped and installed and a cyclotron built in the basement of the Pupin Physics Laboratory at Columbia.

Dunning worked with a small staff. Dr. E. T. Booth, his long-time collaborator and a postdoctoral fellow at the time, worked full time constructing the cyclotron and getting it to work. My own recollections are vivid of Booth, infinitely patient, looking for leaks. As a graduate student hoping to do my thesis experiment with the cyclotron, I was assigned a variety of tasks. Hugh Glassford, an engineer, looked after the more conventional engineering needs. Three junior members of the faculty, G. N. Glasoe, D. P. Mitchell, and Hugh Paxton worked on the cyclotron part time.

Once built, the cyclotron was a huge success. It played a crucial role in the development of nuclear energy and is now on permanent exhibit at the Smithsonian Institution in Washington, D.C.

FISSION OF URANIUM

When fission was discovered in 1938, Dunning was the right man at the right place at the right time. He had a strong source of neutrons from his cyclotron. He had constructed a linear amplifier-ionization combination that could be readily adapted to detect the large energy release expected from the fission of uranium. Moreover, he had a great deal of experience with neutrons, especially slow neutrons.

It is important to point out that the idea of looking for the energy release in fission was attributable to Otto Frisch and his aunt, Lise Meitner. Frisch was the first to realize that the fast-moving nuclei from the splitting of uranium would produce a huge amount of ionization compared with the

background from the alpha particles of uranium decay. Frisch also had a uranium-lined ionization chamber connected to a linear amplifier and he readily saw huge pulses of ionization on an oscilloscope when a neutron source—300 milligrams of radium mixed with beryllium—was brought up to the ionization chamber. It was a historic occasion. Niels Bohr was at the point of leaving for the United States when Frisch came to report these results. Because of his concern about priority, Frisch asked Bohr not to mention these results to the Americans until the paper he was preparing about them appeared in print.

We have Dunning's own recollection of what happened at that time in a speech he gave to the American Physical Society some years later:

> On the morning of Wednesday, January 25, 1939, Willis Lamb, returning from Princeton where Professor Bohr was lecturing, brought further news of Bohr's analysis of Otto Hahn's brilliant chemical identification of lower atomic weight elements like barium in the products resulting from neutron capture by uranium, thus clearly suggesting splitting the uranium-plus-neutron system, rather than the transuranic series postulated before. In discussions around the [Columbia] faculty club lunch table it was clear that large kinetic energy release should accompany such splitting. Fermi, Rabi and others left for the Fifth Annual Washington Theoretical Physics Conference. After returning to the Pupin cyclotron laboratory, it seemed clear we should try to detect the energy, which on elementary mass-defect reasoning ought to be in 100 or 200 MEV range.
>
> Unfortunately, the new cyclotron in the Pupin basement was behaving poorly, and chamber modifications were being made by Dr. E. T. Booth, Dr. F. G. Slack, and Herbert Anderson, but I hoped it could get working that afternoon. A flexible new linear amplifier-ionization chamber-oscillograph system was already installed next to the cyclotron—being used largely as a neutron detector with the cyclotron. After several attempts a small metal disk was finally coated with uranium oxide and installed in the ion chamber as one electrode. The alpha-particle pulses around 4.5 MEV were clearly visible.
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> That evening, while my colleagues still worked on the cyclotron, I fi-

nally brought from the thirteenth-floor laboratory a radon + beryllium fast neutron source—the type used for most of our previous work—and placed it next to the U-containing ion chamber. In great excitement, we saw about one big pulse on the oscilloscope every minute. The rate was so slow we had doubts at first whether it was real or maybe a poor electrical connection. But when I put the neutron source in a paraffin vessel, usually called a slow-neutron "howitzer," my notebooks indicate that the rate went up to seven or so huge pulses per minute. With a cadmium, slow-neutron-absorber screen interposed, the rate dropped to around one or two a minute. Clearly the main effect was due to slow neutrons. A rough calibration of the pulse height versus the 4.5 MEV alpha-particle pulse height indicated around 65 to 100 MEV peak energy. Since in fission, one of the two fragments goes backwards into the electrode plate, the total energy per splitting should be in the 130 to 200 MEV range. Fission energy was clearly a new order of magnitude!

We quit about eleven P.M. My diary that night says cryptically: "Believe we have observed new phenomena of far-reaching consequences," and relates what I have just described.

In addition to Dunning's recollections, the archives of The University of Chicago library preserves a notebook containing my own first observations, as Dunning's graduate student, of fission pulses.

Two days later, Dunning sent a telegram to Fermi in Washington announcing these results. The opening talks by Bohr and Fermi at the Fifth Washington Conference on Theoretical Physics on January 26, 1939, about the implications of the chemical evidence for the fission of uranium obtained by Hahn and Strassmann were sensational. The physical evidence obtained by Frisch a few weeks earlier using the ionization method demonstrated the energy release. Dunning's result confirmed it and was quickly repeated in three other American laboratories. The implications for nuclear power and possibly nuclear explosives were immediately recognized and given wide media coverage. Dunning had helped open the nuclear age.

These results of the Columbia group plus some additional

observations on the nature of the fission process were promptly reported in a classic paper in the March 1, 1939, issue of the *Physical Review,* "The Fission of Uranium," by H. L. Anderson, E. T. Booth, J. R. Dunning, E. Fermi, G. N. Glasoe, and F. G. Slack. Words alone cannot recapture the excitement of those times.

THE CHAIN REACTION

To make nuclear power and nuclear explosives practical, it was recognized that it would be necessary to induce large numbers of fissions using large quantities of uranium. This could be done if neutrons were emitted in the fission process. In this case, it would be necessary to arrange matters so that the new neutrons would cause additional fissions, with further additions from the neutrons from these in turn. With more neutrons produced than absorbed in each generation, there would be a rapid buildup in their number—a chain reaction.

In the late 1930s, there was, as yet, no evidence for the neutron emission. Moreover, the cross-section for fission by slow neutrons in natural uranium was rather small, raising the question of excessive loss of reproduction factor due to parasitic processes.

The question was how to proceed from there. The Columbia team split up. Fermi and Anderson decided to try to obtain a chain reaction using natural uranium and a suitable means for slowing down the neutrons. Dunning, Booth, and Slack—believing that the isotope responsible for the slow neutron fission was U^{235}—opted to enrich the uranium with this isotope by the gaseous diffusion method. This was the surest way to proceed provided the problem of isotope separation could be solved in a practical way. Dunning had no doubt it could be done.

LETTER TO NIER

He lost no time. If he could demonstrate experimentally what seemed plausible from the arguments of Bohr and Wheeler, then the proper course for nuclear energy was by enrichment of the light isotope U^{235}. On April 6, 1939, Dunning dispatched a letter to Alfred O. Nier, then a professor of physics at the University of Minnesota, to enlist his support in making this test. The letter shows how clearly Dunning understood what was involved. Because of its historic importance, I have reproduced the letter here in its entirety:

Dear Professor Nier:

There are a number of things which I hope to be able to discuss with you during the Physical Society meeting in Washington, April 27–29. I trust you will be there as usual as I understand you have a paper.

In order that you will be acquainted with the situation from my point of view so that you can consider the possibilities before we meet, perhaps the following should be outlined.

The demonstration that uranium splits or fissions, particularly with slow neutrons, with very large energy evolution opens many far-reaching possibilities. It is now quite certain that the recoiling fragments emit some secondary neutrons. The fragments have too little positive nuclear charge for their atomic weight, i.e., they have a neutron excess and are unstable. They therefore achieve stability by emitting betas or neutrons or both. This is almost obvious. As a matter of fact Dr. Booth and I started looking for secondary neutrons almost immediately after demonstrating that U fissions the last part of January, although the first experiments were not very conclusive. Later experiments by a number of people here and abroad all indicate the existence of secondary neutrons. There are likely to be somewhere between 1 and 5 secondary neutrons per fission. Fermi is going into that phase of the problem particularly.

If there is on the average at least more than one secondary neutron for each "primary" neutron, so that the net effect of the absorption of neutrons through non-fission processes is more than counterbalanced, then we have the possibility of setting up a self-perpetuating, cascade type of reaction analogous to ionization by impact build-up. The development

of enormous energy through the release of nuclear energy on a large scale is coming closer to realization than most people realize.

From simple physical reasoning, it seems clear, crudely speaking, that the following factors must be considered: On the one hand we have (A). Neutron fission processes: Concentration of fissioning U, together with the effective fission cross-section of the U; on the other hand (B). The summation of the non-fission capture processes: i.e., summation of the concentrations of the various capturing elements or isotopes in the system (including the U), each with its appropriate cross-section. In addition we have (C). The effective number of neutrons liberated per fission; and finally (D). The effective probability of a neutron to stay in the system, i.e., not to escape. (This is always less than 1).

Of course, this must be summed or integrated and the variables considered as functions of the neutron energies. So far as I know, no one has dealt with this problem on any thorough basis, and it is obvious that the exact calculations are going to be quite involved. However the essential physics is fairly simple and it seems that if (A/B)CD is effectively greater than unity, then a chain reaction will occur. (Ed note: The quantity (A/B) should be the fraction of neutron captures that lead to fission; thus, B should include the neutron capture processes that lead to fission.)

There are some very serious problems yet remaining however. The actual cross-section for fission with slow neutrons of uranium is not very large—only about 2 to 5 $\times$ 10^{-24} cm^2 at most, so the numerator A above is not large. Unfortunately, there is also a strong resonance capture of neutrons by U which almost certainly does not give fissions, and this gives a fairly high cross-section all through the slow neutron region as well as the sharp peak at resonance (or resonances). This competing process thus contributes to (B) above. In addition, there are other contributions to (B), inevitably, such as capturing elements in the material of construction or in slowing down media such as H_2-containing materials, or in various impurities such as boron or cadmium which will be especially bad. From what we know of the various cross-sections involved now, I believe there is virtually no safety margin left for a successful chain reaction system with ordinary uranium, certainly not unless extreme purity and special slowing down materials are used, possibly deuterium—ordinary water seems out (H absorption). Very large amounts of material will be required or else the neutron escape factor (D) will be serious. It is clear that making a chain reaction "go" is not going to be easy.

There is one line of attack that deserves strong effort, and that is where

we need your cooperation. The important question is which uranium isotope is really responsible for slow neutron fission? It is a matter of opinion largely, and some theoretical physicists think one way, some think the other. Bohr thinks 235, but Fermi is neutral or inclined toward 238. Bethe and Placzek are on opposite sides of the fence, in fact there is a bet on. It is of the utmost importance to get some uranium isotopes separated in enough quantities for a real test of the whole question.

If U^{235} can be shown to be the one responsible for the slow neutron fission, then it is very certain that the chain reaction can be produced, particularly if the U^{235} is concentrated some. Assuming your figures on the relative proportion in ordinary ores of about 1/140, this would raise the effective slow neutron cross-section from about 2 to 5×10^{-24} cm^2 for ordinary U, to about 275 to 700×10^{-24} cm^2 for pure U^{235} in the (A) term of the discussion above. This would be certain to work even with a very small secondary neutron excess over 1. It would also permit the presence of very much larger amounts of other capturing materials. Furthermore the sizes and amounts of materials required would be much reduced. Thus while the chain reaction may be made to go eventually with ordinary U, clearly if U^{235} is the one, we open a whole new realm of possibilities with a really concentrated energy source. Reasonably pure U^{235} probably will be explosive under some conditions, which may make a great military weapon of enormous power.

We are pushing up the cyclotron neutron output steadily. If you could effectively separate even tiny amounts of the two main isotopes, there is a good chance we could use very tiny samples to demonstrate which isotope is responsible, and study the whole phenomena. There is no other way to settle this business except to work with separated isotopes. Dr. Booth and I have the cyclotron and all the other necessary equipment and techniques. If we could all cooperate, and you aid by separating some samples, then we could by combining forces settle the whole matter.

There is a great opportunity here, as I'm sure you realize. I hope you will give serious consideration to what you could do to rebuild your spectrometer system for this purpose, and let us get together and discuss it all in Washington. It will not be necessary to make a complete separation. A compromise in between for quantity production is more important than resolution.

Sincerely yours,

John R. Dunning

Please excuse the typing—I did it myself.

P.S.

It cannot be overestimated how important this really is. I had already made a number of layouts of atomic energy systems, almost immediately last January. A considerable number of variations are possible depending on the choice of slowing down and neutron "reflector" materials, heat transfer materials (radical departures from standard heat engines are also envisioned—direct conversion). The secondary neutron emission, effective capture and the U^{235} concentration are vital, assuming we can demonstrate it in the face of all the theoretical arguments. (A sketch is given, not reproduced here.) This is only schematic but it shows that these ideas are practical, far more than physicists generally realize yet.

JRD

FISSION OF U^{235}

Some years later, as Dunning's diary recalls it:

Professor Nier eagerly accepted the challenge—building bigger special mass spectrometers, trying UF_6 as Dr. A. V. Grosse had arranged, then UBr_4—and finally, after many difficulties, on March 2, 1940, succeeded in sending us two tiny electrode sections labelled "U^{235}" and "U^{238}" with well under a microgram of U^{235}—quite invisible.

My notebook entry on March 2, 1940, says cryptically: "U^{235} + U^{238} samples from Nier received. Made from UBr_4. Demonstrated conclusively slow neutron fission due to U^{235}. Atomic energy released now definitely assured at last!! Some concentration may be desirable, but the new era can now be seen!

Large scale separation methods are clearly needed now conclusively; considering 1) electrical, 2) centrifugal, 3) thermal diffusion, 4) gas and liquid diffusion."

No time was lost in getting the means for separating the isotopes under way. The following excerpts from a speech by Eugene T. Booth as part of the memorial service for Dunning in 1975 tells the story and shows how Dunning's unique personality made it all possible:

I remember as yesterday when John and I were returning from a trip to Schenectady—I believe it was in 1940. We had stopped for dinner, late in the evening, and reviewed again the various methods of separating isotopes. These were ruled out, one by one, as not suitable for use with uranium on a large scale, all except the gaseous diffusion methods. It was realized that new features would have to be devised, but fundamentally this approach appeared feasible.

From that day on, separation of the isotopes of uranium by gaseous diffusion became an obsession with John, in the creative sense of the word. Nothing would daunt him. After many turbulent periods of uncertainty, the diffusion plants at Oak Ridge were constructed and are still operating today. Further expansion of capacity is being planned even now.

Booth goes on to quote a letter dated May 3, 1950, from General L. R. Groves, a man who dealt with Dunning during the war and was in a good position to evaluate his contribution to the Manhattan Project:

. . . I did have personal contact with Professor Dunning during the Manhattan Project period, as well as since then. I am glad that you saw my letter to him of about four years ago, as I am sure that it expressed my views about his value to the Project—that is, insofar as they could be made public. As a matter of fact, Dr. Dunning was of even more value. There was, as he may have told you, a great deal of adverse opinion among many scientists, and even among the group at Columbia as to the possibility of our being able to make the gas diffusion process an operable affair.

Despite the prophets of doom among the scientific leaders, with respect to this phase of our work, Dr. Dunning never varied in his optimistic attitude. He was a great bulwark to me, as we were proceeding against the very positive advice of many distinguished scientists.

. . . My main impression of Dr. Dunning during the War was that he was a man who was so full of his subject that he could not stop talking about it. It was always difficult to break off conversations with him. It was difficult at times for me to get in a question edgewise, and particularly, to get the answer from the man in actual charge of the particular experiment, as Dr. Dunning always seemed to want to do all the talking. He was just so enthusiastic, he seemed to be bubbling over.

. . . I feel very strongly that Dr. Dunning has not been appreciated by his country for his work on the Project, and primarily, he has not received

the credit due him for his scientific anticipation or intuition and for his courage in standing up against the opinions of his fellow distinguished scientists.

Few people have had such a prominent role in establishing a new and important industry on a world-wide scale. The nuclear power industry today assumes even greater importance in the public mind with the realization that fossil fuels will require supplementation in the years ahead.

As is well known, the first chain reaction was made with a graphite pile using ordinary uranium. Although it was not anticipated in the beginning, it turned out that a by-product of the reaction was Pu^{239}, a new isotope with slow neutron fission characteristics like those of U^{235}. The reactors built at Hanford, Washington, using ordinary uranium, produced Pu^{239} in sufficient quantity to make the first nuclear explosion at Alamogordo, New Mexico. The electromagnetic method produced enough U^{235} for the Hiroshima bomb. The Nagasaki bomb used Pu^{239}.

Because of the difficulties encountered in the development of a practical diffusion membrane, the gaseous diffusion method did not come into its own in time to help end the war. Instead, the first chain reaction was made with ordinary uranium using a graphite pile—Fermi's method.

Dunning recalled those difficult times in a talk to the American Physical Society he gave some years later:

> Unfortunately, we could not convince the Uranium Committee that our U^{235} gas diffusion process should be supported by the government, so we had to carry on the development ourselves. Not until August 1941 did our success gain official support. Then the engineering of the first diffusion separation plant at Oak Ridge gradually got under way in 1942, to ultimate success.

Officially, Dunning became director of research, Division I, SAM Laboratories. "SAM" stood for "Substitute Alloy Materials," code name for Columbia's nuclear laboratory. The original development work for the gaseous diffusion process

was carried out in this laboratory, but the large-scale engineering research and development was done by the M. W. Kellogg Company under the direction of Percival ("Dobie") C. Keith.

For the construction of the huge plants at Oak Ridge, a new company, the Kellex Company, was established. It was completely owned by Kellogg, and staffed with virtually the same officers. The Oak Ridge gaseous diffusion plant, K-25, was built and began operating in 1945. Subsequently, the Oak Ridge complex expanded through several major plant additions. During the Korean War, two additional gas diffusion plants were built at Paducah, Kentucky, and Portsmouth, Ohio. The Union Carbide Company was selected to operate the first two, and the Goodyear Group the third. Dunning maintained close contact with all these entities until the whole enterprise was successfully launched. At the peak of their operations, these plants consumed about 15 percent of the total electrical power produced in the United States.

Dunning could, quite rightfully, take pride in the fact that, increasingly, nuclear power plants were being built using enriched U^{235} for their successful economic design and operation. In 1971, the pioneering work of Dunning and his three colleagues on the gaseous diffusion method for U^{235} separation was recognized by an award of $30,000 each, in lieu of patent royalties, by the Atomic Energy Commission. The work had been recognized as patentable by the U.S. Patent Office, but a patent could not be issued because of the secrecy restrictions.

NEVIS CYCLOTRON

After the end of World War II, Dunning served as scientific director for construction of Columbia's Nevis Laboratories, a cooperative endeavor of Columbia University, the Atomic Energy Commission, and the Office of Naval Re-

search. The principal activity was the construction and operation of the 385 MEV synchrocyclotron. The detailed design and construction as well as much of the initial operation was carried out by Dunning's close collaborator, Eugene T. Booth.

DEAN OF ENGINEERING

In 1946, Dunning was appointed Thayer Lindsley Professor of Applied Science, and in 1950, dean of the School of Engineering and Applied Science—appointments which marked the end of his active participation in research.

After his appointment as dean, Dunning threw himself into a fund-raising campaign that resulted in the construction of the Seeley Wintersmith Mudd and Terrace Engineering Center at Columbia. When he resigned his deanship in 1969, he had raised more than $50 million for the school.

He held numerous posts in the world of American science, including: member of the National Academy of Sciences, elected 1948; member of the board, American Association for the Advancement of Science; trustee, Fund for Peaceful Atomic Development; chairman, New York City Board of Education Advisory Committee on Science Manpower; member, Scientific Advisory Committee, Department of Defense; chairman, Science Advisory Council to the Legislature of the State of New York; chairman, President's Committee on Super-Sonic Transport.

In the 1950s, President Dwight D. Eisenhower and Admiral Hyman G. Rickover consulted him frequently on military matters and on the development of nuclear-powered submarines.

He was a member of the board of directors of a number of corporations and chairman of several. He received nine honorary degrees and eight awards.

MEDAL OF MERIT

President Harry S. Truman signed the citation accompanying the 1946 Medal of Merit. It reads as follows:

DR. JOHN RAY DUNNING for exceptionally meritorious conduct in the performance of outstanding service to the War Department, in accomplishments involving great responsibility and scientific distinction in connection with the development of the greatest military weapon of all time, the atomic bomb. As a physical researcher, he took a leading part in the initiation of the early phases of the project; then he was in charge of essential research in the SAM Laboratories for the Manhattan Engineer District, Army Service Forces, and then he served as advisor to the contractor for full scale operation of his process. A physicist of national distinction, Dr. Dunning's unselfish and unswerving devotion to duty have contributed significantly to the success of the Atomic Bomb project.

PUBLIC SERVICE

A strong believer in informing the public more fully about the nature and implications of atomic energy, Dunning spoke often across the nation before teachers' associations, business conferences, civic clubs, town meetings, as well as on radio and TV programs. These talks ranged over a broad spectrum of subjects: "Education for the Atomic Age," "On the Edge of Disaster—Technological Challenge to America," "On Science Teaching," "The What and How of Nuclear Power," "Sputniks Are Not Enough," "Breakthroughs in Science," "The Next 100 Years," and "Impact—Government Support and Engineering Education."

He took a special interest in explaining abstruse subjects such as nuclear fission to nontechnical audiences, with the aid of contemporary "props" whenever possible. For example, to help explain the principles of nuclear fission to youngsters of school age, he assisted in the production of a "Blondie and Dagwood" comic book that reduced the story of atomic energy to its simplest terms.

Similarly, he enlivened the Columbia Engineering dean's platform talks with a variety of mechanical and electronic gadgets he used to illustrate or dramatize his remarks. These included a radioactive "atomic ray gun"—inspired by Buck Rodgers's famous "disintegrator pistol"—Geiger counters, oscillographs, and various combinations of bells and colored lights that culminated in an "atomic pinball machine"—a miniature atomic power system that demonstrated actual atomic fission energy release.

For Dunning, the phenomenon of radioactivity never lost its fascination. I remember vividly the way he demonstrated the circulation of the blood using radioactivity. He prepared a sample of Na^{24} (15-hour half-life) by irradiating a glass of salt water with the cyclotron. Using a Geiger counter, he first showed that the radioactivity was in the glass. He then stretched out his hand with the Geiger counter at his finger tips: no activity. He then drank the glass of irradiated water. After some anxious minutes, the Geiger counter at the finger tips began to respond—at first weakly—then increasingly, as the circulating blood brought more and more of the radioactive salt to the finger tips. It was a great show. The audience loved it, and so did Dunning.

I WISH TO THANK Professor Dunning's son, John Ray Dunning, Jr., for sending me the Nier letter and the Booth commentary extensively quoted here.

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